




Physical &
Applied Sci.
Serials



Digitized by the Internet Archive
in 2009 with funding from
University of Toronto

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S. L. & E. &c.

RICHARD TAYLOR, F.L.S. G.S. Astr.S. Nat.H. Mosc. &c.

AND

RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.

“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XVI.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,
ANNALS OF PHILOSOPHY, AND JOURNAL OF SCIENCE.

JANUARY—JUNE, 1840.

LONDON:

RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;

SOLD BY LONGMAN, ORME, BROWN, GREEN, AND LONGMANS; CADELL;
SIMPKIN AND MARSHALL; S. HIGHLEY; WHITTAKER AND CO.; AND
SHERWOOD, GILBERT, AND PIPER, LONDON: — BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH
AND SON, GLASGOW; HODGES AND SMITH, DUBLIN:
AND G. W. M. REYNOLDS, PARIS.



THE Conductors of the London and Edinburgh Philosophical Magazine have to acknowledge the editorial assistance rendered them by their friend Mr. EDWARD W. BRAYLEY, F.L.S., F.G.S., Assoc. Inst. C. E., Corresponding Member of the Royal Geological Society of Cornwall and of the Philosophical Society of Basle, Hon. Mem. S. Afric. Inst.; LIBRARIAN TO THE LONDON INSTITUTION, and SECRETARY TO THE ELECTRICAL SOCIETY.

17969

QC

1

P4

ser. 3

v. 16

CONTENTS OF VOL. XVI.

NUMBER C.—JANUARY, 1840.

	Page
On the Theory of Substitutions in Chemistry, proposed by M. Dumas. In a Letter to M. Pelouze from M. Berzelius	1
Mr. Jeffreys's Experiments on Mechanical Exosmose with reference to determining the Existence of any difference of Magnitude between liquid and gaseous Particles	10
Mr. R. Potter on Photometry in connexion with Physical Optics.	16
Mr. Gulliver's Observations on the Blood Corpuscles or Red Disks of the Mammiferous Animals. No. I.	23
Mr. J. T. Cooper on the Employment of Carbon in Voltaic Combinations.	35
Prof. Sylvester on Derivation of Coexistence: Part I. Being the Theory of simultaneous simple homogeneous Equations	37
Dr. Schafhaeutl on the Combinations of Carbon with Silicon and Iron and other Metals, forming the different species of Cast Iron, Steel, and Malleable Iron	44
Mr. Halliwell's Observations on the Authenticity of the Passage in the Treatise of Boetius <i>de Geometria</i> on Numerical Contractions	51
Mr. Warington on the coloured Films produced by Electrochemical Agency and by Heat.	52
Rev. D. Williams on the Geology of Devon and Cornwall, with reference to a Paper read before the Geological Society on December 4, 1839.	59
New Books:—Miller's Treatise on Crystallography,—Transactions of the Cambridge Philosophical Society,—Carpenter's Principles of General and Comparative Physiology.	65
Proceedings of the Royal Astronomical Society.	72
————— Linnæan Society	76
New System of Postage	78
Letter Barometer	79
New Scientific Books	79
Meteorological Observations for November 1839	79
Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Robertson; by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall at Boston; and by Mr. Dunbar at Applegarth Manse, Dumfries-shire	80

NUMBER CI.—FEBRUARY.

Dr. Draper's Account of some Experiments made in the South of Virginia, on the Light of the Sun.	81
----------------------------------------------------------------------------------------------------------	----

	Page
Mr. P. J. Martin on a remarkable Fall of Hail; with Observations on the probable Nature of such Phænomena	85
Mr. J. W. Gutch's Notice of certain Meteorological Phænomena observed at Swansea	87
Mr. Ivory (The Bakerian Lecture) on the Theory of the Astronomical Refractions (<i>concluded</i>)	89
Prof. Forbes's Letter to Richard Taylor, Esq., with reference to two Papers in the Philosophical Magazine for January, 1840	102
Mr. Gulliver's Observations on the Blood Corpuscles, or Red Particles of the Mammiferous Animals. No. II.	105
Mr. T. Hawkins on Galvanic Series formed of Zinc and Inactive Iron	115
Mr. W. S. Harris on Lightning Conductors, and the Effects of Lightning on Her Majesty's Ship Rodney, and certain other Ships of the British Navy, being a further examination of Mr. Sturgeon's Memoir on Marine Lightning Conductors. .	116
Dr. Kane on a new Compound of Ferrocyanide of Potassium with Cyanide of Mercury	128
Th. Scheerer on the Decomposition of the Neutral Sulphate of the Peroxide of Iron by boiling its Solution	130
Prof. Sylvester's Method of determining by mere Inspection the derivatives from two Equations of any degree.	132
Mr. Halliwell's New Researches on the true nature of the Boetian Contractions, especially with reference to the Explanation given by M. Chasles.	136
Mr. R. Hunt on the Permeability of various bodies to the Chemical Rays.	138
Mr. M. Roberts on an anomalous Electric Condition of Iron. .	142
New Books:—Curtis's British Entomology.	144
Proceedings of the Geological Society.	144
————— Royal Astronomical Society	148
Theory of Substitutions—Acetic and Chloroacetic Acids	152
Myronin, Myronic Acid—Essential Oil of Mustard.	153
Polychromatic Acid	154
Cyanil	154
Action of Albumen on Metallic Salts	154
Haydenite	155
Beaumontite, a new Mineral.	156
An Account of the Experiment proposed by M. Arago as a Test of the accuracy of the undulatory Hypothesis of Light	157
Meteorological Observations for December 1839	159
————— Table.	160

NUMBER CII.—MARCH.

Mr. R. Griffith on the true Order of Succession of the Older Stratified Rocks in the Neighbourhood of Killarney and to the North of Dublin	161
------------------------------------------------------------------------------------------------------------------------------------------------------	-----

	Page
Mr. Brooke on Haydenite and Couzeranite	175
Rev. J. H. Pratt's Observations on the relative Temperature of the Sea and Air, and on other Phænomena, made during a Voyage from England to India.	176
Mr. J. Tovey's Researches in the Undulatory Theory of Light, continued: On the Absorption of Light.	181
Mr. G. J. Knox on the Direction and Mode of Propagation of the Electric Force traversing Interposed Media	185
————— Researches on Fluorine	192
On a simple mode of obtaining from a common Argand Oil Lamp a greatly increased quantity of Light; in a letter from Sir J. Herschel, Bart.	194
Mr. Gulliver's Observations on the Blood Corpuscles, or Red Particles of the Mammiferous Animals. No. III.	195
Prof. J. Henry's Contributions to Electricity and Magnetism. No. III. On Electro-dynamic Induction	200
Researches of Mons. R. Piria on the Combinations of Salicyle	210
Mr. R. Potter's Letter to Richard Taylor, Esq., as Editor of the Philosophical Magazine and Journal	220
Mr. Halliwell's Additional Note on the Authenticity of the disputed Passage in the treatise of Boetius <i>de Geometria</i> on Numerical Contractions	221
Dr. R. Kane on a Pseudomorphous variety of Iodide of Potas- sium	222
Proceedings of the Royal Irish Academy.	224
Precipitation of Iron by Zinc	235
Action of Chlorine on the Carburetted Hydrogen of Acetates. .	235
Hydrocarburet of Bromine	236
Native Sulphate of Magnesia	236
Manufacture of Chlorate of Potash	237
Diabetic Blood and Urine.	238
Sir John F. W. Herschel's new Researches on the Solar Spec- trum and in Photography	239
Meteorological Observations for January, 1840	239
————— Table.	240

NUMBER CIII.—APRIL.

Letter from M. Kreil, Director of the Observatory at Milan, to M. Kupffer, Director General of the Physical Observatories in Russia, containing a succinct Account of the principal Results of M. Kreil's Magnetic Observations at Milan	241
Mr. J. D. Smith's Observations on the supposed Formation of Inorganic Elements during Fermentation.	251
Prof. J. Henry's Contributions to Electricity and Magnetism. No. III. On Electro-dynamic Induction (<i>continued</i>)	254
Th. Scheerer on the Natural Products which originate from the action of the Atmosphere on Iron Pyrites	265

	Page
Mr. R. Hunt's Experiments and Observations on Light which has permeated coloured Media, and on the Chemical Action of the Solar Spectrum	267
Mr. Weaver on the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c.....	276
Dr. C. Schafhaeutl on the Combinations of Carbon with Silicon and Iron and other Metals, forming the different species of Cast Iron, Steel, and malleable Iron (<i>continued</i>)	297
Dr. Roget's Description of a Method of Moving the Knight over every Square of the Chess-board, without going twice over any one; commencing at any given square, and ending at any other given square of a different colour	305
Mr. E. Solly's Observations on the Precipitation of Copper by Voltaic Electricity	309
Mr. A. Smee on the Galvanic Properties of the Metallic Elementary Bodies, with a Description of a new Chémico-Mechanical Battery	315
M. Dumas's Memoir on the Law of Substitutions, and the Theory of Chemical Types	322
Proceedings of the Royal Society	329
———— of the Royal Institution	338
Theory of Substitutions. Pond Gas	339
On Arsenic contained naturally in the Human Body	341
Meteorological Observations for February, 1840.....	343
———— Table.....	344

NUMBER CIV.—MAY.

Mr. Lyell on the Boulder Formation, or Drift and associated Freshwater Deposits composing the Mud-Cliffs of Eastern Norfolk	345
Mr. R. Potter on the Method of performing the simple Experiment of Interferences with two Mirrors slightly inclined, so as to afford an <i>experimentum crucis</i> as to the nature of Light	380
Mr. Weaver on the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c. (<i>continued</i>)	388
Mr. W.S. Harris on the Course of the Electrical Discharge, and on the Effects of Lightning on certain Ships of the British Navy (<i>continued</i>)	404
Dr. R. Kane's Remarks on the Compounds derived from the Stearopten of Oil of Peppermint	418
Mr. A. Smee on the Galvanic Properties of the Elementary Bodies, and on the Amalgamation of Zinc	422
Dr. C. Schafhaeutl on the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different species of Cast Iron, Steel, and Malleable Iron (<i>continued</i>)	426

Mr. Lubbock on the Heat of Vapours, and on Astronomical Refractions.....	434
M. Dumas's Memoir on the Law of Substitutions, and the Theory of Chemical Types (<i>continued</i>).....	442
Proceedings at the Royal Institution	447
Meteorological Observations for March, 1840	447
————— Table.....	448

NUMBER CV.—JUNE.

Mr. Brooke on crystallized Native Oxalate of Lime	449
Dr. Draper on the Electro-motive Power of Heat	451
Mr. Halliwell's Illustrations of the History of the Inductive Sciences. No. I. The Reception of the Copernican Theory in England.....	461
Mr. Coathupe on certain Modifications of the Powers of Heat and Light when transmitted through Glass	467
Mr. Weaver on the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c. (<i>concluded</i>).....	471
Prof. Miller on the Form of <i>Eudialyte</i>	477
Mr. Grove on some Phænomena of the Voltaic disruptive Discharge	478
Th. Scheerer on two Norwegian Cobalt Ores from the Skutteruder Mine	482
Prof. Poggendorff on Galvanic Circuits composed of two Fluids, and of two Metals not in contact.....	485
Mr. G. Walker on the moving the Knight over every Square of the Chess-board alternately	498
M. Dumas's Memoir on the Law of Substitutions, and the Theory of Mechanical Types (<i>continued</i>)	501
Prof. J. D. Forbes on an apparent Inversion of Perspective in viewing Objects with a Telescope.....	506
Mr. Lubbock on the Heat of Vapours, and on Astronomical Refractions (<i>continued</i>).....	510
Dr. C. Schafhaeutl on the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different species of Cast Iron, Steel, and Malleable Iron (<i>continued</i>).....	514
New Books:—Davies's Solutions of the principal Questions of Dr. Hutton's Course of Mathematics	524
Proceedings of the Royal Society.....	526
On the Production of Electrotypes, by A. Smee, Esq., Surgeon	530
On the Reduction of Chromate of Lead, by R. F. Marchand. .	532
Portraits in Daguerreotype	535
Meteorological Observations for April, 1840	535
————— 'Table.....	536

NUMBER CVI.—SUPPLEMENT TO VOL. XVI.

	Page
Prof. Poggendorff on Galvanic Circuits composed of two Fluids, and of two Metals not in contact (<i>concluded</i>)	537
Prof. J. Henry's Contributions to Electricity and Magnetism. No. III. On Electro-dynamic Induction (<i>concluded</i>)	551
Mr. Lubbock on the Heat of Vapours, and on Astronomical Refractions (<i>continued</i>)	562
Dr. C. Schafhaeuti on the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different species of Cast Iron, Steel, and Malleable Iron (<i>concluded</i>)	570
Proceedings of the Royal Society	590
Index	601

 PLATES.

- I. Illustrative of Mr. W. SNOW HARRIS's Second Paper on Lightning Conductors.
- II. and III. Illustrative of Mr. GRIFFITH's Paper on the Geology of Killybegny.
- IV. Illustrative of Dr. ROGET's Paper on the Problem of the Knight's Move at Chess.
- V. Illustrative of Mr. LUBBOCK's Memoir on the Heat of Vapours, and on Astronomical Refractions.
- VI. Illustrative of Dr. DRAPER's Paper on the Electro-motive Power of Heat.
- VII. Illustrative of Mr. SMEE's Paper on the Production of Electrotypes.

 ERRATA.

- Vol. xii. p. 608, line 2 from the bottom, *for* Manhand, *read* Marchand.
- Vol. xv. p. 451, last line but three, *for* increasing indefinitely, *read* increasing or diminishing indefinitely.
- p. 452, line 14, *for* $\cos mi = \sqrt{-1} \cdot \sin mi$, *read* $\cos mi + \sqrt{-1} \cdot \sin mi$.
- p. 453, line 28, *for* ρ, a , *read* ρ, a_1 .
- , line 29, *for* a , *read* a_1 .
- p. 454, line 21, *for* (23.), *read* (33.)
- . lines 22 and 23, *for* $e^e x$, *read* $e^e x$.
- Vol. xvi., p. 32, line 15 from the bottom, *for* 1-2554th, *read* 1-3554th.
- p. 108 line 19 from the bottom, *for* Haller, *read* Harvey.
- p. 478, line 2, *for* M.R.S. *read* M.R.I.

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JANUARY 1840.

I. *On the Theory of Substitutions in Chemistry, proposed by M. Dumas. In a Letter to M. Pelouze from M. Berzelius*.*

YOU will perhaps permit me to return once more to M. Dumas's theory of substitutions, especially after the new development that this skilful chemist has lately given it, a development which, according to him, threatens to overturn the theory of chemistry in general, and especially electro-chemical notions. You remember that in a preceding letter, I declared my opinion, that the application which has been made of the theory of substitutions, in considering chlorine, which takes the place of hydrogen, as acting the same part as this latter, is contrary to the principles of the science, and I quoted some examples on this subject, which I think prove it in an unequivocal manner. In begging you to communicate the contents of this letter to the *Académie des Sciences*, I hoped that M. Dumas would have the goodness to explain under what point of view he regards the theory in question. He has, indeed, complied with this request, and in a manner which, with the exception of some details foreign to the question, has entirely satisfied me.

In the *Comptes Rendus* of 1838, (1^{ère} semestre, p. 699 and the following pages,) M. Dumas has disclaimed this explanation of the theory of substitutions: "If I am made to say that chlorine takes the place of hydrogen in such a way that the former acts the same part as hydrogen, an opinion is imputed to me," says M. Dumas, "against which I protest, for it is opposed to all that I have asserted on these matters. The theory of substitutions merely asserts the simple affinity between hydrogen and the chlorine which takes its place in equal volume. It is an empirical law which deserves our attention only as long as it holds good, and if any one has made it of more importance it is not my fault."

* From the *Ann. de Chim. et de Phys.*, vol. lxi. p. 137.
Phil. Mag. S. 3. Vol. 16. No. 100. Jan. 1840. B

This assertion of M. Dumas is in perfect harmony with my way of thinking; what has led to a misstatement of his opinion is probably the improper denomination of *theory of substitutions*, for an empirical law is not a theory, and the word 'substitution' has long since been used in chemistry to signify the replacing [*remplacement*]* of one body by another which acts the same part as this body, and M. Dumas has lately changed this word for another, which is very well chosen, *metalepsie*, signifying replacement [*remplacement*]*.

In the *Comptes Rendus* of 1839, (1^{ère} semestre, p. 909), M. Dumas has more lately given a new explanation of this theory, but quite opposed to that just mentioned. He made the beautiful discovery that crystallizable acetic acid $C^4 H^6 O^3 \bar{H}$, exposed to the light of the sun in an atmosphere of dry chlorine, is gradually decomposed, and that an equal volume of chlorine completely takes the place of the hydrogen. A new acid composed of $C^4 H^6 O^3, \bar{H}$, is the result of this action, some of whose properties he has described.

The conclusions which M. Dumas draws from this fact are the following: the chlorine in taking the place of the hydrogen atom for atom, *acts the same part as the hydrogen*; an acetic acid is the result of this, which differs only from the other in containing six atoms of chlorine instead of six atoms of hydrogen, and on this account he gives it the name of *chloracetic acid*. It possesses the same properties as the ordinary acid, so that if we know the properties of the latter, we equally know those of chloracetic acid. This is caused by the properties of a body depending rather on the type of the composition than on the particular characters of the element which has been exchanged. In organic chemistry, says he, there exist certain types, which are preserved even when equal volumes of chlorine, bromine, or iodine have taken the place of the hydrogen which they contain. He infers from this that there are two great leading principles in chemistry, which are *isomorphism* in mineral chemistry, and *substitution* in organic chemistry. He supposes that these principles originate from the same cause, and that in due time they may be generalised under one common expression.

M. Dumas adds that neither isomorphism nor the theory

* As precision in the use of terms is very important in this subject, is not *metalepsy* objectionable as signifying either *participation* or *succession*?

With regard to the practice of rendering the French *remplacer*, *remplacement*, by the English 'replace', 'replacement', which answer to the French *remplacer*, &c., and have an entirely different meaning, it is a slovenly shift, which ought always to have been resisted, as causing constant ambiguity and confusion. Can chemists find any authority for such a perversion of both languages?—R. T.

of substitution harmonizes with electro-chemical opinions, and as these latter have not, according to him, the property of yielding to facts or of explaining or predicting them, he dispatches the electro-chemical theory by saying, "It must be admitted there is nothing in it."

This is certainly a complete revolution in the state of chemistry.

When questions of such importance to science are treated of, the love of truth should ensure a mature examination of the propositions of the author of such a revolution, for the purpose of engaging us on his side if he is right, and of opposing him if he is wrong.

One of the great advantages of the theory of substitutions over electro-chemical opinions seems to be, that the type of composition preserves the same properties after the exchange of hydrogen for chlorine.

Let us then examine the few properties of chloracetic acid which M. Dumas has described, and compare them with those of acetic acid. It will be seen that these two acids differ infinitely more from each other than do formic acid and acetic acid.

ACETIC ACID.

Smell strong and pungent.
Liquid at common temperatures.
Solidifies at about 50° Fahr.,
boils at about 212°.
Specific gravity 1.06.
Potash salt deliquescent.
Æther very fluid, odour æthereal ; floats on water.

CHLOROACETIC ACID.

Scarcely any smell.
Solid at common temperatures.
Solidifies at about 187° Fahr.
Boils at about 392°.
Specific gravity 1.617.
Potash salt unalterable in the air.
Æther oleaginous, odour like mint ; sinks in water.

These two acids, therefore, have no other resemblance than that which belongs to acids in general, and it might be stated that chloracetic acid resembles sulphuric acid as much as it does acetic acid. Thus the type does not lead to an anticipation of the particular properties of an acid in which hydrogen is replaced by* chlorine.

With respect to the composition of chloracetic acid, M. Dumas is of opinion that it is formed of three atoms of oxygen and of one atom of a radical composed of four atoms of carbon and of six atoms of chlorine. In examining into this composition, it is natural to inquire if other bodies which have an analogous composition are not known, and whether M. Dumas's opinion is not equally applicable to them, for it is evident that any explanation not applicable to similar cases ought to be rejected.

* "*remplacé par.*" See preceding notes.—R. T.

4 M. Berzelius on the *Theory of Substitutions* of M. Dumas.

Chloracetic acid is composed of carbon combined with chlorine and oxygen. There exists a numerous class of similar combinations; it is divided into three subdivisions according to the radicals combined with the chlorine and with the oxygen; these shall be considered in rotation.

1. *Simple electro-positive radicals.*—This subdivision is very numerous and well known: it consists of the basic muriates of the ancient theory of the nature of hydrochloric acid. The general opinion, which has not been contested, is that these bodies are composed of a chloride and an oxide. They are readily decomposed and recomposed.

In applying the new theory of M. Dumas to the compound $\text{Pb Cl} + 3 \text{ Pb}$, it should be expressed by $\text{Pb}^4 \text{ Cl}^2 + 3 \text{ O}$, in which $\text{Pb}^4 \text{ Cl}^2$ is a radical composed of lead and chlorine.

I am persuaded that M. Dumas would protest against this extension of his views; but if they are true, however, it is requisite to do so, and to be consistent, we must go still further, and consider the salts of the oxacids as oxides of a radical composed of that of the acid and also of the base, sulphate of potash, for example, as $\text{KS} + 4 \text{ O}$.

2. *Simple electro-negative radicals.*—The number of combinations known of this class is very limited. They are to be considered under two different points of view. M. Henry Rose, who discovered the combination of chromium belonging to this class, considers it as composed of $\text{Cr Cl}^3 + 2 \ddot{\text{Cr}}$, and this manner of viewing it has been generally adopted. It has since been proposed to consider it as chromic acid in which an equivalent of chlorine is substituted for an equivalent of oxygen. This manner of viewing it is more simple, but it is easy to prove that it is not correct, for the chloride of chromium may be replaced by other chlorides*, and we have several examples of salts formed of an atom of a chloride and of two atoms of chromic acid; but the preceding hypothesis admits only a single relative number of atoms of chlorine and of oxygen, although several are known to exist. Consequently, the only admissible explanation in this class of bodies, is that of considering them as composed of a chloride and of an oxide.

The following are the only combinations of this species hitherto discovered:—

Radicals.	Formula.	Authors.
Sulphur	$\text{S Cl}^3 + \ddot{\text{S}}$	Regnault.
Do.	$\text{S Cl}^3 + 5 \ddot{\text{S}}$	

* "*se laisse remplacer par d'autres chlorures.*"

Radicals.	Formula.	Authors.
Carbon	$\underline{\text{C}} \underline{\text{Cl}^3} + \underline{\text{C}}^*$	Dumas (chloracetic acid.)
Do.	$\underline{\text{C}} \underline{\text{Cl}^2} + \underline{\text{Cl}}$	J. Davy (phosgene gas.)
Molybdenum ...	$\text{Mo} \underline{\text{Cl}^3} + 2 \underline{\text{Mo}}$	H. Rose.
Tungstenium ...	$2 \text{W} \underline{\text{Cl}^3} + \underline{\text{W}}$	Bonnet.
Do.	$\text{W} \underline{\text{Cl}^3} + 2 \underline{\text{W}}$	H. Rose.
Chromium	$\text{Cr} \underline{\text{Cl}^3} + 2 \underline{\text{Cr}}$	Do.

You observe then that the chloracetic acid of M. Dumas is merely a combination of oxalic acid with a chloride of carbon, corresponding to the oxalic acid itself. Its acid characters will be hereafter considered.

3. Compound Radicals.

Radical.	Formula.	Authors.
Acetyle $2 \text{C}^4 \text{H}^6$	$\underline{\text{Cl}^3} + \text{C}^4 \text{H}^6 \text{O}^3$	Malaguti (chloric æther.)
Formyle $\underline{\text{C}^2} \underline{\text{H}^2}$	$\underline{\text{Cl}^3} + \text{C}^2 \text{H}^2 \text{O}^3$	Laurent (acetate of methylen with chlorine).
Benzoïle $\text{C}^{14} \text{H}^{10}$	$\underline{\text{Cl}^3} + 2 \text{C}^{14} \text{H}^{10} \text{O}^3$	Liebig and Whœler (chloride of benzoïle).
Elayle (†) $\text{C}^3 \text{H}^4$	$\underline{\text{Cl}} + \text{C}^2 \text{H}^4 \text{O}$	F. d'Arcet (chloræthæral).
$\text{C}^3 \text{H}^4$	$\text{C}^3 \text{H}^4 \underline{\text{Cl}^3} + \text{C}^3 \text{H}^4 \text{O}^2$	Kane (methylic chloral).
$\text{C}^6 \text{H}^3$	$\text{C}^6 \text{H}^3 \underline{\text{Cl}^3} + \text{C}^6 \text{H}^3 \text{O}^{2*}$	Laurent (chlorophénésic acid).
$\text{C}^6 \text{H}^4$	$\text{C}^6 \text{H}^4 \underline{\text{Cl}^2} + \text{C}^6 \text{H}^4 \text{O}^{2*}$	Id. (chlorophénésic acid).
$\text{C}^7 \text{H}^8$	$\text{C}^7 \text{H}^8 \underline{\text{Cl}^2} + \text{C}^7 \text{H}^8 \text{O}^{5*}$	Piria (chloride of salicine).
$\text{C}^{14} \text{H}^{10}$	$\text{C}^{14} \text{H}^{10} \underline{\text{Cl}^5} + 4 \text{C}^{14} \text{H}^{10} \text{O}^{5*}$	Id. (Id.)
$\text{C}^{14} \text{H}^6$	$\text{C}^{14} \text{H}^6 \underline{\text{Cl}^3} + \text{C}^{14} \text{H}^6 \text{O}^3$ $+ \underline{\text{NH}^3}$	Id. (chlorosamide).

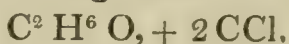
The combinations marked with an asterisk possess the property of uniting with bases without abandoning the chloride combined with them, resembling in this respect the vegeto-sulphuric and vegeto-nitric acids, &c. &c. into the composition of which many different substances may enter. These combinations are five in number: observe then that the chloracetic acid is not a solitary instance of a compound of this kind, and that in order to explain its properties, it is by no means requisite

† The name which I have given to the substance $\text{C}^2 \text{H}^4$ in the oil of the Dutch chemists and in the æthereal salts of platina.

6 M. Berzelius on the *Theory of Substitutions of M. Dumas.*

to isolate it from other analogous combinations, and on its account to imagine new views contrary to the general system of chemical knowledge.

The chloric æther of M. Malaguti may be considered in two different points of view. As obtained directly with æther and chlorine, it is very probable that it is composed, as above mentioned, of an atom of acetic acid and of two atoms of chloride of carbon, corresponding to the radical of this acid. But when, for example, benzoate of ethyle is decomposed by chlorine, it appears that the substance produced, and which remains in combination with the benzoic acid, has the formula which I gave in a preceding letter,



I will also add some examples of combinations of oxichlorides with other bodies; but they are confined to the oxichlorides of carbon. For the sake of brevity,

E will be = $\text{C}^4 \text{H}^{10}$, ethyle;

Me = $\text{C}^2 \text{H}^6$, methyle;

F = $\text{C}^2 \text{H}^2$, formyle.

$(\text{C } \underline{\text{Cl}^3} + \ddot{\text{C}}) + (\text{S } \underline{\text{Cl}^3} + \ddot{\text{S}})$ A. Marcet and myself (sulphuret of carbon treated with nitromuriatic acid).

$(\text{C } \underline{\text{Cl}^3} + \ddot{\text{C}}) + \dot{\text{E}} \ddot{\text{C}}$ Dumas (chloroxycarbonic æther).

$(\text{C } \underline{\text{Cl}^3} + \ddot{\text{C}}) + \text{Me } \ddot{\text{C}}$ Do. (chlorocarbonate of methyle).

$(\text{C } \underline{\text{Cl}^2} + \ddot{\text{C}}) + \text{F } \underline{\text{Cl}}$ (Chloral).

$(\text{C } \underline{\text{Cl}^2} + \ddot{\text{C}}) + \text{F}^2 \underline{\text{Cl}}$ Kane (methylic chloral).

In the last the radical may also be considered as $\text{C}^4 \text{H}^4$, that is to say, that of the citric, tartaric and succinic acids.

In order then to avoid the revolution which threatens electro-chemical views, it will be seen we have only to place the symbols of the formula of chloracetic acid in an order rather different from that of M. Dumas; and by this slight change the new combination is placed in a class of previously known bodies. An epoch has arrived in which a glimpse of a chemical theory of organic combinations has been perceived; but if instead of allowing it to develop as experience extends, it is wished to base it upon isolated facts, viewed without regard to their relations with the system of general knowledge, and on explanations which do not harmonize with the principles of the science; and if, besides, it be

concluded that this want of agreement should make us reject as erroneous principles which are otherwise well founded, the truth will never be arrived at.

This is nearly all which I have thought necessary to say upon this occasion for the defence of electro-chemical opinions; but before I conclude, I will add a few words relative to the discussion on the organic metamorphoses produced by heat, a discussion in which MM. Dumas, Peligot, and Payen have given an opinion contradictory to mine.

M. Dumas thinks that I have deceived myself, because he has found an ethylic combination which includes metamorphosed citric acid. The existence of this combination proves neither more nor less than the existence of the other metamorphosed combinations.

M. Payen has shown that the amylate of lead, heated to 266° Fahr. yields no more water when heated to between 266° and 356° , without being partially decomposed. This is precisely what I have myself proved. I did not examine what took place below 266° . With regard to this I referred to the data contained in the memoir published by this skilful chemist upon starch.

M. Peligot, who, I know not why, considers as an attack upon him the explanation which I have given of an observation made by him, and the accuracy of which I have verified, M. Peligot endeavours to prove that I have advanced inaccurate facts. I am persuaded that M. Peligot obtained the results which he quoted in opposition to my views; but I must beg of him to believe as much of mine. The question is then reduced to determining the conditions under which a body metamorphosed by heat is reproduced by the action of water, and those under which it is not. A careful study of organic metamorphoses will, perhaps, discover these conditions. I shall perhaps at some future period return to this subject.

Your devoted friend,

JAC. BERZELIUS.

Stockholm, July 15, 1839.

Additional Note on Oxichlorides with Compound Radicals.

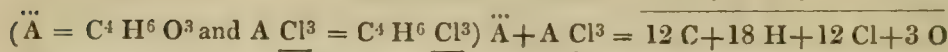
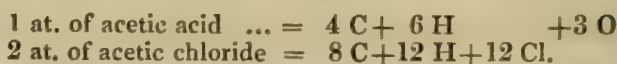
By M. BERZELIUS*.

THE excellent work of M. Malaguti on the reaction which chlorine exerts on æthers, affords no less than seven new examples of this kind of combination.

1. *Chlorinated sulphuric æther* of M. Malaguti is composed, as we have shown, of

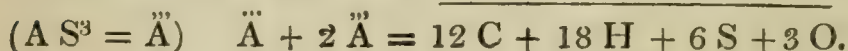
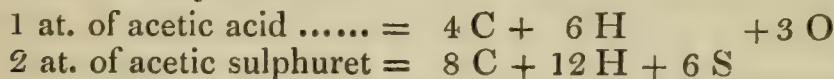
* From the *Ann. de Chim. et de Phys.*, vol. lxi. p. 340.

8 M. Berzelius on Oxichlorides with Compound Radicals.

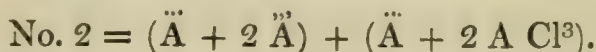


which contains three times the number of simple atoms, in the formula of M. Malaguti = $\text{C}^4 \text{H}^6 \text{Cl}^4 \text{O}$.

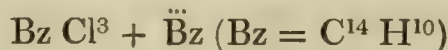
2. *Sulphuretted æther*, $\text{C}^4 \text{H}^6 \text{S}^2 \text{O}$, contains, in tripling the number of simple atoms,



3. *Chlorosulphuretted æther*, = $\text{C}^4 \text{H}^6 \text{S Cl}^2 \text{O}$, the number of simple atoms being multiplied by 6, contains two atoms of acetic acid, two atoms of acetic sulphate, and two atoms of acetic chloride, which may be represented by the combination of an atom of No. 1. and an atom of

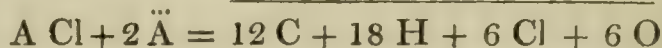
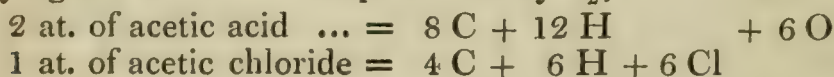


It is probable that in treating the acetic oxichloride (No. 1) by sulphuret of lead, we shall obtain No. 2 or No. 3, as the decomposition may be more or less complete; just as when



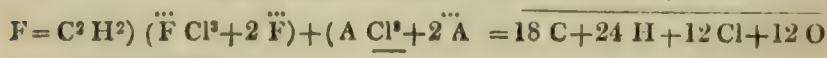
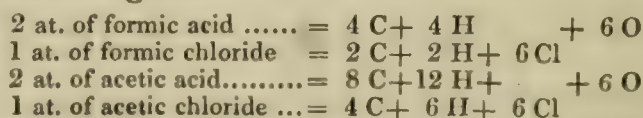
is treated by sulphuret of lead, we obtain $\ddot{\text{Bz}} + 2 \ddot{\text{Bz}}$.

4. *Chlorinated acetic æther*, $\text{Cl}^8 \text{H}^{12} \text{C}^{14} \text{O}^4$, gives, on multiplying the number of simple atoms by $1\frac{1}{2}$,

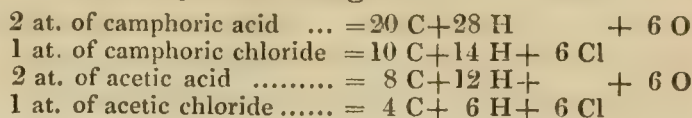


This combination is proportional to those of sulphur, chromium, molybdenum, tungsten, and benzoyl, with chlorine and oxygen.

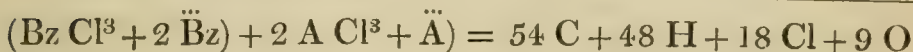
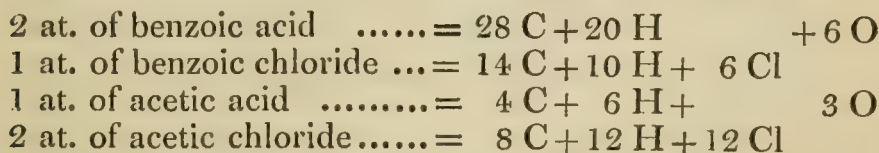
5. *Chlorinated formic æther*, $\text{C}^6 \text{H}^8 \text{Cl}^4 \text{O}^4$, of which if we triple the number of atoms gives



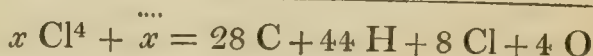
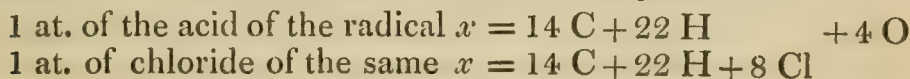
6. *Chlorinated camphoric æther*, $C^{14} H^{20} Cl^4 O^4$, of which we triple the number of simple atoms, gives



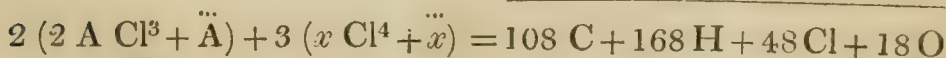
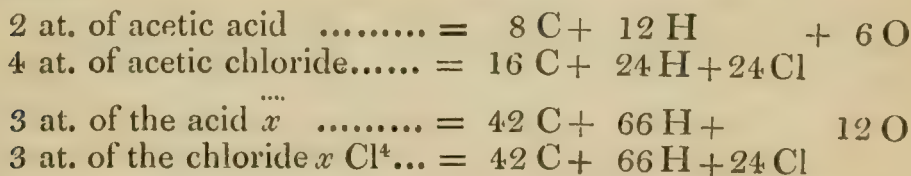
7. *Chlorinated benzoic æther*, $C^{18} H^{16} Cl^6 O^3$, multiplied by 3 gives



8. *Chlorinated œnanthic æther*, $C^{18} H^{28} Cl^8 O^3$, does not contain the radical of œnanthic æther. M. Malaguti has found that the alkalis, in destroying the acetic oxichloride, combine with a new acid, which he analysed, and found to be composed of $C^{14} H^{22} Cl^4 O^2 + \dot{H}$, the radical of which consequently is $C^{14} H^{22}$. Expressing this radical by the symbol x , the acid in the anhydrous state will be composed of



It is therefore one of those oxichlorides, the acid of which combines with the bases without separating the chloride. If, as M. Malaguti supposes, and as is probable, this body enters into the composition of chlorinated œnanthic æther, this latter contains



which makes two atoms of acetic oxichloride for three atoms of the oxichloride or radical $C^{14} H^{22}$, and six times the number of simple atoms indicated in the formula of M. Malaguti.

With regard to the methylic acetate, it appears that it fur-

nished the same combinations as the ethylic formiate, as indeed would be foreseen by theory.

Chloropyromucic æther appears to contain, according to the last experiments of M. Malaguti, ethylic oxide, combined with an acid which is composed of an acid and of a chloride. When it is saturated with bases the acid does not separate from it. Any conjecture as to the composition of the acid and of the chloride united with it would at present be premature.

Stockholm, August 20, 1839.

BERZELIUS.

II. *Experiments on Mechanical Exosmose with reference to determining the Existence of any difference of Magnitude between liquid and gaseous Particles. By JULIUS JEFFREYS, M.R.C.S. late of the Hon. East India Company's Bengal Medical Service.**

EXPERIMENTAL inquiries into the molecular constitution of bodies, being in their nature very difficult, no means of bringing the position or the relative magnitude of their particles under observation have, so far as I am aware, suggested themselves, if we except a faint light which of late electricity, crystallography, and optics have beautifully thrown in among them.

The subject having been of too much interest and importance to be neglected, much speculation has supplied the place of experiment. In physical inquiries, however, hypothesis, valuable in connexion with experiment, is dangerous to philosophy without it, and I should not venture a word of the former, but in its relation to the latter.

The experiment I have to relate may be called, in the absence of any other term, mechanical exosmose. It was performed indeed in 1829, some years before this word was introduced, to denote the interesting phænomena brought to light in connexion with the diffusion of fluids, and it served, I think, to determine something regarding the relative magnitude of liquid and gaseous particles. Though it did not lie in my power at the time to subject to trial more than one gaseous fluid, the experiment not only went to compare the size of its particles with liquid heterogeneous ones, but with such as are homogeneous also. It appeared to establish in a manner sufficiently conclusive, a considerable difference between the magnitude of particles of the same body in its liquid and gaseous state.

Moreover, though limited to a few subjects, a wider object

* Communicated by the Author.

was attained, since it served to point to a method of entry upon a new and interesting field of inquiry.

The phænomena of exosmose which science has of late years made known to us, especially through the valuable experiments of Professor Graham, differ from that of the experiment under consideration in being of an elective character; the transmission varying indeed with the density of gaseous fluid, but yet taking place spontaneously, or in a manner that scarcely admits of a mechanical explanation. Whereas in the case under consideration the fluids were in a state of so great compression within the vessels, that those which passed through their pores were forced out chiefly in a mechanical way.

The material of the vessels was an aluminous composition of considerable density made into bottles of a spheroidal shape, and into these the substances experimented on were condensed.

The means for the experiments offered themselves in a manufactory of factitious mineral waters I was led to establish in the East Indies, at the suggestion of friends, by which a course of experimental pursuits, commenced from the time of my entry into that country, were diverted from a philosophical into a commercial channel. Works, however, were thereby established, as in the case of many of our manufactures here, yielding apparatus which might be employed in scientific experiments, and which for the sake of philosophy alone never would have been constructed.

Earthy minerals may be compounded in a great variety of mixtures, both as to proportion and to mechanical division, which upon being exposed to heat shall undergo various kinds of hardening and condensation; and there are many which pass into different degrees of vitrification, though few can be turned to any useful purpose, for reasons curious in their kind, but which it would be out of place to detail here.

Of the few useful vitrifiable earthy combinations, some in which silica predominates, with a lime or gypsum flux, undergo little condensation up to a certain temperature, when a partial fusion suddenly takes place, and a vitrified and sometimes beautiful texture is obtained where the heat is nicely under command. Of such compounds I had several varieties, but they are not of a nature suited to the objects of the experiment; and I refer to them only that in any repetitions of it compositions of this nature might be avoided.

Another class of vitrifiable mixtures may have a body chiefly aluminous, and one of these combined with a felspathic earth and a small proportion of iron oxide produced the ware

which *yielded* the experiments. This ware underwent, from a dull red up to the time it was at a full white heat, a regular progressive contraction of its parts, by which time it had lost one fourth or one fifth in length. Commencing with pores so open that they would transmit water or gas with little resistance, it became at the heat of fused cast iron as close and dense in texture as glass; and when cooled, was equally impermeable by fluids under any pressure, exerted during any length of time.

Such a substance in its passage through every intermediate degree of density, from that at which all fluids were allowed to pass through it, to that at which they were all confined, might be expected to arrive at some one, at which it would detect any difference in the size of the molecules of two fluids pressing against it for passage, by detaining the one but transmitting the other.

The results were curious. Vessels formed of this composition thick enough to bear the pressure within, were filled with water, containing a small quantity of mineral alkali, and into which was condensed about twelve times its bulk of carbonic acid gas under a pressure of nearly eight atmospheres; their mouths being effectually secured so that no leakage whatever took place through them. The alkali varied from half a drachm to a drachm and a half of carbonate in twenty ounces. Such vessels as were in their most porous state, having been only hardened at a full red heat, allowed both the water and the gas to be ejected by the repulsive force through their pores, the liquid being projected in every direction in the form of a fine rain or cloud, until the pressure within was relieved.

Such vessels again as were in a properly effective condition, having been exposed to their maturing heat, retained their contents without any apparent loss for a year; and when placed near a fire would burst with a violent explosion, without allowing any exudation to take place.

A third kind of the same composition, apparently intermediate as to density between the two already noticed, allowed the gas alone to make its escape through the pores, rather quickly at first, but as the pressure lessened more gradually, until an equilibrium was nearly established, the whole or nearly the whole of the liquid being retained.

In a fourth kind a density was attained much above the last, but short of that of the entire impermeability of the second kind described, and this texture yielded results very curious in themselves, and especially so when contrasted with those last described. Here instead of the gas escaping, and

the liquid being retained, the reverse took place. In process of time, varying from two to four months, every drop of the water was forced out, so that when the vessel was opened it was as dry within as when it came out of the furnace; yet the gas was retained highly condensed, and made its escape with a loud report.

On one occasion a violent explosion took place from a blow which I had reason to suppose would not have been sufficient to cause it, had alkaline solution been present to restrain the elasticity of the gas. In one case the outer surface of the ware was frosted over with the efflorescent alkaline carbonate, which had passed through it in a state of solution: the water was all gone, the vessel being as before perfectly dry within, yet the gas apparently all present.

The power that effected the transmission of the water and alkali being the elastic force of the gas, I could conceive it to act in no other way than mechanically, and to indicate therefore some difference in the size of the elastic and the liquid molecules; the former being too large to find passage through the pores of this closer ware, while the latter, both aqueous and alkaline, all escaped. So far the gaseous were heterogeneous to the liquid particles; and the difference of size indicated might be supposed to depend upon a difference of their original magnitude, and not of magnitude arising from their liquid and gaseous condition. But it will be observed that the alkaline *carbonate* travelled out with the water. Here therefore was carbonic acid in a liquid state, forced out through apertures which refused exit to the same matter in the state of gas. By this it would appear that the atoms of matter are indeed invested, as has been supposed, with an æthereal atmosphere elastic in itself, but forcibly drawn around the atom, so that it can find passage only where there is space for its æther to accompany it, and that this æthereal investment being much larger around gaseous than around liquid particles, apertures through which the latter may be forced may nevertheless be too small for the passage of the former. It appears to me difficult otherwise to explain the phenomena of this experiment.

Again, in that case in which the reverse took place, namely, an exit of the *gas* and a retention of the *liquid*, and which was a result most persons would expect, inasmuch as it is commonly more easy to make vessels water-tight than air-tight, a sufficient explanation may be given which shall not be incompatible with the case just recorded, and which may remove the apparent incongruity of the two experiments. In this case, though the pores were free enough to give passage both to

the water and to the gas, the density was still sufficient to detain the water during the time the gas kept extricating itself, and constantly occupying the pores of the ware to the exclusion from them of the water. Though the chief force, the mechanical one, acted upon both fluids, a little might turn the scale in favour of the relief taking place by a departure of the one and not of the other. Now quite enough to decide the point in favour of the exit of the gas might be found in the well-established diffusive tendency of gases shown in the phænomena of electric exosmose, by which the gas on the one hand would seek a departure into the atmosphere through the ware, while on the other hand the cohesion of the liquid particles would favour their remaining together within it. This process went on until the gas was nearly all discharged, the small remaining quantity being detained by the affinity of the alkali and the water.

In the instance of great porosity first noticed, the results explain themselves at once. Here there was not closeness of texture sufficient for even such a detention of the contents as would enable the gas and liquid to part, and the process of exosmose to remove the former; but both fluids were propelled through the pores, the water in fine rain which formed into a mist.

Atmospheric air was not tried with water, on account of the difficulty of retaining it condensed into water while the vessel is being secured; though with apparatus devised for the purpose the experiment might easily be made, if ware of the peculiar closeness were procurable. The impregnation of water with hydro-sulphurous acid gas, which was being conducted on a large scale at the time, would have enabled me to try it, had the machinery constructed for that purpose been capable of exercising a pressure of more than two atmospheres, which was not sufficient for the effect.

Of the compositions of which the various kinds of stoneware and china are manufactured in England, there are probably some which by a certain heat would be brought to the points of density suited for the experiments. Specimens of Staffordshire earthen and China ware, however, (all of which are absorbent,) upon being treated with greater heat than those at which they had been manufactured, did not pass into any state of great density, but after a certain approximation of their parts, a decomposition of them commenced, attended with a swelling or enlargement of the whole, and an increase of porosity. I also doubt if the body used for the Derby porcelain would pass at any heat into the suitable texture; and from experiments made on oriental china from Canton,

I do not think it could be conducted to the requisite point of closeness without an opposite result taking place to the opening of the pores of Staffordshire ware by intense heat, namely, a closing up of all pores by a vitrification of the body; the Chinese ware being evidently very siliceous, and corresponding in its transitions with mixtures I have already referred to.

There is a brown stoneware manufactured in a large pottery at Belper in Derbyshire, which, from certain qualities, I should judge to be formed of a body likely to yield, at different degrees of heat, the several approximations of parts suited for these experiments, provided it were protected from a glazing of the surface, which would cause an imperfect result, though no glazing is impermeable by fluids under pressure. And it is proper to remark, that although I have described only four degrees of density, they were selected out of several yielding intermediate results; which would be requisite before the heat for the experiments were obtained. These remarks may not appear irrelevant, since they may serve as some guide to a person desirous of repeating or extending the experiments.

The familiar fact that water may be retained in a porous earthen pitcher for a length of time without any escaping excepting by evaporation, although the lower part is under the pressure of a foot or more of the fluid, while such a pressure would suffice to force air quickly through the pores, would commonly be explained by supposing a greater minuteness of parts in the case of air than of water; but the above experiments would prove the contrary.

The right explanation is to be found in the case of the liquid, in the cohesive attraction between the particles which indispose the liquid to that breaking up into minute portions requisite before it can enter the pores, and where it has entered the pores in the capillary adhesion to the substance of the ware by which the liquid refuses to leave its outer surface and run down; while in the case of the gaseous fluid a mutual repulsion favours the separation of its particles, and there is no considerable attraction between them and the earthy surfaces to resist their passage out; the greater subtilty of the air over the water being due to these causes and not to its being composed of minuter particles. In some measure related to this subject I had to notice some interesting facts connected with the transmission of saline liquids through woody fibre, but the space of this paper does not admit of their being detailed here.

JULIUS JEFFREYS.

III. *On Photometry in connexion with Physical Optics.*
By R. POTTER, Esq., B.A., F.C.P.S., Medical Fellow of
Queen's College, Cambridge.*

THAT Photometry should have excited some attention amongst scientific men, could scarcely fail of proving a cause of satisfaction to the writer of the present paper, who had several years ago urged its great importance in laying an experimental foundation in Physical Optics. If the subject had been approached in a philosophical spirit, with a real desire to find out the truth according to the principles laid down by Lord Bacon, that satisfaction must have been the writer's privilege. Very different however has been the notice it has called forth.

The dazzling fruits of Fresnel's genius had so blinded the scientific world, that it has been held to be presumption to examine minutely the accuracy of his results, although his constitution of mind must have indicated the need of this, to the most cursory reader of his various papers. We there see the zeal of the advocate to carry his point, overruling his judgement, and leading him to make objections against the theory he opposed, which were frequently either incorrect in their foundation, or were of trivial importance.

It matters little to say such and such things are in themselves improbable or unintelligible; for if the theory give results in accordance with natural phænomena, we are bound to receive it, although many of its consequences should seem strange and even inadmissible at the first view: thus, I conceive, no one would be justified in rejecting the undulatory theory of light, or any other, if its results were in accordance with experiments.

The fashion of pinning their faith on Fresnel's sleeve having become general amongst the influential in learned societies, and amongst the most eminent in mathematical attainments, it was natural that minds of smaller calibre, or reputations of smaller weight, should feel it a readier and surer way to honours and distinction to follow in the tide, without venturing to dispute the judgement of their superiors or to question their infallibility. Under such circumstances the mere analyst must find himself floating in the tide with those who claimed a more profound knowledge of experimental philosophy, and who were sure to applaud the aid he rendered to the common cause in symbols and formulæ, of which they were frequently as little able as willing to investigate the accuracy.

It is thus that almost any analysis, which professed to support the undulatory theory of light, has been hailed as a magnificent achievement without its minute bearings being ever looked into, provided the general case showed a *prima facie* accordance with some known facts. In no place has this been more prominent than in the Cambridge Philosophical Society.

He is a young man and inexperienced who has so little knowledge of human nature as to suppose that in this state of matters any scientific truth, of however important a bearing, which did not fall in with the popular and fashionable prepossessions, must be decried, and its discoverer would naturally be held out as a factious and refractory pretender in the republic of science.

The scientific public must sooner or later awaken and perceive the liberty which has been taken with its confidence, and there must sooner or later arise a time of return to sound philosophizing in physical optics, when the maxims of Lord Bacon will be acknowledged as the only sure guide.

My objections to Fresnel's formulæ for the intensity of light reflected and transmitted by transparent bodies, although founded on laborious and careful experimental researches, have been treated as though other men's guesses were more worth than my experiments. I shall, however, before I close this paper, bring corroborative evidence of the accuracy of my results which fortunately exists in print amongst the labours of Bouguer and of Sir William Herschel. Dr. Faraday also has given some photometric measures in his Bakerian lecture on the manufacture of glass for optical purposes. If the refractive indices of the heavy optical glass had been given, we should have had a good test of Fresnel's formulæ from the experiments with those glasses. The angle of incidence which Dr. Faraday employed does not furnish an *experimentum crucis* for common glass.

The subject of photometry has been discussed by Professor Lloyd in his report on Physical Optics, read before the 'British Association for the Advancement of Science;' by Professor Powell, in a paper read before the Newcastle meeting of the Association, and lately, by Professor Forbes, in a paper read before the Royal Society of Edinburgh, of which an abstract has been printed under the title "Memorandum on the Intensity of reflected Light and Heat. By Professor Forbes. (From the Proceedings of the Royal Society of Edinburgh, March 18, 1839)."*

As Professors Lloyd and Powell did not think it necessary to make themselves acquainted with the subject they under-

[* Inserted in L. and E. Phil. Mag. for December 1839, vol. xv. p. 479.]

took to discuss, their observations do not call for any further notice in this place. With Professor Forbes, however, I have a much graver controversy.

In regard to Professor Forbes's 'Memorandum,' we must strive to find his meaning in some curious passages. In speaking of Fresnel's formulæ for the intensity of reflected light, he says "scarcely any attempt has been made towards its verification by direct experiment." Before we assent or dissent from the Professor's assertion, we should know what he means by *an attempt at verification*. Attempts, and successful ones too, have been made to find the quantity of light reflected at various incidences by transparent bodies, by the celebrated Bouguer*, a century ago, and by myself latterly†, in a series of experiments which occupied me several months.

As Fresnel's formula will not give either Bouguer's quantities or mine, our labours must be very unsatisfactory, having done nothing towards that desirable verification. Professor Forbes's paper will probably supply this desideratum. A little further on he says, "It occurred to me, about the end of 1837, that the anomalies of photometrical observations being nearly as unsatisfactory as ever, some light might be thrown upon this important subject by ascertaining the law in the case of heat, the intensity of which we have no difficulty in measuring."

With respect to his actual experiments he says, "I have this winter resumed the subject. I have had an apparatus constructed for securing sufficient accuracy in determining the angle of incidence, and I have used reflecting surfaces, both transparent and metallic; the former are wedges of plate-glass, by means of which reflection from the first surface only may be observed, and the latter are plane specula of steel and silver. The prosecution, however, of these apparently simple experiments, has been attended with unforeseen difficulties; and although the relative proportions of heat at different angles of incidence are now pretty well determined for glass in several cases, I am not prepared to say whether the absolute amount is exactly the same as Fresnel's formula would give, assigning to heat its proper refractive index. It is satisfactory, however, to know, that the approximation to it is much greater than direct photometrical measures have yet given, with the single exception of two experiments of M. Arago already referred to; and that I have reason to believe that the experimental law which Mr. Potter has given from direct observation in the case of light, represents my results much less accurately than the theory of Fresnel." This undulatory pas-

* See Bouguer's *Traité d'Optique* or Priestley's *History of Vision*.

† See *Edinburgh Journal of Science* for 1830.

sage, with its ambiguous sign prefixed in the “unforeseen difficulties,” shows that the methods are not likely to furnish results accurate enough for testing important laws of nature. It is not to be wondered at that the Professor, in much variation of results, should find some which were ‘*satisfactory*.’ As a practical answer I here give some comparisons between my own results and those of others. The most striking and most essential difference between Fresnel’s theoretical formula and my experimental one is, that according to the former the whole incident light should be reflected at 90° incidence, or rather, that this is the limit to which the intensity of reflection tends at very high incidences; my formula gives only a quantity varying from about 72 to 76 rays reflected out of every 100, as this limit for the ordinary kinds of crown plate and flint glass. Hence we have a difference which cannot be hidden in errors of experiment. This was one of Bouguer’s particular points. In speaking of the light reflected by water, he says (p. 135), “dans l’incidence pour ainsi dire infiniment petite la lumière réfléchie est les trois quarts de l’incidente ou de la directe.” We must note here that he measured his angle of incidence from the surface and not from the normal, and that a part only, instead of the whole, was reflected at the highest possible incidences. Below I give Dr. Faraday’s results with crown, plate, and flint glass, at 45° incidence (the only angle he used) with my own at 40° and 50° , from the before-named papers, as well as some calculated results.

From Dr. Faraday’s results.

Crown glass, No. 3, reflected	$\frac{1}{24.2}$	= 4.13	of every 100 rays.
„ 7, „	$\frac{1}{23.3}$	= 4.29	„
„ 6, „	$\frac{1}{22.1}$	= 4.52	„
Plate glass, No. 2, „	$\frac{1}{26}$	= 3.85	„
„ 8, „	$\frac{1}{25.8}$	= 3.88	„
Flint glass, No. 9, „	$\frac{1}{19.5}$	= 5.13	„

From my own results.

Incidence,	Crown glass, reflects. of every 100.	Plate.	Flint.
40° . . .	4.94 . . .	4.78 . . .	5.29
50° . . .	5.68 . . .	5.92 . . .	6.73

Calculations by my own formula:

Incidence.	Crown.	Plate.	Flint.
40° . . .	4.767 . . .	4.778 . . .	5.320
45° . . .	5.205 . . .	5.243 . . .	5.884
50° . . .	5.810 . . .	5.882 . . .	6.656

Calculations from Fresnel's formula from Sir David Brewster's paper, Phil. Trans. 1830 :

Incidence.	Crown glass, reflects. of every 100 rays.
40°	4.910
45°	5.366
50°	6.136

It appears from the above that Dr. Faraday's experiments, making every allowance for different kinds of glass being used in the different experiments, do not yield that desirable verification which Professor Forbes found amongst his results. However it will be seen that we are not to seek for *experimenta crucis* at the above angles of incidence, or at any rate, with such low refracting substances. Dr. Faraday's experiments with the heavy optical glasses would have furnished them if we had had the refractive indices of those glasses; for in high refracting bodies the discordance of Fresnel's formula with experiments is palpable, for it gives results frequently one-half more, to twice as much as experiment.

In the Professor's next sentence I have to complain of a want of candour. My first introduction to physical optics was the discovery by experiments, of the law of reflection by metals, of a nature quite different from the suppositions of the scientific world, as well as of myself previously. Sir Isaac Newton had stated his opinion, such as had been received ever since; Bouguer most likely had it presented to him in his experiments with mercury, but blinded by the commonly received theory, could not see it: for he complains, that after all his precautions the fine dust fell so quickly on his mercury as to hinder the reflection at the higher incidences. Sir William Herschel had examined his splendid specula by photometry without falling upon it. Is it to be wondered at, that I should feel greatly proud of this discovery, my first effort in physical optics?

Professor Forbes says, "With respect to reflection at the metals, I believe I may assert that I have verified the remark of Mr. Potter, that metallic reflection is *less* intense at the higher angles of incidence. I have attempted to ascertain whether it reaches a minimum, and then increases up to 90° of incidence, as Mr. Maccullagh supposes; but I have not obtained decisive results. The quantity of heat reflected by the metals is so much greater than Mr. Potter's estimate for light, as to lead me to suspect that his photometric ratios are all too small, which would nearly account for their deviation from Fresnel's law."

Here my discovery comes to be merely a "remark." I

think it should pass for a discovery amongst Professor Forbes's remarks.

The comparison of the former and the latter part of this sentence is a curiosity. In the first part he considers that he has experimentally verified my law, that metals reflect less light at higher incidences, and in the second he finds reason to suspect that my 'ratios' are all too small, which would nearly account for their deviation from Fresnel's law. Now, by Fresnel's formula more light would be reflected at higher incidences. How are we to understand the Professor's meaning of the word *verification*?

I consider Bouguer's observation that he found the dust fall so fast on his mercury as to hinder the reflexion at higher incidences, to be more proof of the correctness of my law, than Professor Forbes's experiments with steel and silver mirrors. It is well known that silver will not polish so as to make telescopic mirrors, and steel is very difficult to polish for that purpose. If the Professor's mirrors are the labours of the cutler and silversmith, I would not value the results they give at a straw, however skilfully and carefully made. With opticians, the most difficult thing they attempt, is to obtain a perfectly flat surface, and so essential in my experiments did I esteem it, that I used my mirrors as the oval mirror in a Newtonian telescope, and proved their accuracy by the distinctness with which double stars were exhibited in it. I stated with respect to the steel mirror, of the figure of which I was less certain than of the others, as follows: "As I had only ground and polished it in the common way for flat surfaces, I was not certain that it might be truly plane, and thought necessary to prove it on some astronomical objects. Accordingly, on the 19th February, with it and a $5\frac{1}{2}$ inch speculum of my own workmanship, of about 50 inches focal length, and with a power of 100, I saw α geminorum beautifully and distinctly defined; and with a power of 150 saw γ leonis to be double at the first view, which I think will be allowed to be a sufficient test of its surface being nearly plane."

Professor Forbes will perceive that my conception of the mode to be pursued in order to discover laws of nature was a good deal different from his own.

With respect to the quantity of light reflected by metals being greater than I had found it, I have need only to give Bouguer's results, and that of Sir William Herschel, side by side with my own, and then to leave the scientific world to judge of the weight to be allowed to the Professor's experiments. Bouguer, in a preliminary discussion, says he found 754 rays to be reflected by mercury of every 1000 at $11\frac{1}{2}^\circ$ in-

clination to the surface, and in another experiment he found 703 reflected. When afterwards he is speaking more particularly on the subject, he says he found at 21° degrees inclination in two experiments 637 and 666 reflected of every 1000. Sir W. Herschel (Phil. Trans. for 1800.) found that his specula reflected 67262 of every 100 thousand rays at an incidence nearly perpendicular.

In my paper on the reflective powers of metals, published in the Edinburgh Journal of Science for 1830, one speculum gave 69.45 rays reflected of every 100 incident at 20° incidence; another set of experiments gave 68.61 at 10° incidence, and another 66.42 at the same incidence; the deterioration in the successive sets being attributed chiefly to my rubbing the speculum too severely, with the intent to prevent being deceived at the higher incidences by any film remaining on the surface.

In the beginning of his memorandum he refers to a paper by Mr. Green, B.A., Fellow of Caius College, in this University, which appears in the forthcoming volume of the Camb. Phil. Trans. which he says accords with Fresnel's formula. This is an example of my statement that any *primâ facie* case is sufficient with the advocates of the undulatory theory. Mr. Green finds that the intensity of the light reflected at the polarizing angle of a transparent substance, when the incident light is polarized in a plane perpendicular to the plane of incidence is represented by the formula

$$\frac{(\mu^2 - 1)^4}{4\mu^2(\mu^2 + 1)^2 + (\mu^2 - 1)^4}$$

for glass and $\mu = 5.1$ this becomes $\frac{1}{10}$ very nearly, which is more than one-half the light reflected at the perpendicular incidence even taking the undulatory formulæ, which give $\frac{1}{25}$, and if such a proportion still existed no one would say that light polarized in the plane perpendicular to the plane of incidence vanishes at the polarizing angle.

To test this further I will use an experiment brought forward by Mr. Airy, in the fourth volume of the Camb. Phil. Trans. in a paper to which Mr. Green refers. It is there stated, and easily verified, that when Newton's rings are formed by placing a piece of glass on diamond, the rings disappear at the polarizing angle for the glass when examined by light polarized perpendicularly to the plane of incidence. Hence there is not light reflected from the glass surface of the thin plate of air, which could interfere with the light from the diamond surface.

Now in Newton's rings, formed between two glasses and

seen by transmission, the light twice reflected causes rings which are easily visible, although one portion of the interfering light is less than $\frac{1}{600}$ th part of the other by the undulatory formulæ, and about $\frac{1}{900}$ th by my experiments; also, in the former experiment, the polarizing angle for glass ($56\frac{1}{3}^\circ$ nearly) is approaching towards the polarizing angle for diamond (about 68°), therefore, the whole light reflected by the diamond would be only a small fraction of the incident light, and that by the glass, according to Mr. Green's formula, would bear a considerable ratio to it, and therefore the rings should be very distinct, in place of which none are to be seen at all; so that Mr. Green's formula neither represents the facts nor agrees with Fresnel's.

Professor Forbes also says, in the second sentence of his memorandum, "but the chief evidence for the truth of this remarkable law rests on the indirect observation of the change of the plane of polarization of an incident ray after reflexion." He here alludes to a series of valuable experiments made by Sir David Brewster, and published in the *Phil. Trans.* for 1830. If we look over Sir David's table at page 75, we see the results of the experiments on the change of the plane of polarization after reflexion by diamond, affording errors of observation, although, from the nature of the experiment necessarily large, (averaging $46\frac{1}{2}'$) yet all lying on one side of the calculated results. When the theory is true we naturally expect the errors of observation to fall sometimes on one side and sometimes on the other side of the calculated place. Hence, it is clear, that the undulatory theory, even in this vaunted case, does not give the accurate results.

Queen's College.

IV. *Observations on the Blood Corpuscles or Red Disks of the Mammiferous Animals.* By GEORGE GULLIVER, F.R.S., Assistant-Surgeon to the Royal Regiment of Horse Guards*.

A COMPLETE history of the blood corpuscles of the mammalia would be a very acceptable addition to anatomical science, and probably no less advantageous to zoology. To accomplish this work, however, the co-operation of so many circumstances is necessary, that the contributions of different observers are especially desirable. I have long been engaged in the inquiry; and, as I have been favoured with the liberal permission of the Council of the Zoological Society† to avail myself of the advantages of their collection, I

* Communicated by the Author, Nov. 23, 1839.

† In returning my best thanks on this occasion to the Council, I cannot

propose to give an account of the blood corpuscles of such animals as I have had an opportunity of examining. Of many of these particles I believe no description has hitherto been published; and some others which have been previously described I have examined anew with a view to more accurate comparisons. I am not aware that the peculiar form of the corpuscles in certain species of the genus *Cervus* has been hitherto observed in any class of animals.

The general results appear interesting to me in many points of view, particularly in respect to certain differences in the size of the corpuscles in the same individual at different periods of existence, as well as in nearly allied species of the same genus; and the resemblance, on the other hand, of the blood disks in some groups of the animal kingdom. The connexion, too, between the size and form of the blood particles and the respiratory organs, I have found to be remarkable. But before entering into conclusions it will be necessary to give the observations in detail, reserving a summary arrangement of them, and the deductions which they appear to warrant, for a future section of this memoir, when an opportunity will be afforded of reference to the labours of others in this field.

The instrument made use of in these observations is a compound microscope, with an achromatic object-glass of one-eighth of an inch focal length, made by Ross, and furnished by him with a micrometer eye-piece divided into spaces corresponding to 1-4000th of an inch. The magnifying power afforded is exactly 800 diameters with a clear definition.

If one space and a quarter of this micrometer were occupied by a single globule, this would of course measure 1-3200th of an inch; if three equally-sized particles lying in a line, and touching at their edges, covered three spaces and a half, the diameter of each of these would be 1-3429th,—if four spaces, 1-3000th of an inch. Now these measurements are mentioned because they are very frequently obtained from the average-sized human blood disks, which are to be distinguished from the remarkable varieties which Mr. Bowerbank has observed among them, and which I have also witnessed, though I think in a less degree than he mentions, in man and various animals. In the human blood corpuscle, the diameter last mentioned is not so common as the two former. In the absolute accuracy of any micrometer applied to objects so extremely minute, it is difficult to place implicit reliance; but

avoid acknowledging the kindness and urbanity of Mr. Ogilby, who entertained my application with his usual zeal in the promotion of zoological inquiries. I have also repeatedly been indebted to the kind assistance of Mr. Youatt, the excellent medical superintendent of the Gardens.

in the relative exactness of the instrument I am disposed from long experience to put much confidence, which is of the greatest importance where the results are to be obtained chiefly by comparisons.

The corpuscles were examined thinly spread on glass and quickly dried, also floating in their own serum, and diluted when necessary with weak saline solutions, or sugar and water, or with urine. The objection to these substances is, that they all more or less alter the figure of the globules, generally rendering many of them cup-shaped and diminishing their size slightly. Indeed, the disks kept in their own serum often appear a little smaller in a very short time after they have been removed from the vessels, as if they possessed some degree of contractility. It might be supposed that the particles rapidly dried on glass would shrink a little; but this is not the case, for they retain a remarkably clear and regular outline, and are commonly, to a small extent, larger than those of the same blood exposed to the air in their own fluid.

In some instances there was certainly a slight enlargement in the dried corpuscles, as compared with those seen in their own serum immediately after they were taken from the animal. In the greater number of trials, however, the sizes of the wet and dry disks corresponded accurately. In most cases the measurements were repeated by Mr. Siddall, an experienced micrographer, with another instrument by Ross, so as to avoid as much as possible accidental inaccuracies. The measurements are always expressed in fractions of an English inch.

As the corpuscles are very liable to change in size and form from very trivial causes, the extreme measurements in no case include those large or small particles which occur but sparingly, and which, perhaps, are not identical with the common red disks. Neither the large white globules nor the granulated particles are estimated, because, independently of their spherical shape, the former are almost uniformly larger and the latter smaller than the blood disks. As noticed by Hewson, the common corpuscles become mulberry-shaped when, from incipient putrefaction, their colouring matter begins to dissolve in the serum. But I have observed the granulated particles in great numbers, both in their serum and in the dry state*, in blood examined immediately after it was obtained from the veins of various animals, particularly young kittens. The nature of these particles is worthy of further and special inquiry. They are to be found plentifully during

* I gave Mr. Owen specimens of these granules in September last, mentioning this fact to him, as he considered the granulated appearance to be the effect merely of drying.

digestion; but, in their deep-red colour and chemical properties, they differ remarkably from the granules observed in the chyle.

1. Dog-faced Baboon, hardly half-grown, (*Simia Cynocephalus*). Blood from the right ventricle of the heart. Corpuscles generally 1-3192nd of an inch in diameter; extreme sizes 1-4000th and 1-2666th of an inch, the largest more frequent than the smallest.

2. Green Monkey, adult, (*Cercopithecus Sabæus*). Average-sized corpuscles 1-3200th to 1-3000th of an inch in the dry state, 1-3600th in their own serum. Extreme sizes 1-4000th and 1-2666th. Thickness of the corpuscles as seen on their edges 1-12,000th to 1-10,000th of an inch, i. e. a rouleau of five or six filled two of the micrometer spaces. The blood examined was obtained from both ventricles of the heart, from the vena cava, and vena portæ, no appreciable difference appearing in the corpuscles.

3. White Monkey, adult, (*Macacus radiatus*, albino var.). Corpuscles from the right ventricle generally 1-3200th of an inch in diameter.

4. Rhesus Monkey, old male, (*Macacus Rhesus*). Size of disks very variable from 1-4572nd to 1-2666th of an inch: most common diameter 1-3200th. Blood of both ventricles examined.

a. In a young male the same measurements were obtained.

5. Capuchin Monkey, adult, (*Cebus Capucinus*). Most common diameter of corpuscles 1-3428th and 1-3200th of an inch dried; in their serum, exposed to the air some hours, 1-4000th was a more frequent diameter. Extreme diameters 1-4572nd and 1-2910th of an inch. Blood from both ventricles of the heart examined.

6. Mangabey Monkey, adult male, (*Cercocebus Æthiops*). Common sizes 1-3200th and 1-3000th. Extreme sizes, also common, 1-4000th and 1-2666th. Blood examined from both ventricles of the heart, from the portal and renal veins.

7. Grivet Monkey, (*Cercocebus griseo-viridis*). Most frequent sizes 1-3200th and 1-3000th. Extreme diameters 1-4000th and 1-2666th of an inch. Blood from both ventricles of the heart.

8. Slow Lemur, full-grown, (*Loris tardigradus*). Most common size 1-3552nd. Extreme diameters 1-4000th and 1-3000th. Blood from a prick of the fore-hand.

9. Common Bat, (*Vespertilio murinus*). Blood from right ventricle. Common diameter of corpuscles 1-4365th to 1-4000th of an inch.

10. Common Squirrel, adult male, (*Sciurus vulgaris*). Cor-

puscles from the right ventricle, dried, all about 1-4000th of an inch in diameter.

11. Water Rat, adult, (*Mus amphibius*). Blood from both ventricles, examined dry; average-sized corpuscles 1-4000th and 1-3600th of an inch diameter.

12. Common brown House Rat, a male, nearly full-grown, (*Mus decumanus*). Common size of corpuscles from right ventricle 1-4000th to 1-3554th of an inch.

13. Common Mouse, (*Mus musculus*). 1-4000th and 1-3600th common sizes. Extreme diameters 1-5333rd and 1-3000th. Blood from heart and vena cava.

a. Fœtus, about half an inch long, from the uterus of the above mouse. 1-2666th the most frequent size, and many smaller corpuscles from 1-5333rd to 1-4000th, the latter being numerous. Many of the corpuscles more globular than usual, that is, more like spheres than disks,—a common appearance in the fœtal blood of mammals at an early period.

From this and several other observations, the comparatively large size of the blood corpuscles in the fœtus becomes manifest. The fact is important, because it is connected with the comparative condition of the respiratory organs, and, moreover, illustrates the difference between the blood of the parent animal and that of the fœtus; so that if there be any communication between the vessels of the placenta and uterus, the blood of the mother and fœtus is at all events different. I have examined the fœtal blood of the human subject, obtained from the heart, placenta, and umbilical vessels, with the same result as above; and it appears to me to be of considerable interest*.

14. Common Guinea Pig, adult male, (*Cavia Cobaya*). Corpuscles from the right ventricle, generally measuring from 1-4000th to 1-3200th of an inch.

15. Domestic Rabbit, adult female, (*Lepus cuniculus*). Most common diameter 1-4000th; extreme sizes 1-5000th and 1-3200th of an inch.

16. Golden Agouti, adult male, (*Dasyprocta aurata*). 1-3600th the most common diameter. Extreme sizes 1-5333rd and 1-3200th. Blood from a vein of the ear.

17. Acouchi, full-grown, (*Dasyprocta acouchi*). 1-3600th a common diameter, as was also 1-4000th. Extreme sizes 1-4572nd and 1-3200th.

18. White-whiskered Paradoxure, adult, (*P. Leucomystax*). Size of corpuscles very variable; most commonly 1-4365th and 1-4000th. Extreme diameters 1-6000th to 1-3200th. Blood from a prick of the tail.

* The accurate Hewson observed the same fact in a fowl and in a viper. (Exp. Inquiries, part iii. p. 39.)

19. Paradoxure Gennet, adult, (*Paradoxurus typus*). Most frequent size 1-5331st. Extreme diameters 1-6600th and 1-4572nd. Size of corpuscles more regular than in the preceding. Blood from a prick of the tail.

20. Gray Ichneumon (*Herpestes griseus*), adult male. Most frequent diameter 1-4572nd of an inch. Extreme sizes 1-6000th and 1-3554th. The dried disks had more distinct central marks, or appearance of nuclei, than usual. Blood from a prick of the tail.

21. Domestic Dog, old mongrel, (*Canis familiaris*). Common diameter of corpuscles 1-4000th to 1-3200th of an inch.

a. Fox-hound Puppy, one day old, a bitch. 1-3000th and 1-2666th the most common diameters of the corpuscles. In the blood of the vena portæ they were generally of the latter size.

b. Fox-hound Puppy, twelve days old, a bitch. Most common diameter of the corpuscles 1-3000th and 1-2885th of an inch. Extreme sizes 1-4000th and 1-2666th.

c. Mongrel Puppy, four months old, a bitch. All the following diameters common, viz. 1-3693rd, 1-3554th, 1-3429th and 1-3200th.

22. Australasian Dog, aged four or five years (*Canis Australiensis*). Most common size of corpuscles 1-3200th and 1-3000th; 1-4000th and 1-2666th were seen, but rarely.

23. Fox, old bitch, (*Canis Vulpes*). The following sized corpuscles were frequent, viz. 1-4572nd, 1-4365th, 1-4000th, and 1-3653rd; the third diameter (1-4000th) was the most common. Besides blood from the heart, corpuscles were examined from the cava and splenic veins.

24. Red American Fox, half-grown dog, (*Canis Vulpes*, var. *Americanus*). Common-sized corpuscles 1-4000th and 1-3693rd of an inch in diameter. A few were observed of 1-5333rd; and 1-3200th was rather a frequent diameter.

25. Black American Fox, a dog, about two-thirds grown, (*Canis argentatus*). 1-4000th of an inch most common diameter of corpuscles: extreme sizes 1-5333rd to 1-2666th. (This animal and the succeeding one came to England together from Hudson's Bay.)

26. White Arctic Fox, about two-thirds grown, (*Canis lagopus*). Same sizes as in the preceding.

The measurements in the genus *Canis* were all made from dried corpuscles obtained from the right ventricle of the heart.

27. Domestic Cat, adult female, (*Felis Catus*). Blood from right ventricle. Most common diameter of disks 1-4000th of an inch; a few were 1-4572nd.

a. A Kitten one day old, female. The common size of

disks was from 1-4000th to 1-3564th, and 1-3000th to 1-2666th were frequent sizes. Corpuscles in the blood of vena portæ and right ventricle of the heart apparently identical.

b. A Kitten sixteen days old. The most frequent size of corpuscles 1-4000th. From Mr. Siddall's observation.

28. Serval, adult female, (*Felis Serval*). 1-4000th of an inch the most common diameter of the corpuscles. They were very irregular in diameter, 1-5000th and 1-3000th being frequent sizes. Blood from all the chambers of the heart examined, as well as from the renal vein.

29. Asiatic Leopard, adult male, (*Felis Leopardus*). Disks very variable in size; 1-4800th the common diameter, many varying from 1-5333rd to 1-3200th. The blood was obtained from the ventricles of the heart, from the renal vein and artery, and from the splenic vein, no difference being apparent in the corpuscles.

30. Lynx, nearly full-grown, male, (*Felis Caracal*). Most common diameter of corpuscles 1-4800th to 1-4365th of an inch. Very variable in size, extending from 1-6000th to 1-4000th. Blood from both ventricles examined.

31. Norway Lynx, about two years old, (*Felis Lynx*). Most common sizes 1-4365th to 1-4000th. Extreme diameters 1-5333rd to 1-3554th. Blood from both ventricles.

32. Coati Mondî, old male, (*Nasua fusca*). 1-3200th the most frequent diameter. Extreme sizes 1-4572nd and 1-2666th: the former extreme most common.

33. Red Coati Mondî, adult female, (*Nasua rufa*). 1-4000th and 1-3554th the most common diameters of the corpuscles. Extreme sizes 1-5333rd and 1-3200th of an inch. Blood from both ventricles and from the abdominal aorta, in this and the preceding species.

34. Horse, gelding, aged three years, and another aged five (*Equus Caballus*). Blood from jugular vein. The average-sized corpuscles in both 1-4800th and 1-4572nd of an inch.

a. Gelding, twenty-six years old. Same size generally, but more irregular, many being seen as large as 1-4000th of an inch. In this and the preceding, when collected into rouleaux, the edges of the corpuscles were from 1-12,000th to 1-14,000th of an inch, that is, a rouleau of six or seven disks occupied two spaces of the micrometer.

35. Ass, old female, (*Equus Asinus*). Most common diameter of corpuscles 1-4000th of an inch. Blood obtained from the jugular vein.

36. Dromedary, full-grown male, (*Camelus Dromedarius*). Disks oval. Long diameter: the most frequent was 1-3200th of an inch, and the following were seen, viz. 1-4000th,

30 Mr. G. Gulliver's *Observations on the Blood Corpuscles*

1-3000th and 1-2666th. Short diameter, 1-6000th, the most frequent, and the following were seen, viz. 1-6600th, 1-6400th, and 1-5333rd, and 1-4800th. Thickness of the edges of the disks 1-16,000th to 1-12,000th of an inch. A very few circular corpuscles, about 1-6400th of an inch in diameter, were observed. Blood obtained from a prick in the skin.

37. Vicugna, adult male, (*Auchenia Vicugna*). Disks oval. Long diameter most frequently 1-4000th of an inch; and the following long diameters were observed: 1-5333rd, 1-3200th, 1-3000th. Short diameter; the most frequent 1-6400th; also many varying from 1-8000th to 1-5333rd of an inch. A few circular corpuscles observed, as in the Dromedary. Blood obtained from skin of the leg.

38. Paco, male, nearly full-grown, (*Auchenia Paco*). Disks oval. Long diameter generally 1-3200th of an inch; some 1-4000th, others 1-2666th of an inch. Short diameter, most commonly 1-6400th and 1-6000th; some of 1-8000th, and 1-5333rd. A few circular particles observed, as in the Dromedary and Vicugna. Blood obtained by pricking the ear.

39. Guanaco or Wild Lama, adult female, (*Auchenia Glama*). Blood obtained from the ear. Disks oval, and not differing appreciably from those of the Paco.

Thus in the Vicugna, Paco, and Wild Lama, the blood corpuscles are elliptical*, as the interesting discovery of M. Mandl has shown them to be in the Dromedary. In the Vicugna the disks are rather smaller than in the Dromedary. They all appear to be very thin or flat in relation to their size, their edges in the Dromedary being not more than 1-16,000th or 1-20,000th of an inch thick. I could see no indication whatever of a nucleus in any of the corpuscles; and those of the Dromedary were subjected to the action of water, acetic acid, and various reagents, with a view to determine this point.

40. Goat, adult female, (*Capra Hircus*). Common diameters of corpuscles 1-6400th and 1-6665th of an inch; the following sized globules are also very frequent, viz. 1-7108th, 1-5333rd, and 1-6000th. Blood from a wound in the neck.

41. Cashmere Goat, old male, (*Capra Hircus*, var.). Average-sized corpuscles 1-7200th to 1-5858th. The corpuscles very irregular in size; all the following sizes were very frequent, viz. 1-8000th, 1-6400th, 1-6000th; there were also a few disks 1-5333rd of an inch in diameter. Blood from the vena cava and from the renal artery.

* As announced in the Phil. Mag. for December 1839, and in the Dublin Medical Press, No. 47. Some observations on the blood and pus of these animals were read at the Royal Med. Chir. Society, Nov. 26, 1839.

42. Common sheep, adult, (*Ovis Aries*). Corpuscles remarkably irregular in size, most commonly 1-6000th and 1-5333rd of an inch. All the following measurements taken from corpuscles which were abundant: 1-8000th, 1-5142nd, 1-4800th, 1-4221st. Blood from right ventricle, and from vena portæ.

43. Bearded Sheep, an old male, (*Ovis tragelaphus*). The most common diameter of corpuscles 1-6000th and 1-5333rd of an inch; extreme sizes, 1-6400th and 1-4000th, the former much more frequent than the latter.

44. Philantomb Antelope, adult male, (*Antilope Philantomba*). Average-sized corpuscles 1-6000th and 1-5333rd of an inch. A large number 1-4365th. Extreme diameters, 1-6400th and 1-4200th. Blood from a prick of the ear; disks examined dry, in their own serum, and diluted with urine. A very few distinct oval or spear-shaped corpuscles were seen, both in the wet and dry specimens.

45. Gazelle Antelope (*Antilope Dorcas*). Most common sizes 1-5333rd and 1-4800th. Extreme diameters 1-6000th and 1-4000th. Thickness of the edges of disks 1-16,000th of an inch. Blood from a vein of the ear.

46. Calf at the seventh month of utero-gestation, (*Bos Taurus*). Most of the corpuscles 1-4000th and 1-3428th of an inch. Extreme diameters 1-6000th and 1-3200th. Blood from jugular, umbilical, and portal veins.

a. Cow, adult, giving milk. Size very variable, from 1-5333rd to 1-3555th. Average size, 1-4268th and 1-4000th of an inch diameter. Blood from jugular vein.

b. Brahmin Cow, adult, (*Bos Taurus*, var.). 1-4572nd and 1-4800th very common sizes. Extreme diameters 1-6000th and 1-3557th.

47. Fallow Deer, full-grown buck, (*Cervus Dama*). Average-sized corpuscles 1-4572nd of an inch. Extreme measurements 1-6000th and 1-3200th. Several corpuscles 1-5333rd. Blood from jugular vein.

a. A buck Fawn, four or five days old. Average-sized corpuscles 1-4365th of an inch. Extreme diameters 1-4000th and 1-5333rd. Blood from jugular vein.

48. Mexican Deer, adult female, (*Cervus Mexicanus*). Circular corpuscles generally 1-6000th of an inch in diameter; oblong corpuscles, most frequently from 1-3200th to 1-2400th long, and from 1-12,000th to 1-8000th broad, at their gibbous part.

The blood of this animal contains a large quantity of lunated or crescentic corpuscles, besides many of the ordinary circular form. The singular lunated corpuscles are remark-

ably distinct and characteristic, and I believe unlike any hitherto described in the animal kingdom. They are generally acutely pointed at the ends and gibbous in the middle; sometimes they are not curved, and then, to use a botanical term, they present a lanceolate figure. A more particular description of them, with drawings, is in preparation.

49. Napu Musk Deer, adult female, (*Tragulus Javanicus**). Average size of disks 1-12,000th of an inch. Extreme diameters 1-15,000th and 1-9600th. Blood examined from a prick of the ear, also from a small vein of the leg.

Hence the blood disks of this beautiful little animal are smaller than those, hitherto described, of any other mammal. They were remarkably distinct, and very well seen in dry specimens, also diluted with serum or urine.

50. Pig, about half-grown, (*Sus Scrofa*). Common diameters 1-4000th and 1-3728th. Extreme sizes 1-5333rd and 1-3200th, and the corpuscles generally very variable. The blood examined was arterial and venous mixed.

51. Elephant, male, apparently an adult, (*Elephas Indicus*). Most frequent diameters of disks 1-3000th, 1-2666th, and 1-2462nd of an inch. Other sizes, 1-3329th, 1-2910th, 1-2823rd, and 1-2286th. Blood obtained from a vein in the ear.

Consequently the blood corpuscles of this animal are larger than any hitherto described in the mammalia, being considerably larger than in man. But a reference to the comparative magnitude of the blood disks of the horse and the bat, and some species of *Mus*, will show how little relation there is between the size of the animal and the diameter of the red particles. In the mouse for instance they are larger than in the horse.

52. Rhinoceros, full-grown male, (*Rhinoceros Indicus*). Common size of disks 1-4000th and 1-2554th. Many were observed with diameters of 1-4572nd and 1-3200th of an inch, with of course every intermediate gradation of size. Blood from a prick in the nose.

53. Common seal (*Phoca vitulina*), 1-3200th the most frequent diameter. Extreme sizes 1-3554th and 1-2666th. The disks very regular in size. Blood from prick of caudal fin.

The particles are slightly larger than in the adult human subject, and approach in appearance to corpuscles of the monkey's blood.

* See Phil. Mag. for Dec. 1839, and Dublin Medical Press, No. 47. *Moschus Javanicus* of Pallas. Since my observation of the singular blood-disks of this animal was published, Professor Owen has given an account of them in another species (*M. pygmaeus*), in which they seem to be equally small, as he remarks he had anticipated.

54. Common Otter, full-grown, (*Lutra vulgaris*). Most common sizes 1-3600th and 1-3200th. Extreme diameter, 1-4572nd and 1-2910th of an inch. Blood from right ventricle, and from renal artery and vein.

55. Kangaroo, adult male, (*Macropus Bennettii*). Most common diameters 1-3600th and 1-3432nd of an inch; and the following sizes were rather frequent: 1-4000th, 1-3200th, and 1-3000th. Blood from a prick at the end of the tail.

56. Flying Opossum, adult female, (*Petaurus Sciurus*). Average-sized disks 1-3600th. Extreme diameters 1-4800th and 1-3000th of an inch. Blood from a prick in the nose.

57. Ursine Opossum, adult male, (*Dasyurus Ursinus*). Most frequent sizes 1-3428th and 1-3600th. Extreme diameters 1-4365th and 1-3000th. Thickness of the edges of the disks from 1-10,000th to 1-12,000th of an inch. Blood from a vein in the ear.

58. Another species, (*Dasyurus viverrinus*). 1-4000th a very common diameter of the disks; 1-4800th and 1-3554th were also the sizes of several of the disks. Blood from the left ventricle.

59. The Perameles, (*Perameles lagotis*). Common sizes 1-4572nd and 1-4000th of an inch. Several disks were seen of the following diameters: 1-4800th, 1-3428th, and 1-3200th. Blood from a prick in the ear.

Thus the blood disks of these five Australasian animals agree in form and size with the red particles most common in other mammals.

Since the foregoing paper was printed I have examined the blood of some other mammiferous animals. I subjoin the notes in the order they happen to have been made. All the observations will soon be systematically arranged, so as to exhibit in some measure the relation between the blood corpuscles and the organization of the animal. The blood particles of the different species of the genus *Cervus*, Antelope, and the congeners of the Napu Musk Deer, appear to me to be especially deserving of further observations; and it is very probable that any physiologist who may prosecute the inquiry will be rewarded with some interesting results.

60. White-fronted Lemur, adult male, (*Lemur albifrons*). Size of corpuscles very variable, from 1-4800th to 1-3000th. The most frequent diameter 1-3600th. Blood from both ventricles, from the splenic and portal veins, and the disks apparently identical in all.

61. Sambur Deer, adult buck, (*Cervus hippelaphus*). Dry; size very variable, 1-4000th and 1-3600th most common.

34 Mr. G. Gulliver's *Observations on the Blood Corpuscles.*

Extreme diameters, rather frequent, 1-4572nd and 1-3200th. Blood from a prick of the nose.

62. Moose Deer or Elk, adult hind, (*Cervus Alces*). Average-sized corpuscles, 1-4000th and 1-3764th. Extreme diameters 1-5333rd and 1-3200th. Blood dried, from a prick of the nose.

63. Barbary Deer, adult hind, (*Cervus Barbarus*). Dry; most frequent diameters 1-4800th. Extreme diameters 1-5333rd and 1-4365th. Blood from the upper lip.

64. Wapiti Deer, full-grown hind, (*Cervus Wapiti*). Dry; 1-4363rd most common size. Extreme diameters 1-5333rd and 1-3554th. Blood from the upper lip.

Carefully compared with the blood of the preceding, and the corpuscles generally pretty regular in size, and certainly larger than in the Barbary Deer.

65. Hog Deer, adult male, (*Cervus porcinus*, albino var.). Common size of corpuscles 1-5333rd and 1-6000th. Many oblong lunated corpuscles, corresponding in size and form to those observed in the blood of the Mexican deer.

66. Gnu, adult female, (*Antilope Gnu*). Dry; corpuscles varying much in size, 1-4800th most common. Extreme diameters 1-6000th and 1-4000th. Blood from prick of upper lip.

67. Indian Antelope, male about two-thirds grown, (*Antilope cervicapra*). Dry; most frequent diameter 1-4800th and 1-5000th. In their own serum the corpuscles were very irregular in size and shape, and certainly smaller than when dried. The extreme sizes of the dried disks 1-6000th and 1-4000th; in their serum or diluted with urine 1-8000th and 1-5000th.

68. Giraffe, adult male, (*Camelopardalis Giraffa*). Most frequent size of corpuscles 1-4572nd. Extreme diameters 1-5333rd and 1-4000th. Blood from a prick of the lip.

69. American Buffalo, full-grown female, (*Bos Bison*). 1-4000th of an inch, a very common diameter of the corpuscles. Extreme sizes 1-4572nd and 1-3554th. Blood examined dry, from the nose.

70. Weasel-headed Armadillo, adult male, (*Dasypus sexcinctus*). Most frequent size of corpuscles 1-3429th, and the following common, viz. 1-3692nd, 1-3552nd, 1-3368th, 1-3330th. Extreme diameters 1-4000th and 1-3000th. Blood from a prick of the ear.

71. Lesser American Flying Squirrel, adult, (*Pteromys volucella*). Dry; 1-3600th the most common size of disks; and 1-4000th not uncommon. Extreme diameters 1-4800th and 1-3428th. Blood from a prick of the ear.

72. Palm Squirrel, adult, (*Sciurus palmarum*). Dry; most commonly 1-3692nd, extreme sizes 1-4800th and 1-3000th. In their own serum or in urine they seemed contracted, the corpuscles often cup-shaped, bent, or shrunk, ranging from 1-4800th to 1-4000th. Blood from a prick of the tail.

73. Furnier's Capromys, adult, (*Capromys Furnieri*). Dry; common diameters 1-3530th and 1-3429th. Extreme sizes 1-4000th and 1-3000th. Blood from a prick of the ear.

74. Bandicoot Rat, adult, (*Mus giganteus*). Corpuscles in their own serum very irregular in size, most frequently 1-4000th of an inch, and many from 1-5333rd to 1-3200th. There was evidently considerable shrinking of the corpuscles while under examination, as observed in several trials. In dried specimens the disks were generally from 1-4000th to 1-3600th. Extreme sizes 1-4800th and 1-3200th. Blood from a prick of the tail.

75. Hoary Marmot or Whistler, an old animal, (*Arctomys pruinosa*). Dry; most commonly 1-3600th. Extreme sizes 1-4000th and 1-3000th. Blood from a prick of the upper lip.

Regent's Park Barracks, Nov. 22, 1839.

V. *On the Employment of Carbon in Voltaic Combinations.*
By MR. JOHN THOMAS COOPER, *Lecturer on Chemistry,*
&c. &c.

To Richard Phillips, Esq., F.R.S., &c.

DEAR SIR,

IT occurred to me on reflecting upon the use of the platinum as employed by Mr. Grove in the construction of his very energetic voltaic combination*, as it was only to conduct the electricity from the decomposing nitric acid, that any cheaper substance which conducted electricity, and upon which nitric acid had no action, might be employed with equal advantage, and probably supply the place of the more expensive material. With these views I was induced to make trial of *charcoal*, and the other forms of carbon, viz. plumbago, and a peculiar kind of carbon which is frequently met with as an incrustation in the retorts in which coal is decomposed for the purpose of gas lighting; and was gratified on making the experiments in finding my anticipations fully realized. In order to show the comparative value of each of the substances, I here subjoin the results of some of the experiments made with acids of the same strength, and with amalgamated zinc cylinders, each presenting to the action of the dilute sulphuric

[* See Lond. & Edinb. Phil. Mag., Oct. 1839, vol. xv. p. 287.]

36 *On the Employment of Carbon in Voltaic Combinations.*

acid as nearly an equal surface as it was possible to obtain *. The figures represent the volumes of gas obtained by the voltmeter in equal times.

Thick platinum foil soldered
to the zinc.

3·4
3·4
3·4
3·6
3·6
3·5
3·75
3·5
3·5

9)31·65

Mean 3·517 cub. inch.

Well-burned charcoal clamped
to the zinc.

2·8
3·0
3·1
3·2
3·25
3·25
3·25
3·4
3·25

9)28·50

Mean 3·17 cub. inch.

Plumbago clamped
to the zinc.

3·2
3·2
3·3
3·2
3·2
3·6
3·6
3·7
3·5

9)30·5

Mean 3·4 cub. inch.

Hard carbon from gas retorts
clamped to the zinc.

2·6
2·8
3·3
3·4
3·4
3·45
3·5
3·6
3·4

9)29·45

Mean 3·27 cub. inch.

From the above results it will be evident that the platinum appears to possess a trifling advantage over the other substances; this, however, I am inclined to ascribe to the more perfect contact of the platinum with the zinc by soldering than to its superior qualities as a conductor of electricity; and when the difference of expense is taken into consideration in the construction of large and extensive combinations, the application of the above-mentioned substances must be regarded as of great importance to the chemist, and to those who may have occasion to employ such combinations as electro-motors, seeing that either with the charcoal, plumbago, or hard carbon,

* The battery consisted of four zinc cylinders, each having $6\frac{1}{2}$ inches surface, and the pipeclay cups, 1 inch in diameter and $1\frac{1}{4}$ inch long, which gave the above quantity of gas in two minutes.

the purity of the nitric acid is a matter of indifference, strong commercial acid answering every purpose.

I remain, dear Sir, very truly yours,

JOHN THOMAS COOPER.

82, Blackfriars Road, London, Dec. 10, 1839.

P.S. I have found within these few days that some kinds of common coke, such as is very brilliant, close-grained, and has a columnar fracture, is equally good with the other varieties of carbon, and admits of being selected of almost any variety of form and size.

J. T. C.

VI. *On Derivation of Coexistence*: Part I. *Being the Theory of simultaneous simple homogeneous Equations*. By J. J. SYLVESTER, F.R.S. & R.A.S., Professor of Natural Philosophy in University College, London*.

Art. (1.) WE shall have constant occasion in this paper to denote different quantities by the same letter affected with different subscribed numerical indices.

Such a letter is to be termed a "Base."

Every character consisting of a base and an inferior index I call an argument of the base, viz. the first, second, or n th argument, according as 1, 2, or in general (n), be the number subscribed.

Art. (2.) I use the symbol PD to denote the product of the differences of the quantities to which it is prefixed (each being to be subtracted *from* each that follows); thus

PD ($a\ b\ c$) indicates $(b-a)(c-a)(c-b)$.

PD ($o\ a\ b\ c$) indicates $a\ b\ c\ (b-a)(c-a)(c-b)$.

PD ($o\ a\ b\ c\ \dots\ l$) indicates $a\ b\ c\ \dots\ l \times$ PD ($a\ b\ c\ \dots\ l$).

Art. (3.) For want of a better symbol I use the Greek letter ζ to denote that the product of factors to which it is prefixed is to be effected after a certain symbolical mannner. This I shall distinguish as the zeta-ic product.

The symbol ζ will never be prefixed except to factors, each of which is made up of one or more terms, consisting solely of linear arguments of different bases, i.e. characters bearing indices below but none above.

I am thereby enabled to give this short rule for zeta-ic multiplication: "Imagine all the inferior indices to become superior, so that each argument is transformed into a *power* of its base; multiply according to the rules of ordinary algebra; after the multiplication has been *done fully out* depress all the

* Communicated by the Author. Part I. appeared in L. & E. Phil. Mag. Dec. 1839, vol. xv. p. 428.

indices into their original position; the result is the zeta-ic product*."

Thus for example $\zeta(a_r \cdot b_s)$ is the same as simply $a_r \cdot b_s$, but $\zeta(a_r \cdot a_s)$ represents not $a_r \cdot a_s$ but a_{r+s} .

So in like manner

$$\zeta((a_h - b_k)(a_l - b_m))$$

$$= a_{h+l} - a_h \cdot b_m - b_k \cdot a_l + b_{m+k}$$

$$\zeta((a_l - b_l)(a_l - c_l)(b_l - c_l))$$

$$= \text{the depressed product of } (a-b)(a-c)(b-c)$$

$$= \text{the depressed value of } a^2(b'-c') + b^2(c'-a') + c^2(a'-b')$$

$$\text{i.e.} = a_2 \cdot b_1 - a_2 \cdot c_1 + b_2 \cdot c_1 - b_2 \cdot a_1 + c_2 \cdot a_1 - c_2 \cdot b_1.$$

Art. (4.) We shall have occasion in this part to combine the two symbols ζ , PD: thus we shall use

$$\zeta \text{ PD } (a_l b_l) \text{ to denote } \zeta(b_l - a_l)$$

$$\zeta \text{ PD } (a_l b_l c_l) \text{ to denote } \zeta((b_l - a_l)(c_l - a_l)(c_l - b_l)).$$

Art. (5.) For the sake of elegance of diction I shall in future sometimes omit to insert the inferior index when it is unity; but the reader must always bear in mind that it is to be *understood* though not expressed.

I shall thus be able to speak of the zeta-ic product of such and such bases mentioned by name.

Art. (6.) We are not yet come to the limit of the powers of our notation. The zeta-ic product of the sum of arguments will consist of the sum of products of arguments, each argument being (as I have defined) made up of a base and an inferior index. Now we may imagine each index of every term of the zeta-ic product *after it is fully expanded* to be increased or diminished by unity, or each at the same time to be increased or diminished by 2, or each in general to be increased or diminished by r . I shall denote this alteration by affixing an (r) with the positive or negative sign to the ζ . Thus

$$\zeta(a_l - b_l)(a_l - c_l) \text{ being equal to } a_2 - a_1 \cdot c_1 + b_1 \cdot c_1 - b_1 \cdot a_1$$

$$\zeta_{+1}(a_l - b_l)(a_l - c_l) \text{ is equal to } a_3 - a_2 \cdot c_2 + b_2 \cdot c_2 - b_2 \cdot a_2$$

$$\zeta_{-1}(a_l - b_l)(a_l - c_l) \text{ is equal to } a_1 - a_0 \cdot c_0 + b_0 \cdot c_0 - b_0 \cdot a_0.$$

In like manner $\zeta \text{ PD } (a b c)$ indicating

$$b_2 \cdot a_1 - b_2 \cdot c_1 + c_2 \cdot b_1 - c_2 \cdot a_1 + a_2 \cdot c_1 - a_2 \cdot b_1$$

* It is scarcely necessary to add that an analogous interpretation may be extended to any zeta-ic function whatever. Thus

$$\zeta(a_1 + b_1)^2 = a_2 + 2a_1 b_1 + b_2$$

$$\zeta \cos(a_1) = 1 - \frac{a_2}{1 \cdot 2} + \frac{a_4}{1 \cdot 2 \cdot 3 \cdot 4}, \&c.$$

$\zeta_{\pm r}$ PD ($a b c$) indicates

$$b_{2\pm r} \cdot a_{1\pm r} - b_{2\pm r} \cdot c_{1\pm r} + c_{2\pm r} \cdot b_{2\pm r} - c_{2\pm r} \cdot c_{1\pm r} \\ + a_{2\pm r} \cdot c_{1\pm r} - a_{2\pm r} \cdot b_{1\pm r}.$$

I shall in general denote $\zeta_{\pm r}$ PD ($a b c \dots l$) *actually expanded* as the zeta-ic product of $a, b, c, \dots l$ in its r th phase.

Art. (7.) *General Properties of Zeta-ic Products of Differences.*

If there be made one interchange in the order of the bases to which ζ is prefixed, the zeta-ic product, in whatever phase it be taken, remains unaltered in magnitude, but changes its sign.

Art. (8.) If in any *phase* of a zeta-ic product two of the bases be made to coincide, the expansion vanishes.

Art. (9.) Let \int_1 be used, agreeably to the ordinary notation, to denote the sum of the quantities to which it is prefixed, \int_2 to denote the sum of the binary products, \int_3 of the ternary ones, and so on.

Thus let $\int_1 (a, b, c)$ or $\int_1 (a b c)$ indicate $a + b + c$

and $\int_2 (a, b, c)$ or $\int_2 (a b c)$ indicate $a, b + a, c + b, c$

and $\int_3 (a, b, c)$ or $\int_3 (a b c)$ indicate a, b, c

we shall be able now to state the following remarkable proposition connecting the several phases of certain the same zeta-ic products.

Art. (8.) Let $a, b, c, \dots l$, denote any number of independent bases, say $(n-1)$; but let the arguments of each base be periodic, and the number of terms in each period the same for every base, namely (n) , so that

$$\begin{array}{ll} a_r = a_{r+n} = a_{r-n} & a_n = a_o = a_{-n} \\ b_r = b_{r+n} = b_{r-n} & b_n = b_o = b_{-n} \\ c_r = c_{r+n} = c_{r-n} & c_n = c_o = c_{-n} \\ \cdot & \cdot \\ l_r = l_{r+n} = l_{r-n} & l_n = l_o = l_{-n} \end{array}$$

r being any number whatever. Then

$$\begin{array}{l} \zeta_{-1} \text{ PD } (o a b c \dots l) = \zeta (\int_1 (a b c \dots l) \cdot \zeta \text{ PD } (o a b c \dots l)) \\ \zeta_{-2} \text{ PD } (o a b c \dots l) = \zeta (\int_2 (a b c \dots l) \cdot \zeta \text{ PD } (o a b c \dots l)) \\ \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \\ \zeta_{-r} \text{ PD } (o a b c \dots l) = \zeta (\int_r (a b c \dots l) \cdot \zeta \text{ PD } (o a b c \dots l)) \end{array}$$

This proposition admits of a great generalization*, but we have now all that is requisite for enabling us to arrive at a proposition exhibiting under one *coup d'œil* every combination and every effect of every combination that can possibly be made with any number of coexisting equations of the first degree, containing any number of *repeated*, or to use the ordinary language of analysts, (variable or) unknown quantities.

Art. (9.) For the sake of symmetry I make every equation homogeneous; so that to eliminate n repeated terms, no more than n equations will be required.

In like manner the problem of determining n quantities from n equations will be here represented by the case in which we have to determine the *ratios* of $(n + 1)$ quantities from n equations.

Art. (9.) *Statement of the Equations of Coexistence.*

Let there be any number of bases ($a \ b \ c \ \dots \ l$), and as many repeated terms ($x \ y \ z \ \dots \ t$), and let the number of equations be any whatever, say (n). The system may be represented by the *type* equation

$$a_r \cdot x + b_r \cdot y + c_r \cdot z + \dots + l_r \cdot t = 0.$$

In which r can take up all integer values from $-\infty$ to $+\infty$. The specific number of equations given will be represented by making the arguments of each base *periodic*, so that

$$a_r = a_{\mu n + r} \quad b_r = b_{\mu n + r} \quad c_r = c_{\mu n + r} \quad \dots \quad l_r = l_{\mu n + r},$$

μ being any integer whatever.

Art. (10.) *Combination of the given Equations.—*
Leading Theorem.

Take $f, g, \dots k$ as the *arbitrary* bases of new and absolutely independent but periodic arguments, having the same index of periodicity (n) as $a \ b \ c \ \dots \ l$, and being in number $(n - 1)$, i. e. one fewer than there are units in that index.

The number of *differing* arbitrary constants thus *manufactured* is $n \cdot (n - 1)$.

Let $Ax + By + Cz + \dots + Lt = 0$ be the general *prime* derivative from the given equations, then we may make

$$A = \zeta \text{ PD } (o \ a \ f \ g \ \dots \ k)$$

$$B = \zeta \text{ PD } (o \ b \ f \ g \ \dots \ k)$$

$$C = \zeta \text{ PD } (o \ c \ f \ g \ \dots \ k)$$

$$\dots \dots \dots$$

$$L = \zeta \text{ PD } (o \ l \ f \ g \ \dots \ k).$$

Art. (11.) Cor. (1.) *Inferences from the Leading Theorem.*

Let the number of equations, or, which is the same thing,

* See the Postscript to this paper for *one* specimen.

the index of periodicity (n), be the same as the number of repeated terms ($x y z \dots t$), then one relation exists between the coefficients: this is found by making the $(n - 1)$ new bases coincide with $(n - 1)$ out of the old bases. We get accordingly, as the result of elimination,

$$\zeta \text{ PD } (o a b c \dots l) = 0.$$

Art. (11.) Cor. (2.) Let the number of equations be one more than that of the given bases, there will then be two equations of condition. These are represented by preserving one new arbitrary base, as λ . The result of elimination being in this case

$$\zeta \text{ PD } (o a b c \dots l \lambda) = 0.$$

Ex. The result of eliminating between

$$a_1 \cdot x + b_1 \cdot y = 0$$

$$a_2 \cdot x + b_2 \cdot y = 0$$

$$a_3 \cdot x + b_3 \cdot y = 0$$

is $\zeta \text{ PD } (o a b \lambda) = 0$

$$\text{i. e. } \lambda_3 \cdot b_2 a_1 - \lambda_3 \cdot b_1 a_2 + \lambda_1 \cdot b_3 a_2 - \lambda_1 \cdot b_2 a_3 + \lambda_2 \cdot b_1 a_3 \\ - \lambda_2 \cdot b_3 a_1 = 0,$$

from which we infer, seeing that $\lambda_3 \lambda_2 \lambda_1$ are independent,

$$b_2 \cdot a_1 - b_1 \cdot a_2 = 0$$

$$b_3 \cdot a_2 - b_2 \cdot a_3 = 0$$

$$b_1 \cdot a_3 - b_3 \cdot a_1 = 0,$$

any *two* of which imply the third.

In like manner, in general, if the number of equations exceed in any manner the number of bases or repeated terms, the rule is to introduce so many *new* and *arbitrary* bases as together with the old bases shall make up the number of equations, and then equate the zeta-ic product of the differences of zero, the old bases and the new bases, to nothing.

Art. (12.) Cor. (3.) Let the number of equations be *one* fewer than the number (n) of bases or repeated terms; the number of introduced bases in the general theorem is here $(n - 2)$. Make these $(n - 2)$ bases equal severally to the bases which in the type equation are affixed to $z, u \dots t$, then

$$C = 0$$

$$D = 0$$

$$\dots\dots\dots$$

$$\dots\dots\dots$$

$$L = 0,$$

and we have left simply

$$\zeta \text{ PD } (o a c d \dots k l) x + \zeta \text{ PD } (o b c d \dots k l) y = 0.$$

In like manner we may make to vanish all but A and C , and thus get

$$\zeta \text{ PD } (o a b d \dots k l) x + \zeta \text{ PD } (o c b d \dots k l) z = 0,$$

author to play no secondary part in the development of some of the most curious and interesting points of analysis.

Let there be $(n-1)$ bases $a, b, c \dots l$, and let the arguments of each be "recurrents of the n th order*," that is to say let

$$a_i = \phi \left(\cos \cdot \frac{2 \pi i}{n} \right) \quad b_i = \psi \left(\cos \cdot \frac{2 \pi i}{n} \right) \quad c_i = \chi \left(\cos \cdot \frac{2 \pi i}{n} \right) \\ \dots \dots l_i = \omega \left(\cos \cdot \frac{2 \pi i}{n} \right).$$

Let R_t denote that any symmetrical function of the r th degree is to be taken of the quantities in a parenthesis which come after it, and let \mathfrak{S} indicate any function whatever. Then the zeta-ic product

$$\zeta \left(\zeta R_t (a \ b \ c \ \dots \ l) \times \zeta_{\epsilon} \mathfrak{S} \text{PD} (o \ a \ b \ c \ \dots \ l) \right)$$

is equal to the product of the *number*

$$R_t \left(\left(\cos \frac{2 \pi}{n} + \sqrt{-1} \cdot \sin \frac{2 \pi}{n} \right) \left(\cos \frac{4 \pi}{n} + \sqrt{-1} \cdot \sin \frac{4 \pi}{n} \right) \right. \\ \left(\cos \frac{6 \pi}{n} + \sqrt{-1} \cdot \sin \frac{6 \pi}{n} \right) \dots \dots \\ \left. \cos \left(\frac{(2n-1) \pi}{n} + \sqrt{-1} \cdot \sin \frac{2(n-1) \pi}{n} \right) \right)$$

multiplied by the zeta-ic phase

$$\zeta_{\epsilon-t} \mathfrak{S} \text{PD} (o \ a \ b \ c \ \dots \ l) !!$$

* I am indebted for this term to Professor De Morgan, whose pupil I may boast to have been. I have the sanction also of his authority, and that of another profound analyst, my colleague Mr. Graves, for the use of the arbitrary terms zeta-ic, zeta-ically. I take this opportunity of retracting the symbol S P D used in my last paper, the letter S having no meaning except for English readers. I substitute for it Q D P, where Q represents the Latin word Quadratus. On some future occasion I shall enlarge upon a new method of notation, whereby the language of analysis may be rendered much more expressive, depending essentially upon the use of similar figures inserted within one another, and containing numbers or letters, according as quantities or operations are to be denoted. This system to be carried out would require special but very simple printing types to be founded for the purpose.

In the next part of this paper an easy and *symmetrical* mode will be given of representing any polynomial either in its developable or expanded form.

VII. *On the Combinations of Carbon with Silicon and Iron and other metals, forming the different species of Cast Iron, Steel, and Malleable Iron.* By Dr. C. SCHAFHAEUTL, of Munich.

[Continued from vol. xv. p. 428.]

THE chemical ingredients of the iron are easily to be ascertained; but the information thus obtained is of no value in investigating the real chemical nature of iron, and can only be used as a preliminary method which must guide or verify further proceedings.

I will only here remark, that by separating silica in the usual way by means of alkalis, there is considerable difficulty in rendering silica insoluble in water when combined with a great quantity of oxide of iron, it requiring a great length of time to drive away the last traces of water and acid from the evaporated solution; and by a quick evaporation, if the residuum is not heated almost to a red heat, the silica either dissolves in a great measure again, or goes through the filter after a most tedious filtering process. It is always a laborious process, causing much loss to separate it afterwards, when the solution contains manganese. On the contrary, if the heat is increased to so high a degree, the separated silica retains a great quantity of oxide of iron, from which it is the most readily freed by treating it, after ignition, with hot chlorohydric acid; I mention this purposely, as Baron Thenard cautions us to use, for evaporating the solution containing the silica, only a moderate heat, in order that the chlorides may not be decomposed.

In ascertaining the exact quantity of carbon, of more than half-a-dozen given directions one only is of real value, which was likewise first used by Berzelius, viz. the burning the iron in a current of oxygen gas, or mixing it with chlorate of potash and chromate of lead, and igniting it in a glass tube after the well-known practice used in the analysis of organic bodies. All other methods give, instead of pure carbon, carbon combined with silicon, or carbon combined with hydrogen, azote, and silicon.

To ascertain the quantity of azote, where the metal is in large quantity, I use Dumas's method, viz. the combustion of iron in a vacuum; but where the quantities are small, I employ the following means. I put into a tube of German glass, from four to five lines wide, and about twelve inches long, shut at one end, a few grains of the body from which I intend to separate the azote, and afterwards about six times its weight of a mixture of caustic potash and caustic barytes; the open

end of the tube is then drawn out over the lamp into a short capillary tube, the tube is afterwards bent about five inches from its closed end into a siphon-like figure, the capillary tube is immersed in a test glass until it nearly touches the bottom, holding not more diluted chlorohydric acid than sufficient to fill the drawn-out leg of the syphon to the height of three inches, whilst the other end is gradually heated over a spirit-lamp. The azote forming with hydrogen ammonia is driven out in a very small stream into the acid, and when the decomposition of the iron is completed, the heat is gradually diminished, and the acid ascends in the same ratio into the end of the siphon. As soon as all the acid of the test tube is absorbed, air is streaming through the capillary tube and the acid into the siphon, thus establishing the equilibrium between the interior and exterior of the tube. The heat is afterwards again raised, till the absorbed acid is driven out again into the test glass, and after this operation all the ammonia will be found to be absorbed by the acid. The larger end of the siphon is then cut off and well washed with distilled water, and the quantity of ammonia ascertained by means of a solution of chloride of platinum, added in excess, the liquor of course being evaporated nearly to dryness in the water-bath, and treated and washed with absolute alcohol. The compound of muriate of ammonia and chloride of platinum, insoluble in alcohol, remains, from which the quantity of ammonia is calculated very easily.

The action of acids upon iron and the products afterwards are highly curious and interesting. The products formed by the action of acids upon iron depend first, on the chemical constitution of the iron itself; secondly, on the greater or less division of the mass; thirdly, on the chemical constitution of the acids; fourthly, on the greater or less concentration of the acids; fifthly, on the temperature; and sixthly, on the presence or exclusion of the atmosphere.

The acid whose action I have most studied is the hydrochloric acid, and in some respects nitric acid. I therefore confine my observations to those two only. The specific gravity of the hydrochloric acid used by me was 1.169 to 1.17. The iron was in fragments from the size of a nut to that of a lentil. For the sake of illustrating the action of hydrochloric acid upon iron, I select first a fragment of a steel bar of the highest conversion of the Dannemora iron. A drop of already melted steel adhered with a very broad base on one side. The fragment about the size of a walnut in a temperature of 48 F., was treated in an open wine-glass with chlorohydric acid. The action of the acid was very rapid, but the outside of the bar

fragment was more rapidly attacked than the bright crystalline fracture of the steel. That most rapidly attacked was the before-mentioned adhering drop of steel; a light yellow powder was first separated, which partly rose to the surface and partly descended to the bottom; next a gray precipitate was generated, which increased until the acid ceased to act upon the steel. All parts of the steel fragment which showed an incipient melting and silver-white colour, retained this colour during the action of the acid; the other parts of the outside of the fragments showed a blackened granular formation, traversed by white shining needles.

After having taken the fragment from the acid, I filed the surface of the before-mentioned steel drop off, and laid open a grayish surface traversed by a venous network, white and shining, and much more difficult to be attacked by the file than the lower gray and softer parts betwixt the network. On pouring fresh acid again over it, the filed surface was soon covered with a deep black velvety crust, traversed by a silver white elevated network, somewhat similar to the veins in some marbles. The interstices between the network, after removing this black crust, were found to be filled up with small crystalline needles, and all the white places, showing an incipient fusion, which the day before had remained white, disappeared partially on the second day, leaving only a few spots like the remainder of a skin, which covered a similar composition, formed of an aggregation of needles. The faces of the cubical crystals seemed also to consist only of a silvery skin, which being corroded and eaten through by the acid, showed underneath a granulated texture. A gray precipitate was found on the bottom of the glass, distinctly intermixed with a little yellowish granulated powder.

A separated cubical piece of the same bar, treated in the same manner in a wine-glass, was not very rapidly attacked by the acid, but milky streams were observed ascending to the surface. Fresh acid was then substituted. The liquid soon became milky; a copious white precipitate fell, which two hours afterwards assumed a whitish flocky form, and only a few small black flocks seemed to be mixed with copious yellowish flocks.

In order to obtain some further information respecting this powder, which I considered as silica, I used fragments of the size of a pea of a highly-cubical crystallized steel bar, made from burnt English iron, prepared after my method; secondly, a granulated fragment of the exterior of the same bar; thirdly, a small crystallized sponge-like piece from the before-mentioned burnt bar, before its conversion into steel. I put over

those specimens, in three different glasses, hydrochloric acid. No. 1. was attacked very violently: the liquid became milky, and a yellow precipitate with a very few traces of black flocks remained. During a space of nine weeks the solution had imbibed so much water from the surrounding atmosphere, that the glasses were nearly overflowing. The acid was then poured off and distilled water poured over the precipitate; the precipitate was now white and flocky, and the most copious of the three samples.

No. 2. was not so violently attacked, and did not become milky: first, fine black scales became separated, adhering to the sides of the glass very regularly; afterwards, the interstices between these black scales or spots became filled up with the before-mentioned yellow powder. The whitish powder appeared very fine, never granulated or flocky, and adhered to the sides of the glass almost entirely. The whitish precipitate of Nos. 1. and 3. fell to the bottom of the glass.

No. 3. was the most rapidly attacked by the acid, and became milky, the same as No. 1. The liquid was, after a lapse of nine weeks, rather turbid and showed the smallest quantity of black scales swimming in the liquid. A few hours after the acid had been poured over it, it became turbid and very yellow, which showed that it had combined the quickest with the oxygen of the atmosphere.

The day after the acid had been poured on No. 2. the surface only began to acquire a yellow colour. In No. 3. the black flocks seemed entirely to have disappeared, and the yellowish powder ascended in part to the surface, and the remainder fell to the bottom. The difference therefore between the granulated exterior of the same bar, and the interior crystallization, seems to consist in this, that the exterior produces less white powder in a very divided state and more carbonaceous scales, or that the granulated parts, be they inside or outside of the bar, contain less of the white powder and more black scales than the crystallized; I therefore infer, that the carbon is here substituted for silicon; and as No. 3. contained more silicon than No. 2., it seems either that the silicon of No. 2. was partially driven away during its preparation by the carbon, or rather that the silicon remained in combination with carbon.

A fragment of Bombay wootz was also very slowly attacked by the acids, and deposited on the sides of the glass the before-mentioned white powder.

21.71 grains of No. 3. left white residuum dried at the heat of boiling water = 0.3437 grains.

A part of a coloured blister of Dannemora steel deposited likewise a *white powder*, and was like the before-mentioned samples.

I took afterwards two pieces of iron from the puddling furnace, one just before the granulated mass began to become coherent, and the other just before the iron was ready for balling or for being made into balls, in order to bring them under the forge hammer. These two samples were treated with acids as before. The first sample, weighing 29.89 grains, separated a gray powder, and left a black skeleton of the iron, which very easily crumbled into powder. The weight of the gray powder was 0.421, and the black skeleton weighed 0.250.

The second sample, which had been forged and consisted of a mixture of grains and fibres, weighing 36.625 grains, left grayish-green powder first, and on the acid being changed for the third time, it deposited white powder and left also a black skeleton, which oxidized very rapidly and was soon converted into a brown powder. Concentrated nitric acid would not act at all on this remainder, but on the addition of hydrochloric acid the powder became of a bright red. The remaining powders together weighed 0.8125, and the black skeleton 0.4531.

We learn from this that the *black remainder* increases with the progressive advancement from cast iron to that of malleable, and that the gray powder belongs to the gray granulation, and the white remainder to the finished fibrous iron.

The relation of the black remainder to the gray appears to be in all cases the same, only that the quantity increases progressively towards the finishing of the puddling.

We gather further from these facts, that the yellow powder appears only on using concentrated hydrochloric acid, and with that species of iron which nearly approaches steel and wrought iron.

At first I considered the yellow powder to consist of *silica* with a small portion of iron, and with the view of ascertaining with certainty the correctness of my opinion, I collected the yellow residuum of the three before-mentioned specimens on a very small filter, and separated as much powder as I possibly could.

The powder retained, after being dried, its light yellow colour, and was neither attacked by acids, except by very *concentrated hydrofluoric* acid; it became white by heating it on a platinum foil, and melted with soda on charcoal before the blowpipe, under effervescence, into a transparent globule of a ruby colour, which retained its transparency after cooling; a circumstance which seems to indicate the presence of silica as well as *sulphur*; the presence of the latter was also ascertained by the smell.

0·41 of this yellow powder was melted in a platinum crucible with carbonate of soda, and the effervescence was very vivid. I separated silica to the amount of 0·115. The solution, from which the silica was separated, saturated with carbonate of ammonia, left alumina with a little silica, which amounted to 0·107; no trace of iron could be detected.

Fourteen grains of filings of English iron, made after my peculiar method for steel, were then dissolved in a test glass with five drams of hydrochloric acid. The iron dissolved rapidly; a dark gray oily skinny scum collected on the surface, and the glass was filled to an inch in height with a whitish gray saline granulated precipitate of protochloride of iron, which, on having removed the acid and substituted distilled water, disappeared completely. The solution, stirred and quickly poured into another vessel, left *gray heavy scales* of the form of the filings on the bottom.

These gray-white scales, well washed, were very slowly attacked by diluted hydrochloric acid; concentrated, it acted rather more powerfully, and the liquid became yellow and milky fourteen hours after the action of the acid had ceased. A few black spots remained on the bottom; a grayish viscid mass floated on the surface, like sulphur, separated by aqua-regia from sulphurets; the sides of the tube were covered by a layer of yellowish-white substance, which I could sublime over a lamp and volatilize. The fumes escaping had, in a degree, the scent of French brandy and fennel oil.

The black viscid mass, slowly ignited on a platinum foil, emitted first fumes of volatilized sulphur; then some sulphurous acid appeared; and lastly, the mass began suddenly to glow like tinder, and burnt without further assistance, leaving a residue of darkish brown powder, which, boiled with muriatic acid, assumed a vivid red colour, was scarcely attacked by aqua-regia, and only dissolved completely after being boiled again in hydrochloric acid. This scaly remainder consisted therefore of

Iron and	} a great quantity ;
Sulphur	
Carbon and	} very little ;
Hydrogen	

probably a sulphuret of carbon mixed with iron, or a carburet of iron mixed with sulphur.

The water, quickly separated from the heavy scales as before-mentioned, became, after a little time, clear, a white precipitate having fallen to the bottom. This precipitate, heated first on a platinum foil, gave out sulphur, then a small quantity of sulphurous acid, when the mass began to glow and a *light white powder of silica* remained.

This light precipitate consisted therefore of

Silica,	} without iron,
Carbon,	
Hydrogen,	
Sulphur,	

and was, in all probability, a carburet of silicon mixed with a carburet of sulphur.

The same quantity of steel filings from a razor made by Rogers of Sheffield, gave nearly the same result, with the exception, that the sediment was darker, and there remained more of the white sulphurous precipitate, covering not only the sides, but, in a thick layer, the bottom also of the test tube.

Iron which I made at Axat, in the Oriental Pyrenees, from a mixture of spathose iron and iron glance, differed only from the foregoing specimens in this, that the sediment appeared of a lighter colour and the sides of the tube remained uncovered by the *white precipitate*.

The first decanted liquid of this iron treated with sulphuretted hydrogen let fall a reddish yellow precipitate, soluble in ammonia, leaving white sulphur.

Further, I treated powdered cast iron from the Maesteg iron-works, near Neath in South Wales, with caustic potash in a test tube. After the evolution of ammonia had ceased, the mass was dissolved in distilled water, and a part of the iron was found remaining. Half of this powder was dissolved in hydrochloric acid; hydrogen escaped, and a whitish gray flocky precipitate remained. The other half of the remaining iron being again melted with caustic potash, and ammonia was again disengaged, leaving also a black granulated mass of iron, which was rather tough under the hammer, and afterwards being likewise dissolved in hydrochloric acid, a perfectly *white precipitate* remained. The acid was removed and distilled water substituted, until no trace of hydrochloric acid was to be found. I considered it to consist of sulphur and silica; but on heating it carefully over a spirit lamp, a very volatile vapour was disengaged, having some distant resemblance to the smell of cyanic acid gas. A little white powder remained on the bottom, and the sides of the test tube were covered by a dew of a perfectly transparent liquid. Having poured a few drops of distilled water into the tube, and afterwards a drop of solution of nitrate of silver, a white precipitate fell which retained its colour on exposure to the rays of the sun; a proof that it was not chloride of silver.

[To be continued.]

VIII. *A few Observations on the Authenticity of the Passage in the Treatise of Boetius de Geometria on Numerical Contractions.* By J. O. HALLIWELL, Esq., F.R.S., F.S.A., F.R.A.S. &c.*

I VENTURE to add the following remarks to what I have previously written†, in consequence of a postscript by M. Libri, who wishes for more substantial evidence on the point of authenticity than has hitherto been produced. Such crude arguments as I am able to furnish must not by any means be considered as the result of a strict or lengthened inquiry, but are rather intended to show that the question is well worthy of much greater attention than has yet been paid to it.

M. Libri was the first who conjectured that this passage might be an interpolation, and with some justice; for it may be reasonably asked, why does not Boetius allude to the new system in his treatise on arithmetic. Again, from the abacal system employed in that treatise, I should be inclined to think that the articulate and composite divisions were certainly not introduced until after that period.

In the library of Trinity College, Cambridge, there is a very beautiful quarto MS. of the *eleventh century* on vellum, containing the treatise of Boetius *de Geometria* plentifully illustrated with neat diagrams. This manuscript, one of the most ancient in this country, does *not* contain the disputed passage. M. Libri has challenged me to produce such an evidence; *accipe si vis*—the manuscript may be found under the press mark R. xv. 14, and is briefly mentioned at p. 99 of Bernard's Catalogue, No. 491.

It would, perhaps, be scarcely fair to make its extreme singularity an argument against its authenticity, but we may be permitted to argue on the probability or improbability of such a passage being written at so early a period. Is it likely, that at a time when the arithmetic of the West was a mere geometrical adaptation of quantity, when Boetius himself recognised the Roman abacal system, and when it required no inconsiderable depth of foresight to appreciate the advantage of an arbitrary system of digital characters, that Boetius would have inserted so extended an innovation in a treatise written expressly on a science that has no immediate relation with that into which the improvement was introduced? At any rate it is a fair subject for discussion, whether we could reasonably suppose any writer fully acquainted with the merits

* Communicated by the Author.

† See p. 447 of the present volume.

of both systems and giving superior attention to the worst, if written previously; and if otherwise, why is no reference made to the treatise conducted with the common Roman notation? These considerations are almost sufficient of themselves to throw a doubt on its genuineness, although we must wait for the discovery of more direct evidence before any definite conclusion is broached. I know not what the forthcoming work of M. Chasles may contain, but every one interested in these matters must be well aware how greatly we are indebted to that able writer, and will readily leave the discussion of this point in his hands.

IX. *On the coloured Films produced by Electro-chemical Agency and by Heat.* By R. WARINGTON, ESQ.*

THE following paper is intended as an answer to a memoir of the late Prof. Nobili on this subject, the translation of which appeared in the first volume of the Scientific Memoirs, p. 94, under the title of a “Memoir on colours in general, and particularly on a new chromatic scale deduced from metallochromy for scientific and practical purposes.” As this paper contains a great detail of matter on the physical characters and properties of colours and coloured films, and as it is only with respect to the chemical part of the subject that I propose treating it, it will be necessary to extract such sentences as refer more immediately to the questions at issue. I should not have ventured to attack a memoir coming from so high an authority as Professor Nobili, but from its having as yet elicited very little notice, and the views taken being so startling to the chemist and so perfectly original, I am induced to offer some practical experimental remarks on the subject. The memoir naturally resolves itself into two distinct subjects; namely, the nature of the coloured films produced through the medium of electro-chemical agency; and secondly, those produced by means of heat; these it is my intention to treat of separately and distinctly in their order.

Professor Nobili dates his *discovery* of the electro-chemical appearances as far back as the year 1826, and the account of them was laid before the French Institute in November 1828, and by their advice the distinctive appellation of metallochromy was adopted. The method by which these appearances are produced is thus described, p. 94: “A plate of platina is laid horizontally at the bottom of a vessel made of glass or china. A platina point is vertically suspended over

* Communicated by the Author.

this in such a manner that the distance between the point and the plate may be about half a line. A solution of acetate of lead is next poured into the vessel, so as not only to cover the plate, but to rise two or three lines higher than the point. The plate and the point are now brought into communication, the former with the positive and the latter with the negative pole of an electric pile. At the moment when the voltaic circuit is closed, a series of rings similar to those formed at the centre of the Newtonian lenses is to be seen on the surface of the plate precisely under the point." The same process has been adopted to produce the films, the investigation of which will be detailed in an after part of this paper. The voltaic powers consisted of two small batteries, constructed on the principles of Professor Daniell's, and the effects were produced in small platina dishes or capsules for the convenience of investigation. We must now pass on to another part of Professor Nobili's memoir, in order to collect the facts and their explanation. At p. 106, we find, "The appearances which constitute the chromatic scale are due to the electro-negative elements of the solution (oxygen and acid), which being transferred by the current to the positive pole, are then spread out into thin transparent films, from which all the colours of the scale arise. The electro-positive elements (such as hydrogen and the metallic bases) are, on the contrary, transferred to the negative pole, and then deposited in layers which never produce the colours of thin plates." Again, at page 109, Professor Nobili adds, "I will not undertake to say by what species of affinity or force it is that these elements" (oxygen and acid) "are attracted to each other and spread out into thin films on the platina. It is certain however, that they attach themselves to the platina without oxidizing it in the slightest degree. We must not suppose that this happens because platina is a metal difficult to be oxidized. Iron and steel belong to the class of metals most easily oxidized, and yet it is well known that they will bear to be covered with electro-negative layers without becoming rusted. My electro-chemical experiments, multiplied and varied in a thousand ways, leave no room for reasonable doubt on this point: they show that oxygen and certain acids may adhere to the surface of metals without producing the slightest chemical change in them. This is a novel state for oxygen and the acids, and is distinguished from their ordinary combination by the three following peculiarities: 1st, the metal retains, beneath the deposited layer, its natural brilliancy; 2ndly, this layer produces the phænomenon of the

coloured rings in all its beauty; 3rdly, instead of oxidizing the metal, these electro-negative elements contribute to secure it against oxidation in every part to which they are applied. A fact so unprecedented is interesting to chemistry, and is entitled to particular attention, as tending to enrich the science by the introduction of new ideas."

In a foot-note the following theory is offered, "that the electro-negative elements disposed in thin layers on the surface of the metals are at too great a distance from the molecules of these substances to enter into combination with them." These extracts, although of no great length, will yet put the reader in possession clearly of Professor Nobili's views of the subject, and enable him to appreciate the bearing of the following experiments.

The "unprecedented fact" then "so interesting to chemistry" is entitled to particular attention, as it tends to enrich the science, and introduce new ideas. What is this fact? it is the production of coloured films at the positive pole (the poles being of platina) of an electric circuit, the connecting medium being a solution of acetate of lead. These coloured films, Professor Nobili states, consist of oxygen and acid precipitated, as it were, upon the surface of platina, iron, or steel, without producing any oxidation of these metals, and the correctness of which statement the multiplicity of the experiments, he states, places beyond a reasonable doubt.

Some of these splendidly coloured films, produced as stated before, were well washed with distilled water and acted upon by dilute nitric acid, which did not remove them, but appeared to dilute, as it seemed, the intensity and brilliancy of the colours: the solution was decanted and evaporated to dryness to remove all excess of acid, and on being tested gave indications of lead. Muriatic acid instantly destroyed all trace of the coloured films with evolution of chlorine gas and the formation of a curdy or crystalline chloride of lead. Another experiment was made by heating the films to redness and then acting upon them by dilute nitric or acetic acid, which dissolved them rapidly and yielded the usual indications of the presence of that metal. It would appear from these experiments that these electro-chemical appearances are therefore nothing more than extremely thin films of peroxide of lead or of red lead spread out on the surface of the platina, but from their great tenuity it is likely that they may modify in a degree the action of the acids used. I may be allowed to remark here, that if the voltaic arrangement is too powerful, or the solution of acetate of lead too strong, when

the poles are brought into too close proximity, brown oxide of lead is thrown down in a powdery form. Now it is a well-known fact, which may be found in almost every work on chemistry, that peroxide of lead is precipitated from solutions of that metal by means of a voltaic current, platina forming the poles *. It is much to be regretted that Professor Nobili did not during the long period of his experiments put this matter to the test of chemical investigation.

The circumstance of lead in a high stage of oxidation being deposited in aggregated films at the positive pole of a voltaic circuit, will bear out an observation of Prof. Schœnbein, made at the last meeting of the British Association at Birmingham, that he believed that peroxide of hydrogen was sometimes formed during the decomposition of water, for that the volume of hydrogen gas eliminated was often more than double that of the oxygen. It will also afford a reason why iron should assume the inert or inactive state in voltaic combinations, particularly in an experiment exhibited by Prof. Schœnbein, of heating one extremity of an iron wire so that it was covered with a coating of oxide, in the form of a coloured film, and thus becoming inactive, while the end which had not been submitted to a similar operation was attacked with energy.

We now pass on to the consideration of the second division of our subject, the nature of the coloured films produced on the surface of metals by means of heat. This is a part of the subject more familiar to all parties, whether scientific or not, for every one must have noticed the beautiful colours produced on the polished steel bars of an ordinary stove, or on the surface of a copper tea-kettle. In the arts its applications are numerous and highly interesting, and the processes of tempering steel, the formation of the beautiful bronzing powders, of rose copper, and a variety of others, are too well known already to need anything more than a slight allusion to them.

Before entering on the analysis of the subject I must put you in possession, as briefly as possible, of Professor Nobili's views of the question. At p. 108, he states, "As to these colours, the most generally received opinion is, that they depend on a principle of oxidation. Berzelius calls the metallic

* I was not aware until the morning of the day on which this paper was read before the Mathematical Society, that this matter had been investigated by Prof. Schœnbein, and published by him in the *Bibliothèque Universelle* for May, 1837, and that this investigation had succeeded a hint to that effect from Professor Faraday, in volume x. of the *Lond. and Edin. Philosophical Mag.*: for this information I am indebted to Mr. Brayley.

layer which is thus coloured a *suboxide*." In a foot-note at p. 110, we read, "Berzelius was more sensible of the difficulty," (of accounting for these coloured films) "perhaps, than any one else: but would not an open avowal have been better than the attempt to evade it by the adoption of the term *suboxide*, which is quite as vague and undefined as the *principle of oxidation*, for which it was offered as a substitute?" This is rather strong language to be used against such an authority as Berzelius, every one of whose statements is backed by investigation, and brought by Prof. Nobili, who does not adduce a single experiment in proof of his statements. But to proceed: "I have always entertained some doubts as to the correctness of this explanation; because each degree of oxidation has a colour peculiar to itself, and in no way related to that variety of tints of which we speak. I was also struck by the well-known practice of giving steel a violet colour in order to secure it from rust." "Were this tint, as it is presumed to be, the effect of oxidation, it would, in my opinion, instead of preventing, serve only to accelerate oxidation." "But this is not all; the superficial colours of which we speak are changeable, and belong evidently to the same class as those produced by thin plates. Now the pure metals are, from their opacity, incapable of this species of coloration. Can they acquire that capacity in their first degree of oxidation by becoming suddenly transparent in consequence of their union with a small quantity of oxygen? The hypothesis far exceeds the bounds of probability, and the phenomenon requires to be otherwise explained." Again, "Confining myself in this place to the colours produced on metals by the action of fire, I do not hesitate to say, that I think their origin now placed beyond the reach of doubt. It may be safely laid down as a general proposition, that the oxygen of the atmosphere produces them, not, as is supposed, by oxidizing the surface of the metal, but by becoming fixed in the form of a thin plate or film, similar to those of the electrochemical appearances." Professor Nobili then gives a detail of the production of the colours by heat, and observes, that as long as the colours are seen there is no oxidation, but that when the metal loses its brilliancy and lustre it has become oxidized, and that if removed from the heating medium before this effect takes place, the oxygen will cover the metal and adhere as a varnish.

In Berzelius's System of Chemistry, under the heads of the various metals and the action of heat upon them, he states distinctly, that copper, lead, and tin form protoxides, and that palladium forms a suboxide; so that out of four metals which

produce coloured films by heat, only one is stated to form a suboxide. The next point is, that each degree of oxidation produces colours peculiar to itself; this is offered as a reason for doubting the principle of oxidation. No person will imagine that each tint on the surface of the metal is owing to a different oxide, but that it arises, as experiment proves, from the films of the oxide being of various thicknesses, and that the surface of the bright metal below reflects the light through the film, as is the case with the oxide of lead produced by the electro-chemical agency; and that the brighter the metallic surface the more vivid and brilliant are the colours produced. With respect to the preserving power of a film of oxide on the surface of the metal, it is well known; and the principle on which it acts is the exclusion of moisture and carbonic acid from contact with that surface*. The next point is the capacity of metals to become transparent when united with oxygen, and here I think the evidence is satisfactory indeed. Copper, tin, antimony, titanium, zinc, and iron all occur in the mineral form transparent or translucent on the edges, and this in mass: of course, if reduced to thin films, such as are produced by heat, they would be perfectly so.

The same beautiful display of colours on the surface of metals may be produced by iodine: the manner in which I have formed these is simply to place a very small piece of iodine on the centre of a disc of metal,—copper or silver succeeds extremely well,—and cover the surface with a flat dish, so as to prevent the vapour of iodine from being dissipated: the circles of coloured films are very distinct and brilliant. The application of the thinnest or pale yellow of these films on silver by M. Daguerre, to the production of his pictures, is now well known. Sulphur and the application of heat produce similar effects. The process of oxidation by heat, and the disappearance of the metallic lustre, which Prof. Nobili attempts to use as an argument, are accounted for very simply thus; that the application of the heat being continued, the film of oxide gradually becomes thicker, until at last it becomes opaque; but in the course of my experiments I have had these coloured films peel off from the surface of the metal with all their transparency and all their beautiful tints unimpaired. The same effect has taken place, by long-continued electro-chemical action, with the oxide of lead.

* M. Zumstein, in August 1820, fixed a polished iron cross on the summit of Monte Rosa, in the Alps; and on again visiting it in 1821, it was found neither rusted nor corroded, but had merely acquired a tarnish of the colour of bronze, owing to the extreme dryness of the air at that elevation.

While arguing at page 110 against the probability of the principle of oxidation, Prof. Nobili observes that the violet tint on steel, "does not, perhaps, consist solely of oxygen, as it does when the metals are pure. Steel is a carburet of iron, and the oxygen of the air on being precipitated on this compound, becoming combined with the carbon, in some manner or other might form the layer in question." Now this is a most extraordinary statement. After arguing so long and so energetically against all thoughts of the oxygen uniting with the metallic surface, it is here hinted that, in some manner or other, the oxygen will take the carbon away from the iron, with which it was in combination, unite with it, and form, not a film of oxygen, but of carbon and that element; I suppose carbonic oxide or carbonic acid; and yet the theory advanced as an explanation for these phænomena, supposes that the electro-negative elements disposed in thin layers are at too great a distance from the molecules of the metal to enter into combination with them. These statements surely are contradictory. Allusion is made to the iridescent surface of the specular iron ore, and they are successfully imitated by electro-chemical means as given in the first part of this paper, but it must be borne in mind, that by this means lead in a high stage of oxidation is produced, and not a film or films of oxygen alone.

To sum up the whole of this subject in a few words, then, it appears: 1st, that the appearances called electro-chemical are not films of oxygen and acid, but lead in a high stage of oxidation thrown down on the surface of the metal by means of a voltaic combination acting through a medium formed by a solution of acetate of lead; 2ndly, that these colours owe their varied tints to the varying thickness of the precipitated film, and that the light is reflected through them from the polished metallic surface below; 3rdly, that the colours produced on the surface of metals by the application of heat are owing to the formation of thin films of oxide of the metal in consequence of exposure to the air during the process; that this does not involve the necessity of any one oxide being always formed, as this must vary according to the affinity of the metal used for oxygen, under the influence of a raised temperature; 4thly, that the opacity of the metal is not in the slightest degree an argument against the transparency of the oxide, as we have both in nature and art numerous cases which place this question beyond a doubt; 5thly, that we can produce analogous appearances by substituting other elements for oxygen, such as iodine, chlorine, bromine, sulphur, phosphorus, carbon, &c.

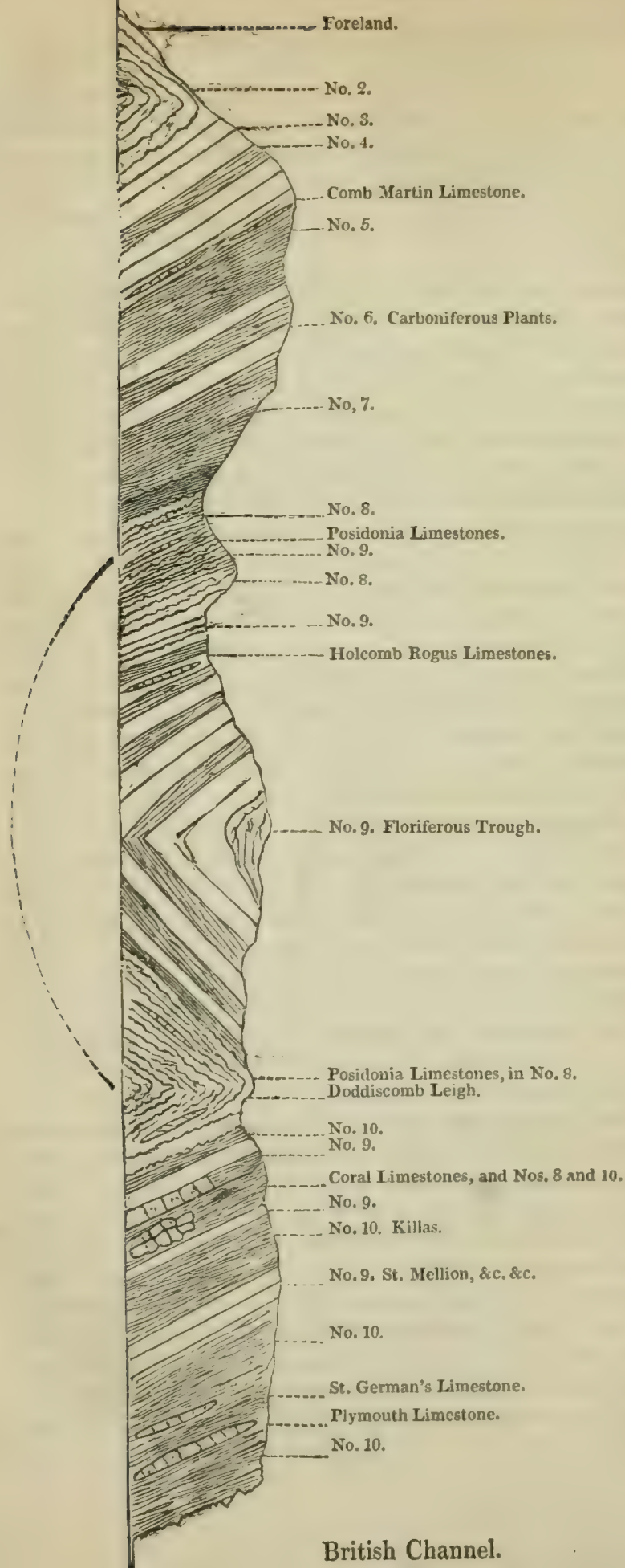
X. *On the Geology of Devon and Cornwall, with reference to a paper read before the Geological Society on December 4th, 1839. By the Rev. D. WILLIAMS, F.G.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

AS I do not consider the substance and spirit of my paper on Devon and Cornwall, which was read at the meeting of the Geological Society on the 4th inst. is fairly reported in the *Athenæum* of the 7th, I request you will favour me with an opportunity of righting myself with your readers, and of reporting progress since my communication which was published in your last October Journal (vol. xv. p. 293). I feel assured that I am not intentionally misrepresented in the *Athenæum*; the abstract however imputes to me (as I hastily read it before I left London) that I hold mineral characters to be everything and organic evidences nothing, in determining the relative ages of strata. Now it was very distinctly read by the Secretary, Mr. Darwin, that I did not consider the law proposed by Dr. Smith to be of any value in classifying the rocks of the earth in remote localities, if it did not suppose a final and universal extinction of genera and species; and in as much as some plants and animals would probably be enabled by the Creator to survive mutations which would be death to others, I considered that a classification of the older rocks should be regulated by some per-centage test, such as Mr. Lyell had applied to the tertiaries, rather than by a more restricted rule. I quote from memory, not being able to refer to either the *Athenæum* or to my paper. I stated that I unequivocally believed in the extinction of genera and species, severally at distant epochs, and therefore did *not* believe that the *Posidonia* and *Goniatite*, which I discovered in some trashy lentiform limestones in Devonshire, were specially created for the mountain limestone alone, when I knew it could be proved to demonstration, that those *Posidonia* limestones of Devon, and all their associated rocks, not only bore no lithological resemblance to any of the mineral types of any portion of the great English coal-field, but that they underlaid the coral limestones of South Devon, and the whole of the slates of Cornwall. I exhibited sections evidencing the actual supraposition of the Cornish killas on the floriferous series, No. 9, and the Coddon grit, No. 8, and I pointed out the localities. In all fairness then it remains for gentlemen to disprove those facts, instead of requiring me to show what I believe to be an impossibility, viz. the identity of the plants I found in the great floriferous outlier on the south and

Bristol Channel.



British Channel.

south-west of Callington, with those which had been procured from the fine culm shales near Bideford. I believe it to be an impossibility, on account of the same coarseness of the matrix in the great outlier, as exists everywhere, that I have seen, in the rocks of the floriferous series generally, where I have never met with clearly defined specimens, except in the finer culm-shales just mentioned. All I am prepared to prove then is, that about St. Mellion and Pillaton, between Callington and Plymouth, *plants in the same imperfect condition are found in precisely the same slates and shales, which are parted by thick beds of the same sandstone, and in intimate association with that singularly characterized and unique formation the Coddon Hill grit; there is the same triple association of the same rocks, and in the same order of succession, that we witness in the base line of the floriferous series along the north and south borders of the trough, and where on earth could they come from if not from the same sources which supplied the constituents of the same rocks elsewhere in the same county?* The only deviation on the S. and S.W. of Callington from the normal types of the floriferous series elsewhere, is frequent intercalations in it of undoubted killas, and beds of a composite or neutral character, constituted of moieties of killas and Coddon Hill grit, or of killas and floriferous, round the confines of the outlier seen not only in repeated alternation, but in other instances, their wedge-shaped extremities interlocking into each other like the teeth of a rat-gin. Here, as elsewhere along the confines of these two vast formations, Nos. 9 and 10, whether we advance towards the floriferous area on the one hand, or towards the killas on the other, we distinctly observe the one becoming thinner and evanescent as the other augments into unity and fulness.

Nature has manifestly conducted her operations of deposition and elevation in this region on a vast scale, and if her works be not regarded in their just proportions, we never shall arrive at the truth: thus as we explore the confines of Nos. 9 and 10, we are startled almost at the vastness of the ties and adjustments by which they are indissolubly united, till we reflect that they are only in a ratio to the magnitude and dimensions of their respective masses; that it is only the same transition and alternation on a larger scale, that we observe throughout Exmoor between the several members from No. 2 to No. 9, on a smaller; for while I hesitate as to the diameter of No. 9; No. 10, I repeat, is upwards of eight miles, measured according to Professor Playfair. Thus again, if we take a *coup d'œil* view of this country from one channel

to the other, we have nothing more than one great wave (of probably some far extended undulation) consisting simply of two convex arcs inclosing a central trough, apparently the result of the same system of forces acting on a vast floor of matter, successively and regularly accumulated; either an overlying mass, or a fractured section of an original continuation of the Cambrian and Silurian deposits; for if we compare the precipitous and vertical cliffs of Nos. 2, 3, 4 and 5, of Exmoor (in echelon arrangement beetling over the deep tide way of the Bristol Channel) with the carboniferous limestone and secondary rocks of the opposite coast of Wales, we have all the evidences of an enormous fault.

But what are the results if we compare the positive testimony afforded by gradation, alternation, succession, and conformable supraposition, with that afforded by organic remains? The Petherwin fossils near Launceston, and those of the coral limestones of South Devon, I include without doubt or hesitation in a lower horizon, or subdivision of the floriferous series, No. 9, *above* the Posidonia limestones; so that if we suppose the ratio of extinction of vegetable species not to have been governed by the causes which effected that of marine zoophyta and testacea, the exceptions afforded by the Posidonia limestones are but as dust in the balance of organic evidence; and in this respect alone, geologists will involve themselves in inextricable difficulties and contradictions, if they reject the maximum and rely on the minimum amount of organic evidence. I repeat the fact, that the lower floriferous, and Coddon Hill grit series are overlaid in the south by the slates of Cornwall, which comprise in their ascending terms, first, the St. Germans, and lastly, the Plymouth limestones; so that it appears to me, that the great consecutive series from No. 2 to 10, evidences a transition of organic type, in progress, as it were, from the *grauwacké towards* the carboniferous limestone; that the latter, or its coal-field, is not represented here at all, but that the coarse slaty and red arenaceous beds which overlies the Plymouth limestones, extending thence to Rame Head on the south, probably do appertain to the early period of the Old Red Sandstone proper.

The relations of the floriferous, No. 9, to the coral limestones, and killas, No. 10, are explained with the greatest clearness and simplicity at and around Chudleigh; to aid my brief description I refer your readers to the accurate and faithful sections of Mr. De la Beche. (See Report, Plate IV. fig. 7 and 8). But why that able observer should assign a different position to the many other groups of coral limestone

of South Devon, is to me unaccountable, because they are all manifestly of the same age and order with the Chudleigh limestones, viewed stratigraphically, zoologically, or mineralogically, and are seen under precisely the same associations, for I do not remember a single exception to the fact of the floriferous, Coddon grit, and killas, being either interstratified with, or underlying and overlying them: so that nothing can be predicated of the Chudleigh, that may not equally be affirmed of the coral limestones everywhere, if the parallel ridges immediately north and south of Chudleigh be the same floriferous, No. 9, and to doubt it is to doubt the plainest evidence of the senses: the controversy is at an end, for we trace them here continuously, from the culm-field; and I ask any fair and indifferent geologist merely to compare the rocks on the right bank of the Teign near Chudleigh-bridge, with those on the left, where a cutting for the road to Newton affords a good section of the west extremity of the Ugbrook ridge; and if he does not pronounce their perfect identity, the same dull olive-coloured sandstones parted by the same black shales, I will no longer advocate what I know to be the truth, and allow error to maintain the ascendant. No one doubts that chalk is chalk, or oolite oolite, or lias lias, elsewhere! The Chudleigh reef of limestones, which is lost under Haldon to the eastward, and cuts out near the Teign to the west, is a great alternation between the two floriferous ridges just mentioned, the three sequents dipping together at about the same angle to the south; while a careful examination of the coral limestones shows them to be based here immediately upon thick *black culmy* beds, and higher up to be parted by Cornish killas, beds of Coddon Hill grit and volcanic ash, with *plants*. The Creator has been so explicit here, that his works cannot be misinterpreted, if the laws recorded on these tables of stone be read without prejudice or control.

On discovering in the month of May last, at Doddiscomb Leigh, five miles north of Chudleigh, the *Posidonia* limestones (as everywhere else), included in the Coddon Hill grits, and together constituting the anticlinal axis of the south border of the trough to the east of Dartmoor (thus manifestly underlying the Chudleigh series, a fact confirmed by good cuttings and natural sections along the west bank of the Teign) the scales fell from my eyes—every difficulty and apparent anomaly vanished as if by magic, and the structure of the entire region, from one channel to the other, was presented to my mind's eye in all the grandeur of its simplicity; from Plymouth to Linton it was a simple series of successive

emergence. A thousand embarrassing facts on the west of Dartmoor, and elsewhere, at once were reconciled, and the rocks appeared before me, like a cloud of witnesses, to testify that the floriferous series was overlaid by the Cornish killas, and requiring me, as it were, to restore each to his rightful throne.

My long section exhibited at the Geological Society did not perhaps show the south anticlinal axis sufficiently prominent or distinct, for I see by my maps that the Coddon Hill grit, commonly dipping *south*, occupies nearly two miles of country from north to south; and that at and about Doddiscomb Leigh, it is in the same parallel with the great line of fracture on the W. of Dartmoor which ranges by Launceston to Bos-Castle; and that this line continued through Dartmoor will intersect it at Amicomb Hill, between Fur-Tor and Yes-Tor, which Mr. MacLauchlan has determined to be the highest points of elevation in the West of England. Any omission in my section, however, I request may be imputed to my deficiency in tact in getting up a section, and not to any imperfection in the evidences afforded by the country; but in reply to the objection urged by Mr. Murchison, I may state, that the Posidonia limestones being only insulated patches in the Coddon Hill grit, and therefore part and parcel of the mineralogical axis, are quite as likely, in the southern fall, to dip away from the trough, as to dip into it; my section, however, gives the floriferous rocks as the most prominent of the anticlinal, which I still think is very near the truth, and may be explained by supposing them to arch over the subordinate Coddon Hill grits; or still better by the fact, that in the N. of Devon the Coddon grits are divided into an upper and lower, by great wedge-shaped masses of the floriferous rising into prominent hills, viz. south of Barnstaple, and north of Bampton, so that the lower range of these grits may not be exposed here at all.

All I have to say further is, that since the day I picked up the master-key at Chudleigh and Doddiscomb Leigh, I have not met with the least difficulty or embarrassment; nor do I anticipate anything hereafter but additional confirmation, from the conviction that nature will not be, as she has not been, permitted to deny herself; and I again earnestly invite Prof. Sedgwick and Mr. Murchison, or Mr. Weaver, to review the county; for after all, there are no gentlemen to whom I would sooner refer this question than to themselves.

I have the honour to remain, Gentlemen, &c.

Bleadon, near Cross, Dec. 16th, 1839.

D. WILLIAMS.

XI. *Notices respecting New Books.*

A Treatise on Crystallography, by W. H. Miller, F.R.S., Professor of Mineralogy in the University of Cambridge.

IT is well known to those who have attended to the subject of crystallography, that the classification of crystalline forms introduced by Haüy, as well as the methods of expressing these forms and of calculating their relations, have been in a great measure superseded by other modes of treating the subject. The distinction of *systems* of crystallization proposed by Weiss and Mohs has been generally accepted among crystallographers; and the angles made by faces, edges, and the like, instead of being deduced by means of geometrical reasoning, have been obtained by the more general methods of spherical trigonometry and analytical geometry. Weiss may be looked upon as the person who first introduced this more general mode of calculation; and he has been followed by G. Rose, Kupffer, Köhler, Naumann, Neumann, Grassmann, Hessel, and others, in Germany, and by Mr. Levy, Mr. Brooke, and Mr. Whewell in England. Along with these different modes of calculation, different modes of notation for crystalline forms have also been employed. The old unsystematic notation of Haüy has been modified and retained by Mr. Brooke, Mr. Levy, and Mr. Phillips in England, and by several French writers; while Professors Mohs and Weiss have each introduced his own method of notation.

The notation of Mohs, in itself most superfluously cumbrous and unsymmetrical, has been made the basis of a much improved system of notation by Prof. Naumann; and the symbols of Weiss, which are really the most general, and depend upon a single convention, have been somewhat simplified by Mr. Whewell. In this state of the subject, we turn with great interest to the treatise of Professor Miller, who from his familiarity with analysis is able to give to crystallographical methods all the generality and simplicity of the best school of mathematics, and who likewise, from his acquaintance with special minerals, is not likely to fail in furnishing abundant exemplifications of his general methods. We may state in Professor Miller's own words the selection which he has made of a notation and mode of calculation. "The crystallographic notation adopted in the following treatise is taken, with a few unimportant alterations, from Professor Whewell's memoir on a general method of calculating the angles of crystals, printed in the *Transactions of the Royal Society* for 1825. The method of indicating the positions of the faces of a crystal by the points in which the radii drawn perpendicular to the faces meet the surface of a sphere, was invented by Prof. Neumann of Königsberg (*Beiträge zur Krystallonomie*), and afterwards, together with the notation, re-invented independently by Grassmann (*Zur Krystallonomie und geometrischen Combinationslehre*). The use of this method led to the substitution of spherical trigonometry for the processes of solid and analytical geometry in deducing expressions for determining the positions of the faces of

Phil. Mag. S. 3. Vol. 16. No. 100. Jan. 1840. F

crystals and the angles they make with each other. The expressions which in this treatise have thus been obtained are remarkable for their symmetry and simplicity, and are all adapted to logarithmic computation. They are, it is believed, for the most part new."

It is not possible for us to give any detailed account of Professor Miller's methods; but we may observe that each face of a crystal is determined by the portions cut off from the three *axes* of the crystal, and is expressed by a *symbol* ($h\ k\ l$) in which the indices depend upon these portions. When several contiguous faces have their intersections parallel, they may be considered as belonging to a *zone*; and this zone is indicated by its symbol $[u\ v\ w]$. Some of the simplest methods of determining the law of derivation of a proposed face consist in referring it to such zones. Thus if we have, given, the symbols of two zones $[p\ q\ r]$, $[u\ v\ w]$, the symbol of the face common to the two zones ($h\ k\ l$) is known from the equations

$$h = v r - w q, \quad k = w p - u r, \quad l = u q - v p.$$

The mathematical student of crystallography cannot fail to be delighted with the completeness and symmetry with which, in Professor Miller's Treatise, formulæ of this kind are obtained for each *system* of crystallization; and with the great and instructive variety of examples to which they are applied. It will be found, by attention to these examples, that the methods employed in this work are not only, analytically speaking, the most general and symmetrical, but also practically the most compendious and convenient for the determination of the laws of derivation of any proposed form.

We cannot help thinking, however, which we do with regret, that this book, mathematically so admirable, will be a sealed book to a large body of crystallographical students. It is written with a rigorous brevity, worthy of the ancient mathematicians; a quality, in itself, doubtless, a beauty, but one of those stern beauties which repel, rather than attract, common beholders. There is not a single phrase in which the author shows any sympathy for those of his readers who have not been disciplined in mathematics to the extent which his investigations require. And this requisite discipline is, in truth, not slight; for though the knowledge which he presupposes in his reader does not go beyond the doctrines of spherical trigonometry, no one can follow Prof. Miller's reasonings with any facility, except his habits of mathematical generalization and abstraction have been well matured. And even the method of indicating the positions of the faces of crystals by their *poles* upon a *sphere of projection*, although it much simplifies the calculation, obscures our conception of the relations of the crystalline form; at least it does this when we are first called upon to employ the method, and before it is become familiar to us. This, however, is an inconvenience attendant upon most simplifications of physical problems, and we speak to regret rather than to blame it in the present instance. But perhaps we might venture to express a wish that the practical rules for the calculation of crystals had been separated from the mathematical investigations which contain the demonstra-

tions of the rules. If Prof. Miller would detach from these mathematical reasonings a body of *Precepts*, such as might enable the crystallographer, from proper measurements, to determine the symbols of the faces of any proposed crystal, putting these precepts in such a form that they should be capable of being employed by any person conversant with the processes and symbols of algebra, he would render his work useful to a much wider circle of calculators than will, we fear, now venture to apply his processes. Nor would this addition to the work at all mar the great mathematical beauty of matter and style which all competent judges will allow it to possess.

We cannot conclude this brief notice without expressing our satisfaction, that this subject of crystallography, after being put in so many forms for the last half century, has here assumed a shape which, so far as mathematical simplicity and symmetry go, leaves us nothing to desire, and therefore no reason for further change.

Transactions of the Cambridge Philosophical Society, vol. vii. Part I.

These Transactions have a claim upon our notice, not only from their general scientific importance, and especially from their containing the labours of several of our best British mathematicians, but also, in the part now before us, from the peculiar and comprehensive interest of the problems to which most of the memoirs refer. There are *three* great problems which at the present time have a manifest right to the best exertions which mathematicians of the highest class can employ in favour of physical science; and this claim has recently been allowed and acted upon to a great extent by the most eminent mathematicians of England, France, Germany, and Italy. These three problems are, the motion of waves in water; the undulations of the fluid or fluids by which light, heat, and similar phænomena are supposed to be produced; and the molecular forces by which the particles of bodies are held together; and of these, the two latter ones are closely connected with each other. All the papers in the present Part of the Cambridge Transactions, with one exception (the elegant memoir of Mr. Holditch on Rolling Curves), refer to these three problems; which have also been the subject of several investigations in previous parts of the Transactions.

On the subject of the first of these three problems, the motion of waves in water, we have a memoir by Mr. Green, who had in a previous memoir solved the problem of the motion of waves in a canal of small *variable* depth and width; a case which we believe had not been before successfully attacked by any mathematician. In the present memoir Mr. Green employs himself upon two or three other cases of the general problem, and in particular on the motion of waves in a *deep* sea. After solving this case, he adds, "We shall be able to deduce a singular consequence which has not before been noticed, that I am aware of." This consequence is, that any particle of the fluid revolves continually, (he might have added *uniformly*,

as his expressions show,) in a vertical circular orbit of which the radius decreases very rapidly as the depth below the surface increases. We may point out to Mr. Green that this conclusion had already been virtually drawn by Laplace in his *Memoir on the Tides*, published by the Academy of Sciences in 1775. We may, however, observe that this result has acquired a new interest since the experimental researches of the Webers on this subject, with which it is in complete accordance. Mr. Green has in another case (that of a canal with a triangular section) compared his theoretical results with the experiments of Mr. Russell, and finds the agreement much more close than that which is given by Mr. Russell's own empirical formula.

The constitution and motions of the supposed fluid of light and heat form a wider subject of investigation. Ever since it appeared by the great discoveries of Young and Fresnel, that the hypothesis of transverse undulations explains with such marvellous exactness the most complex phenomena of light, mathematicians have been endeavouring to demonstrate the mechanism of such undulations, and to determine their laws under various circumstances.

M. Cauchy in France, Sir William Hamilton and Prof. Macculagh in Ireland, Prof. Airy, Mr. Green, Mr. Kelland, Mr. Tovey in England, have employed on investigations of this kind all the higher resources of mathematics. In the volume now before us, we have, bearing on this subject, Mr. Green's memoir "*On the Laws of the Reflection and Refraction of Light at the common surface of two non-crystallized Media*;" and Mr. Earnshaw's "*On the Nature of the Molecular Forces which regulate the Constitution of the luminiferous Æther*." Mr. Green explains the peculiar starting-point of his researches in this manner. M. Cauchy had considered the luminiferous æther, and the bodies which act upon it, as systems of molecules in which every two particles act upon each other in the direction of the straight line which joins them. But this supposition, Mr. Green says, seems to involve too narrow a restriction; for many phenomena, those of crystallization for instance, seem to indicate certain polarities in the particles, which have never yet been shown to be resolvable into direct attraction and repulsion. Hence he selects for his basis a wider assumption, which may be expressed analytically, and which involves the precarious physical hypothesis of M. Cauchy as a particular case. He obtains from this principle various results, and in the first place this; that in the luminiferous æther the velocity of transmission of waves propagated by *normal* vibrations is very great compared with that of ordinary light. Mr. Green investigates the intensity of the waves reflected at the common surface of two media; and in the case of light polarized in the plane of incidence, obtains precisely the values given by Fresnel. In the case of light polarized perpendicular to the plane of incidence, it appears from the present investigations, that the expressions given by Fresnel are not rigorously true, but are only very near approximations. It appears that the intensity of the reflected wave will never become absolutely null, but only obtain a minimum value;

which value, in the case of reflection from water at the proper angle, is $\frac{1}{151}$ part of the intensity of the incident wave. This minimum value increases rapidly as the index of refraction increases; and thus the quantity of light reflected at the polarizing angle becomes considerable for highly refracting substances; a fact, which has been long known to experimental philosophers.

In Mr. Earnshaw's memoir the ætherial medium is treated as a system of detached particles; and he is led by his investigations to various conclusions, of which the most important are, that the molecular forces which regulate the vibrations of the æther do not vary according to Newton's law of universal gravitation, but that these forces are repulsive, and vary according to an inverse power of the distance greater than two. M. Cauchy, in his "*Mémoire sur la Dispersion de la Lumière*," had inferred from his analysis that "in the neighbourhood of contact, the action of two particles is repulsive, and reciprocally proportional to the fourth power of the distance."

Mr. Kelland, in a memoir contained in the previous volume of these Transactions, (vol. vi. Part I. p. 178) had been led by calculations founded upon the phænomena of the dispersion of light, to conclude that the particles of the æther act on each other with forces varying inversely as the square of the distance. We shall not here pretend to discuss the difference of the results thus obtained by these two mathematicians. But we must notice Professor Kelland's memoir "On Molecular Equilibrium," contained in the volume now under our notice. In this notice Mr. Kelland pursues a train of speculation somewhat similar to that employed in the last century by Dr. Knight, in his "Attempt to explain all the phænomena of Nature by two principles, attraction and repulsion;" and by Boscovich, in his "Theory of Natural Philosophy reduced to a single law of the forces which exist in Nature." Mr. Kelland states his assumption as follows: "I purpose to commence my investigation by retaining M. Mosotti's hypothesis of two systems of particles*, repulsive towards atoms of their own kind, but each respectively attractive towards the atoms of the other. We will call one system of particles *caloric*, and the other *matter*." He then adds the other suppositions by which these two elements are distinguished from each other; the atoms of caloric are distributed through space, the atoms of matter occupy only given positions. In both the density will vary from point to point; but the particles of matter are supposed to be much more widely separated than the particles of caloric; so that a material particle may be considered as a nucleus about which the particles of caloric are collected, forming its atmosphere. But Mr. Kelland afterwards determines the conditions of equilibrium of a system in which the atoms of caloric are repulsive of those of matter; and on the same hypothesis he determines the mutual action of two particles of matter, together with the caloric surrounding them. For the general relations between

* Taylor's Scientific Memoirs, vol. i. p. 448, and L. and E. Phil. Mag. vol. x. p. 320.

density of caloric, temperature, cohesion, and attraction of finite masses, which result from these investigations, we must refer to the memoir itself.

We shall notice, in the last place, Mr. Holditch's memoir "On Rolling Curves." The object of this paper is to determine curves of such a form that, revolving about two centres, one of them may communicate motion to the other, as in the case of the teeth of wheels; with the condition that the curves, in this communication of motion, are to roll upon each other without friction. Euler in the *Acta Petropolitana* had deduced the characteristic property of these curves, but he did not follow out the investigation so as to furnish actual forms of curves; nor has the method of obtaining such curves been pointed out by any previous writer. They are commonly found by a tentative process; but Mr. Holditch thought it worth while to search for rules and forms for their construction; and these he has found and given the present memoir. Some of the results are very curious and novel.

Principles of General and Comparative Physiology, intended as an Introduction to the Study of Human Physiology, and as a Guide to the Philosophical pursuit of Natural History. By WILLIAM B. CARPENTER, M.R.C.S., late President of the Royal Medical and Royal Physical Societies of Edinburgh, &c. &c. With 240 Figures on Copper and Wood. London, 1839. 8vo, pp. 480.

The science of physiology has been too generally considered by physical philosophers as beyond their pale. The nature of the phenomena which it embraces, and the mode in which it is to be pursued, have been regarded as sufficiently distinct to limit the cultivation of it, with few exceptions, to those who make it a part of their regular professional studies. We cannot but think that such a state of things may be advantageously modified. Men of general science are constantly invoking the aid of the physiologist, for the determination of most important and intricate questions; and too often is it found that this aid is unattainable, in consequence of the exclusive notions of the latter, who, from his want of truly philosophical principles, cannot meet the difficulties which he is expected to solve; and, on the other side, physiologists are too often content with a smattering of knowledge on physics and chemistry, which is more likely to lead them wrong than right. We do not mean to assert that there are not many bright exceptions on both sides; but we maintain that the cause of philosophy would be benefited if the barrier which is supposed to exist between the science of vitality and that of general physics were broken down, and if the cultivators of each were to make themselves acquainted with the *principles* of the other, and with the best mode of pursuing and extending both.

Such appears to have been the object of the author of the volume before us. From the dedication of it to Sir J. Herschel, we infer that he has been trained in the school of physical science; and throughout the work we perceive the influence of those grand prin-

ciples of inductive philosophy which have been and still are too much neglected by physiologists. All classes of living beings are regarded by him as of equal importance in a scientific view, as furnishing *instances*, by the collection and comparison of which *general laws* may be established. It is thus perceived that what is obscure in one is frequently evident in another; that the life of the simple zoophyte may elucidate, if properly observed, the varied phenomena presented by man; and that the functions of the humblest plant may be traced as fundamentally the same, though gradually becoming more complex, in the ascending scale of the animal as well as of the vegetable creation. Such a work, we cannot but hope, may contribute to excite and facilitate the study of physiology amongst those who make science their pursuit. We need hardly point out, that the connexion between these different branches of knowledge is daily being rendered more intimate, especially by the researches of the geologist and of the organic chemist; the former of whom requires to know those general laws which govern the conformation and distribution of organized beings, while the latter seeks to elucidate the mysteries of vital action, by ascertaining the extent to which the physical properties of matter are concerned in it. One of the most interesting examples of the value of such inquiries which has recently come under our notice, is the discovery of M. Poisseuille, that viscid fluids may be propelled through capillary tubes with much less effort than water or other liquids of aqueous consistence; and that a solution of gum, gelatin, albumen, &c. will pass readily through tubes so small as to resist the passage of water, whatever may be the degree of force employed.

A brief account of the contents of this treatise will serve to display its scope and tendency. The author states himself to have been led to its production by having "felt the want of a treatise which should give a comprehensive view of the science, embracing whatever general principles may be regarded as firmly established, and illustrating them as fully as could be done within moderate limits, yet without distracting the attention by profuseness of detail." It commences with an introduction, which presents a general account of the peculiarities of organized bodies, the elementary structure of plants and animals, and an outline view of the chief natural groups of these kingdoms, intended to facilitate the comprehension of the strictly physiological portion of the work. The first book is devoted to general physiology; and here we are conducted through a profound but lucid investigation into the nature and causes of vital actions, which we particularly recommend to the attention of those who have been accustomed to refer to the "vital principle" as an easy solvent for all difficulties. By comparing the phenomena of vital action with those of the inorganic world, the author shows that they are equally reducible to general laws which result from the properties with which matter has been endowed by the Creator. Of these properties some manifest themselves under the simple conditions which the ordinary changes in the inorganic world supply, and thus perform the actions termed

chemical and *physical*; whilst others can only be called into play under conditions of a more complex nature, which are only supplied by a living organized system, where many particles being combined by a previously-existing life into one structure, exhibit actions of a peculiar character, dissimilar to any they have heretofore presented, which are denominated *vital*.

The dependence of life upon external stimuli is then pointed out, and a great variety of interesting facts, many of them novel, are collected, relative to the influence of heat, light, and electricity upon living beings. The general laws which have been ascertained to govern the structure and actions of organized beings are then enunciated, and their application illustrated by examples. To pursue their application through the whole range of the animal and vegetable kingdoms is the object of the second book, entitled *Special Physiology*. Here each function is considered in detail, in the various phases under which it appears in the ascending scale both of the vegetable and animal kingdoms; the fundamental unity which prevails throughout is displayed; and the very extraordinary correspondence which exists between the transitory states of different organs in the embryo condition of the higher classes of each kingdom, and the permanent forms of the same in the lower, is demonstrated in a striking and satisfactory manner. This part of the volume is illustrated by a large number of well-executed figures, which greatly aid the comprehension of the text.

The author appears to us to have fulfilled his design in the most satisfactory manner; and as his work has been received with high approbation by the Medical Press, we can feel no hesitation in recommending it to our scientific readers as the one best calculated to impart to them a sound knowledge of the principles of physiology. We may add, that the clearness of its style, and the simple manner in which the highest truths on this deeply-interesting subject are presented to the student, render it not only a useful, but an agreeable book to any reader of ordinary intelligence.

XII. *Proceedings of Learned Societies.*

ROYAL ASTRONOMICAL SOCIETY.

Nov. 8, THE following communications were read:—

1839. **T** On the Determination of the Orbits of Comets, from Observations. By G. B. Airy, Esq. Astronomer Royal.

The author begins by remarking, that the generality given by Laplace to the investigation of the orbits of comets is so complete, and the variations on the method introduced by other writers so numerous, that, as regards generality and facility, the subject may probably be considered as exhausted. The method which is developed in the present Memoir professes to be merely a modification of Laplace's method, directed by considerations of a purely practical nature, which are known to the working astronomer; but

which, probably, have not occurred to the distinguished mathematicians, who have laboured on the theoretical difficulties of the problem.

“Every method,” the author remarks, “which I have yet seen requires that the observed geocentric places of the comet be reduced to longitude and latitude. The places must, however, in the first instance, be observed in right ascension and declination. Now, the conversion of right ascension and declination into longitude and latitude is one of the most troublesome operations that commonly occurs. It requires the use of 7-figure logarithms, and is liable to errors. An alteration in one original \mathcal{R} , or declination, requires a complete repetition of the calculations; and when all is done, the elements of the comet’s orbit are obtained as referred to the ecliptic; and, for convenience of calculating predicted places, it is generally necessary to refer them back to the equator. For these reasons, it has long since appeared to me desirable that the orbits should be deduced at once from the right ascensions and declinations. Since I have become familiar with the instruments used for observing comets, an additional reason has suggested itself. It is known that on the assumption of a parabolic orbit, the equation given by three complete observations, or by observations which furnish the \mathcal{R} and declination at a certain time, and their first and second differential coefficients, are one more than are necessary; and, therefore, it rests with the computer to use his discretion in rejecting one of the observations. Now, it often happens that the instrumental or observing errors in right ascension are of an order quite different from those in declination; and, if the method of computation proceeds at once from right ascensions and declinations, the astronomer can at once determine which of the observations ought to be rejected in the calculation, on the score of possible inaccuracy in the observation.”

The principal objection which has been made to Laplace’s method is the trouble of investigating the differential coefficients of the spherical co-ordinates. It must be avowed, that the process pointed out by Laplace is very laborious; but it may also be asserted, that the principal part of the labour is introduced without any necessity. Three observations, made at proper intervals, are sufficient to give the motion of the comet in either direction, and its two differential coefficients, with an amount of labour that is quite insignificant; or a great number may be introduced by a simple process well known to every computer, and involving very little trouble. In the present paper it is shown, that by adopting for epoch the middle time between the first and second observations, the great mass of the calculations of every kind may be made immediately after the second observation; and the operation, therefore, completed in a very short time after the third.

The author divides his paper into three sections. In the first he gives the “theory,” or analytical solution of the problem. On substituting, in the general equations of motion, the right ascension and declination of the comet at the epoch with their first and second

differential coefficients, which are given by the observations, he arrives at two equations in which the unknown quantities are ρ and

$\frac{d\rho}{dt}$ (ρ denoting the comet's distance from the earth). On elimina-

ting $\frac{d\rho}{dt}$ an equation is found of the following form—

$$C \cdot \rho = \frac{D}{(\rho^2 - E\rho + F)^{\frac{3}{2}}} + G,$$

where C, D, E, F, G, are known numerical quantities. The solution of this equation may be obtained with great facility (in respect of the general difficulties of the problem) by the method of trial and error; and the author recommends, that in all cases which admit of it, the equation be formed, and the solution found; not only because the method is comparatively easy, but also because it is perfectly general, no assumption of parabolic, circular, or any other form of orbit, having been made.

The author next proceeds to consider the cases in which the equation fails. These are, first, when the comet is in conjunction with, or in opposition to, the sun; or when the sun, the earth, and the comet, are in the same straight line. In this case the first side of the equation becomes 0 divided by 0; and, as the two equations which involve the first differential coefficient of the comet's distance, taken with respect to the time, also vanish in the same circumstances, the failure is absolutely beyond remedy, and we can only wait until the comet is in a different part of its orbit. Secondly, the equation fails when the apparent path of the comet is directed to or from the sun's place; but in this case, the two equations involving the first differential coefficient of the distance do not necessarily fail; and, in fact, they cannot both fail, excepting under the supposition of the first case; therefore, by using one of them, or a new combination of them together with some new single assumption (as for instance, that the comet is moving in a parabola of unknown perihelion distance), we may still determine the comet's distance. Thirdly, the application of the equation may fail from causes connected with instrumental observations; for as the second differential coefficients of the right ascension and declination both occur on the first side of the equation, and as these coefficients are affected by the whole of the errors of observations, which, if the interval between the observations is short, receive very small divisors, any failure in the instrumental determination will produce a large error in their proportionate values. As it will sometimes occur that the observations made in declination are far more accurate than those made in right ascension, or *vice versâ*, in most cases one of the two equations which contain the comet's distance and its first differential coefficient, will be preferable to the other; and the combination of this with the equation deduced from the assumption of a parabolic orbit, will lead to the elimination of the differential coefficient, and, consequently, give the distance.

Among the various changes to which the comet's apparent path is subject, and of which an arbitrary choice may be made, for the purpose of determining the distance in the cases in which the general equation fails, or becomes unsafe, the author considers the following to be the best:—viz. first, the curvature of the comet's path, produced by the sun's action (or the deflection measured only in the direction perpendicular to the apparent path); second, the acceleration in its path, produced by the sun's action (or the deflection measured only in the direction of its path); third, the deflection in the direction in which both the sun's action on the comet and the sun's action on the earth would cause a change of the comet's apparent place (or the deflection measured along the great circle joining the comet with the sun). These changes are severally considered, and the method of forming the equation proper for each condition explained, and rules deduced for the guidance of the computer in all the particular cases in which the direct method cannot be followed. In these investigations the correction of observed places of the comet for parallax is entirely omitted, as it is most convenient, when ρ is approximately found, to correct the observations for the corresponding parallax, to make the proper alteration in the second differential coefficients, and then to repeat the process of approximation to the value of ρ .

Having given the methods for finding the distance and its differential coefficient, the author concludes his first section with an indication of the process by which the elements of the orbit are computed. In the rules for the selection of the equations on the parabolic assumption, some considerations are introduced which are new and important.

The second section contains remarks on the method of obtaining numerical values of the differential coefficients of the right ascension and declination from the observations. In the use of these quantities, what we have to consider is, not the effect of absolute error in their values, but of proportional error. An error of a single second in the value of the second differential coefficient of \mathcal{R} may produce an ultimate error as great as would be produced by twenty seconds in the value of the first differential coefficient; or as great as would be produced by ten minutes in the \mathcal{R} itself. This consideration allows the computer to determine many of the numbers which enter into the equations after the second observation: the method of proceeding is as follows:—

“Adopt for the epoch the middle time between the first and second observations: then the first differential coefficients of α and β (α denoting the right ascension, β the declination) will be obtained accurately by dividing the changes of α and β by the intervening time; and the values of α and β for the epoch will be obtained with sufficient accuracy, by taking the means of α and β for the two observations.”

The third and last section of the Memoir gives practical rules for the computation of the observations. The successive steps of the process, from the first observations to the determination of the

different elements of the orbit, and the values of the quantities required for predicting geocentric places, are minutely and distinctly stated, so that the ordinary computer will find no difficulty in applying the method.

Extract of a Letter from Professor Schumacher to the Astronomer Royal, relative to the determination of differences of Longitude, by observations of Shooting Stars.

M. Schumacher states that, although observations of shooting stars have long since been proposed by Mr. Benzenberg as a means of determining differences of longitude, no attempt has yet been made to carry the plan into practice. With a view to ascertain the degree of exactness with which such observations can be made, he resolved to make some trials on the night of the 10th of last August. He preferred to observe the *extinction* of the meteor, because its apparition gives warning, and in some measure prepares the observer for the phænomenon. Having given no notice of his intention to other astronomers, he had no expectation of obtaining corresponding observations; but was agreeably surprised when he subsequently obtained them from Bremen, Breslau, and even Königsberg. They did not give very accurate differences of longitude, because the observers at those places had observed the apparition and not the extinction; and because, not having the same object in view, they did not ascertain the equation of the clock with precision. Nevertheless the observations gave approximate differences, and showed that the method is practicable.

LINNÆAN SOCIETY.

April 16, 1839.—Read, “On a Gall gathered in Cuba, by W. S. MacLeay, Esq., on the leaf of a plant belonging to the order *Ochnaceæ*.” By the Rev. M. J. Berkeley, M.A., F.L.S.

The gall is remarkable for its very close resemblance in habit and form to some epiphytous Fungi, for possessing a distinct operculum, and, especially, for bursting through the cuticle, which surrounds it in the form of a few laciniae at the base. Mr. Berkeley pointed out various forms of galls and other productions of insects which have been described as Fungi, but in none is the resemblance so striking as in the present. He regretted that he was not able to throw any light upon the animal by which it is caused, though he was able to state positively that it is an animal production, as in most instances decayed exuviae were found in its cavity, and in one case a little imperfect grub, which was however unfortunately lost.

May 24, 1839.—The Lord Bishop of Norwich, President, in the Chair.—This day, the Anniversary of the birthday of Linnaeus, and that appointed in the charter for the election of Council and Officers, the President opened the business of the Meeting, and in stating the number of Members whom the Society had lost during the past year, gave the following notices of some of them:—

Samuel Brookes, Esq.—Mr. Brookes was devoted to the science of Conchology, and possessed a valuable collection of British and Fo-

reign Testacea. He was the author of an Introduction to the Study of Conchology which appeared in 1815.

The Rev. Martin Davy, D.D., F.R.S., Master of Caius College, Cambridge.

The Rev. Richard Dreyer, LL.B.

John Lord Farnham.

Charles Holford, Esq.

Lawrence Brock Hollinshead, Esq.

John Hull, M.D.—Dr. Hull was ardently attached to the study of Botany, and in the midst of an extensive medical practice, he found occasional moments of leisure to devote to the cultivation of his favourite pursuit. We are indebted to him for the publication of a British Flora in 1799, of which a second edition appeared in 1808; and the Elements of Botany, in 2 volumes, 8vo, in 1800. These works, highly creditable to their author, tended to increase the taste for botanical pursuits.

Matthew Martin, Esq.—Mr. Martin reached the advanced age of 90. He became a Fellow of this Society in 1791.

George Milne, Esq.—Mr. Milne pursued with much ardour the study of Entomology for more than half a century, and his name is familiar to the cultivators of that branch of science in this country. He possessed an extensive cabinet of insects, particularly rich in British and Exotic Lepidoptera. He had retired from London for several years to his native place Johnshaven, Kincardineshire, where he died some months ago at an advanced age.

The Rev. Robert Nixon, B.D., F.R.S.

William Younge, M.D.—Dr. Younge was the early friend and a fellow student of our late distinguished President and Founder Sir J. E. Smith, and the companion of his tour on the continent in the years 1786 and 1787, of which an account appeared in three volumes 8vo, in 1793, and a second edition in 1807. Dr. Younge was elected a fellow of this Society at its first institution in March 1788.

Amongst the Foreign Members occur *M. Frédéric Cuvier*, Member of the Academy of Sciences of the French Institute, the younger brother of the great Cuvier, and eminently distinguished as a systematic zoologist. He was the author of a work on the value of the teeth as affording zoological characters in the class mammalia, and of a number of valuable papers on Descriptive Zoology in the *Annales et Mémoires du Muséum*. He likewise wrote the principal part of the text to the *Histoire Naturelle des Mammifères*, a work which he had undertaken in conjunction with Geoffroy St. Hilaire. Among his last productions may be noticed his *Mémoire sur les Gerboises et les Gerbilles*, printed in the second volume of the Transactions of the Zoological Society of London. He was distinguished, like his brother, for his candour and frankness of character, and a total freedom from those petty jealousies which too often beset men of science.

M. Charles de Gimbernat.

Gaspard Count Sternberg, Founder and President of the Royal Museum of Natural History at Prague, a distinguished patron of

science, and author of a valuable original work on Fossil Plants, which were chiefly obtained from his own coal mines in Bohemia, and of an excellent Monograph of the genus *Saxifraga*, illustrated by coloured figures. To him we are indebted for the recovery of the vegetable treasures collected by Hænke in Peru, Cochabamba, and in the Philippines, whither he had accompanied the Spanish voyage of discovery under the celebrated, but unfortunate, Malaspina. These interesting plants have been published by Presl, under the auspices of Count Sternberg, in a work entitled '*Reliquiæ Hænkeanæ.*' Count Sternberg was distinguished for his urbanity, hospitality, and an eager desire to promote every useful work. He left his collections and books of Natural History to the Museum already mentioned.

Among the Associates are the following :—

Mr. John Hunneman.—Mr. Hunneman having been long the medium of communication between the botanists of this country and those of Germany, Switzerland, and Russia, our collections have been enriched through his means with a vast variety of new and interesting plants. A curious Mexican genus, belonging to the natural family *Papaveraceæ*, bears his name, and commemorates the services rendered by him to science.

Mr. George Penny.—He was well acquainted with the plants which he successfully cultivated, and was the author of the '*Hortus Epsomensis*', and of several papers on Garden Botany in Mr. Loudon's *Gardener's Magazine*.

Mr. William Weston Young made the drawings for Mr. Dillwyn's valuable work on British *Confervæ*, and a series of drawings of British birds now in the possession of Mr. Yarrell.

The President also announced that twenty Fellows, five Foreign Members, and two Associates had been elected since the last Anniversary.

At the election, which subsequently took place, the Lord Bishop of Norwich was re-elected President; Edward Forster, Esq., Treasurer; Francis Boott, M.D., Secretary; and Richard Taylor, Esq., Under-Secretary. The following five Fellows were elected into the Council in the room of others going out, viz. W. J. Burchell, Esq., J. W. Lubbock, Esq., Hugh Duke of Northumberland, John Forbes Royle, M.D., and William Yarrell, Esq.

NEW SYSTEM OF POSTAGE.

Our experience during the past month of the New System of Postage, though as yet in its incipient and imperfect state, has been most satisfactory and gratifying, in the facility and copiousness of intercourse with our scientific correspondents, however distant. Information, observations, suggestions, corrections, proofs, drawings, inclosures of various kinds, already begin to be interchanged with a freedom which is as delightful as it is new and strange, and therefore not yet enjoyed to its full extent. We shall find, we are persuaded, ample cause for gratitude to Mr. Hill, by whom so great a benefit has been suggested and perseveringly matured; and not less to Mr. Baring, the present Chancellor of the Exchequer, for having faithfully, diligently, and strenuously surmounted every obstacle to the accomplishment of an object, of which he has duly appreciated the importance, as regards not only the commercial, but the moral and intellectual interests of the country.

LETTER BAROMETER.

The equitable system of rating the postage of papers by weight gives rise to the necessity for a ready method of determining what charge any letter or packet may be liable to.

If extreme accuracy be desired, nothing will be found equal to good scales and weights; but as this is seldom essential, and such an apparatus is not very convenient on a writing-table, many contrivances of a less cumbrous nature and less subject to derangement have been produced.

These are generally variations of the steelyard, and of course require the adjustment of a counterpoise for each different case, which is somewhat troublesome and liable to mistakes.

We have lately seen a very simple and ingenious instrument, which avoids these inconveniences. It indicates the weight at once, is not subject to get out of order, and while it occupies but little space, is rather of an ornamental form than otherwise.

The instrument consists of a small tube containing a portion of quicksilver, in which is immersed a rod, furnished on its top with a tablet, on which a letter or even an unfolded sheet of paper may be placed. The rod sinks into the mercury precisely in proportion to the weight placed upon it, and by a graduation on the stem, it is at once seen what the charge of postage will be.

This little contrivance is the invention of John Taylor, Esq., F.R.S., and the instruments are very neatly made and sold, in a variety of forms, by Mr. Lund, No. 24, Fleet Street.

NEW SCIENTIFIC BOOKS.

Scripture and Geology.—On the Relation between the Holy Scriptures and some parts of Geological Science. By John Pye Smith, D.D., F.G.S., Divinity Tutor in the Protestant Dissenting College at Homerton. 1 Vol. 8vo. Jackson and Walford. Of this highly interesting and important work we hope shortly to give an account in some degree worthy of its merits.

Dr. Meyen's Report on the Progress of Vegetable Physiology during the year 1837. Translated from the German by William Francis, A.L.S.

METEOROLOGICAL OBSERVATIONS FOR NOV., 1839.

Chiswick.—Nov. 1. Hazy: rain. 2. Rain. 3, 4. Foggy: rain. 5. Rain: fine. 6. Hazy: rain. 7. Rain. 8. Hazy: rain. 9. Fine: drizzly. 10. Hazy: rain. 11. Clear. 12, 13. Hazy: overcast. 14. Fine: rain. 15. Hazy. 16. Overcast: clear and fine at night. 17. Rain. 18. Heavy rain. 19. Fine: a large halo round the moon at night. 20. Fine: rain. 21. Stormy and wet. 22. Overcast: fine. 23. Clear. 24. Rain. 25. Cloudy: rain: almost a hurricane at night. 26. Clear. 27. Dense fog. 28. Hazy. 29. Heavy rain: 30. Overcast: heavy showers.

Boston.—Nov. 1. Stormy. 2. Cloudy: rain P.M. 3. Cloudy. 4, 5. Rain: rain early A.M. 6, 7. Cloudy. 8. Cloudy: rain P.M. 9. Cloudy. 10. Cloudy: rain A.M. and P.M. 11. Cloudy. 12, 13. Foggy. 14. Cloudy: rain P.M. 15. Cloudy: rain A.M. 16. Fine. 17. Fine: rain P.M. 18. Cloudy: rain early A.M. 19, 20. Fine. 21. Cloudy: rain early A.M.: rain P.M. 22. Cloudy. 23. Fine. 24. Rain. 25. Cloudy. 26. Cloudy: rain early A.M. 27. Fine: rain and snow P.M. 28. Cloudy. 29. Rain: rain early A.M. 30. Stormy.

Applegarth Manse, Dumfries-shire.—Nov. 1. Storm of wind with slight showers. 2. Fair: weather moderated. 3. Fair: fine. 4. Rather moist. 5. Clear and cold. 6. Quiet day and cloudy. 7. The same: slight drizzle P.M. 8. Cloudy and moist. 9. The same: rain A.M. 10. Quiet day: moist atmosphere. 11. Calm day: still moist. 12. Showery all day. 13. Mild day throughout: no rain. 14. Drizzly and gloomy: a true Nov. day. 15. Rain all day: heavy P.M. 16. Showery throughout. 17. Fine day and fair. 18, 19. Drizzling day. 20. Very fine day: rain P.M. 21. The same: rain A.M. 22. Fine: one shower P.M. 23. Frosty morning: shower at noon. 24. Rain all day. 25. Showery A.M.: cleared up P.M. 26. Cold and frosty morning: shower snow. 27. The same: frost increasing: more snow. 28. Freezing all day: snow lying. 29. Storm of wind and rain: snow gone. 30. Rain nearly all day.

of the Horticultural Society at Chiswick, near London; by Mr. VEALL at Boston, and by Mr. DUNBAR at Applearth Manse, Dumfriesshire.

Days of Month. 1839. Nov.	Barometer.			Thermometer.				Wind.				Rain.			Dev. point. Lond. Roy. Soc. 9 a.m.				
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. Self-register.		Chiswick.		Boston. 8½ a.m.	Dumfries-shire.	Chiswick.	London: Roy. Soc. 9 a.m.		Dumfries-shire.			
		Max.	Min.		Max.	Min.	Max.	Min.	Max.	Min.									
1.	29.714	29.730	29.677	29.50	29.88	29.75	41.2	41.7	39.8	49	42	41.5	47	40	E.	E.	.044	.14	39
2.	29.618	29.641	29.585	29.30	29.74	29.60	42.7	46.0	40.0	49	42	46	48	43	E.	E.	.044	.30	41
3.	29.612	29.629	29.518	29.26	29.58	29.57	48.2	49.0	42.7	52	48	47	50	43	E. calm	E. calm	.302	.28	41
4.	29.512	29.533	29.503	29.01	29.48	29.43	50.3	51.7	46.2	42	39	50	49	45½	E. E.	E. E.	.263	.11	46
5.	29.572	29.768	29.599	28.99	29.49	29.62	47.7	53.3	45.2	50	39	46	47	42	SW. NW.	N. E.	.088	.01	46
6.	29.746	29.763	29.659	29.22	29.62	29.60	45.7	46.5	45.3	51	45	46	47	37	W. calm	ESE.	.116	.09	45
7.	29.624	29.681	29.608	29.23	29.60	29.60	48.3	49.2	45.6	51	47	47	49	42	E. calm	E.	.033	.10	46
8.	29.806	29.699	29.623	29.16	29.49	29.41	49.8	52.0	48.5	55	46	50.5	50½	44½	E. calm	E.	.033	.05	46
9.	29.506	29.507	29.446	29.	29.31	29.23	49.3	49.8	48.7	55	50	50	54	46	E. SE.	E.	.01	.03	46
10.	29.378	29.381	28.978	28.85	29.17	29.10	52.4	54.6	48.7	56	50	50	52½	47½	E. SE.	E.	.01	.55	49
11.	29.248	29.499	29.253	28.70	29.04	29.10	48.7	48.4	47.5	55	38	50	50	39	SW. SW.	SW.	.463	.37	49
12.	29.418	29.579	29.411	29.	29.24	29.40	47.9	48.7	46.0	55	43	40	49½	38	E. calm	SW.	.463	.37	48
13.	29.748	29.944	29.739	29.25	29.63	29.74	48.7	49.4	47.4	51	43	47	49	41	E. calm	SW.	.463	.37	48
14.	29.988	29.984	29.939	29.45	29.70	29.70	47.4	48.0	47.4	55	49	47	48	40	E. calm	SE.	.463	.04	48
15.	29.980	29.978	29.824	29.50	29.70	29.52	52.5	52.8	47.4	56	46	51	54	45½	E. calm	SE.	.463	.04	48
16.	29.886	29.993	29.872	29.34	29.51	29.55	52.8	53.0	51.5	56	48	50.5	56	49	E. calm	SSW.	.036	.12	50
17.	30.044	30.029	29.998	29.43	29.73	29.79	51.8	52.4	50.6	57	51	48	51	43	E. calm	SSW.	.036	.06	50
18.	29.992	30.280	29.983	29.44	29.73	29.74	54.8	55.3	50.6	37	43	53	52	44	E. calm	SW.	.150	.36	52
19.	29.992	29.985	29.948	29.39	29.60	29.65	49.4	50.3	47.9	55	38	46	50½	47	E. calm	SW.	.063	.08	50
20.	30.064	30.150	29.990	29.56	29.83	29.63	44.7	49.7	44.6	35	29	42	46	37	SW. NW.	E.	.063	.32	45
21.	29.472	29.700	29.400	29.13	29.42	29.72	45.2	48.2	42.4	48	36	43	46	39	E. SE.	E.	.244	.14	44
22.	29.916	30.152	29.907	29.49	29.91	30.07	42.2	42.7	39.6	48	30	42	45	33	E. calm	SW.	.036	.03	42
23.	30.208	30.272	30.150	29.80	30.00	29.99	38.2	40.5	35.7	46	30	33.5	43	28	E. calm	W.	.036	.05	39
24.	30.156	30.151	29.826	29.65	29.71	29.48	40.3	40.7	37.0	54	40	52	51½	40½	NW. calm	WSW.	.036	.01	38
25.	29.680	29.675	29.223	28.97	29.23	29.20	50.7	51.2	40.3	56	36	42	49½	41½	SW. calm	NE.	.169	.12	45
26.	29.420	29.432	29.410	28.98	29.21	29.21	42.2	42.5	38.2	46	21	41	38	30	W. calm	NNE.	.036	.25	45
27.	29.436	29.538	29.420	29.01	29.29	29.38	34.7	44.6	34.6	36	32	31.5	36	30	E. calm	NNE.	.036	.88	37
28.	29.546	29.584	29.569	29.15	29.41	29.53	39.2	39.8	33.4	41	32	37.5	34	23	SE. calm	ENE.	1.460	.06	37
29.	29.268	29.298	29.159	29.04	29.41	29.24	43.3	44.0	39.4	51	41	45	44	28½	E. SE.	ENE.	.133	.37	39
30.	29.606	29.665	29.621	29.16	29.19	29.47	42.4	42.7	42.3	48	28	43	45½	43	S. NW.	ENE.	.250	.04	38
Mean.	29.705	29.771	29.627	29.23	29.528	29.533	46.4	48.0	43.8	50.53	40.06	45.3	47.7	39.6			Sum. 4.207	4.27	Mean. 44.

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

FEBRUARY 1840.

XIII. *An Account of some Experiments made in the South of Virginia, on the Light of the Sun.* By JOHN WM. DRAPER, M.D., Professor of Chemistry in the University of New York.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE just seen in the Journals for the current month, brought out by the British Queen, a letter from Sir J. Herschel to the [British] Association for the Advancement of Science, in reference to some remarkable actions of the different colours of the solar spectrum.

About five years ago, having the advantage of a bright and almost tropical sky, I amused myself with attempting a repetition of Morichini's experiment for the magnetizing of steel, and was led to some results in respect to the chemical action of the sun's rays, which appear to bear very much on the subject of the letter above alluded to. Most of these have been published in the Journal of the Franklin Institute of Philadelphia; but as they do not appear to have been noticed in England, I will ask the favour of a page or two of your excellent Magazine, to give my testimony on a subject, which now appears to excite so much interest.

1. If you pass a beam of the sun's light through a solution of chromate of potassa, it can no longer blacken a piece of sensitive paper; if you converge the light which has thus passed through a stratum of this fluid, by means of a lens, chloride of silver will remain for a long time without much change, in the focus.

The list, which was published in the Journal above named, of solutions possessing this power, is as follows:

Phil. Mag. S. 3. Vol. 16. No. 101. Feb. 1840. G

Bichromate of potassa.

Chromate of potassa.

Yellow hydrosulphuret of ammonia.

Hydrosulphuret of lime.

Muriate of iron.

Chloride of gold.

Chloride of platinum.

It is to be remarked, that every one of these solutions is *yellow*, but I also found that a great many *vegetable coloured infusions* would in like manner absorb the chemical rays, especially those which have a *yellow* tint.

2. When I exposed pieces of paper covered with a layer of chloride of silver, to a beam which had passed through the red sulpho-cyanate of iron, the paper became of a brick-red colour; if to a beam which had passed through a solution of sulphate of copper and ammonia, it became of a blue brown; and lastly, on exposing a piece in a box which I shall presently mention, for five days, to light which had been acted on by bichromate of potassa, it became perceptibly of a faint yellowish green.

3. It is very probable, that there exist in the sunlight, rays having particular chemical powers.

A beam which has passed through bichromate of potassa, does not appear to cause the union of a mixture of chlorine and hydrogen. I kept such a mixture for several hours in it, and could not perceive any change.

But this same beam can nevertheless enable vegetable leaves to effect the decomposition of carbonic acid. I took a wooden box, about a cubic foot in dimensions, and having removed its bottom, replaced it with a pair of parallel plates of glass, so adjusted that there was an interstice between them of half an inch or thereabouts. Into the trough thus formed, I poured a solution of bichromate of potassa, or any other salt under trial, and the box being raised on one end, served as a closet in which bodies could be exposed to the action of beams that had passed through any given medium.

In this little chamber, its trough being filled with a solution of the bichromate, I placed a matrass containing water slightly impregnated with carbonic acid, and a few vegetable leaves; after a little while, air bubbles were copiously given off; there had been placed, similarly in all respects, another matrass in the direct rays of the sun, and when a quantity of gas sufficient for analysis was evolved, it was found that carbonic acid had in both cases been decomposed, though, as might have been expected, in the latter more energetically. The result gave a mixture of carbonic acid, oxygen, and ni-

trogen : the uniform appearance of this latter body was subsequently traced to the leaves employed.

Plants, also, become green in light that has been submitted to the action of these yellow salts, and therefore deprived of the rays that blacken chloride of silver. I took a number of pea-plants out of the garden, in May 1837, and caused them to vegetate in light modified in this way, and also in light which had passed through sulpho-cyanate of iron, and sulphate of copper and ammonia, &c., but in every instance the leaves became green. It may also be mentioned, that seeds of common cress were caused to germinate and grow under these circumstances; the young plants after reaching a certain size were always green, but those which had grown in the dark had yellow leaves and white stalks.

Professor Silliman states, in one of the early numbers of his Journal, that he witnessed an explosion of hydrogen and chlorine, caused by the light of a common fire.

4. Ritter was the first who asserted, that the opposite extremities of the spectrum possess opposite powers of chemical action : he states that phosphorus will emit fumes in the red ray, but if the violet be thrown on it, it ceases to smoke : this experiment I repeated often, and under favourable circumstances, but could not make it succeed.

5. I could succeed, however, in showing very beautifully the interference of that class of chemical rays which blacken chloride and bromide of silver, but failed in trying to produce their polarization, for want of proper apparatus. An electric current circulating in a wire does not seem to have any influence on these chemical rays ; I found that the same neat magnified image of the wire was obtained, on chloride paper, when it was placed in a beam diverging from a lens, whether the current was made to pass or was stopped.

So much for chemical actions ; let me now ask your attention to a mechanical result of solar light, which is very curious.

(a). Having made a large air-pump jar very clean and dry, place a few pieces of camphor on the plate of the pump, and exhaust. Carry the pump with its receiver into the sunshine, and very soon you will see all that side which is nearest the sun covered with crystals, but there will be few or none on the side which is furthest from him. With the brilliant sun of Virginia, I have seen this effect take place, and beautiful stellated crystals appear *in four minutes*, literally covering the whole of the upper parts of the jar nearest the sun.

(b). Or, make in a tube of half an inch or more in diameter, and upwards of thirty inches long, a torricellian vacuum ; pass up through the mercury a fragment of camphor. The

tube may now be kept for any length of time in the dark, without anything happening; but bring it into the beams of the sun, and in a few minutes crystallization will happen, on the side next the luminary.

(c). Again, paste on the inside of an air-pump jar, a piece of tinfoil an inch in diameter, and having operated as in experiment (a) expose this side towards the sun. Crystals will soon form, but the tinfoil will protect the glass in its vicinity, and none will be found within a certain space round the metallic circle.

(d). Crystallization is not necessarily connected with these results: the vapour of mercury in a torricellian void is condensed towards the light; so also the dew which settles on the inside of a jar containing water is always on the side nearest the window. The rays of the sun have also the power of decomposing a solution of chloride of gold: the metalline spangles are deposited on that side of the glass which is nearest the light.

Artificial light gives none of these results.

(e). Having removed the piece of tinfoil used in experiment (c), place it on a little stand in front of the receiver; it will hinder the crystallization taking place in the parts on which its shadow is cast, and also for a certain space in the vicinity.

(f). Take a jar that has already been coated with crystals, place the tinfoil before it, and it will remove all those crystals which are within its shadow.

(g). Instead of using a piece of tinfoil as in experiment (c), make the receiver hot, and rub upon it a piece of resin, so as to leave a transparent circle of that substance; expose to the light, and it will be found that the resin cannot protect the glass.

(h). If along the inside surface of a vessel, about to be exposed to the sun, a glass rod be rubbed, rows of crystals will be deposited on the lines which were described by the end of the rod, but the vessel must be very dry for this experiment to succeed.*

Now, can we explain these singular results on any other *known* principle than this; that the side of the jar nearest the sun radiates freely the heat that it receives, back again, whilst radiation is interfered with at the other side; that in point of fact the anterior side is the colder, and the other the hotter?

Yours respectfully,

University, New York, Nov. 28, 1839.

JOHN W. DRAPER.

* This result would appear to point to some change of the mechanical condition of the glass, affecting either the radiation from its surface, or that through its substance, or both.—EDIT.

XIV. *On a remarkable Fall of Hail ; with Observations on the probable Nature of such Phænomena.* By P. J. MARTIN, Esq., F.G.S.

To Richard Taylor, Esq.

DEAR SIR,

NOT having met with any notice of the remarkable fall of hail which took place during the storm of thunder and lightning that passed over the counties of Sussex, Surrey, and Middlesex on the evening of the 7th of last July, I beg leave to offer you the following memorandum respecting it, with some short observations on the probable nature of such phænomena, which, if not altogether new, may coincide with and serve to strengthen the opinions of more experienced meteorologists.

Except in the fall of very large hail, the storm above-mentioned did not differ materially from the wide-spreading and grander thunder storms which sometimes gather on our coasts, and pass over the metropolis, from the south-west, after a sultry day or two in the middle of summer, but of which we have not had any very remarkable examples for the last five or six years.

One of these storms generally rises slowly over the South-Downs towards evening, in the form of a flimsy cirrostratus, gradually deepening as the night advances, and engendering denser cumuli as it draws inland. In general it is met by an under current of air from the north-east; but this is not invariably the case, for the under current often comes in at an acute angle with the motion of the gathering clouds, from the south or south-east; and sometimes, as in this instance, there is a dead calm below, whilst the clouds are advancing with considerable rapidity in the higher regions. I may observe, in passing, that a long-continued and steady influx of a warm north-easterly current towards the points of precipitation generally characterizes the grandest of these exhibitions; so that it is common to hear it said here that “a tempest,” for so the Sussex people call such a storm, “comes up against the wind.”

On the evening in question, the storm gathered on the line of coast between Selsea Bill and Beachy Head, was heavy over Brighton, and seemed to have its nucleus or central range from about Shoreham over Henfield, East Grinstead, and Croydon. On its western verge it passed over Arundel, Pulborough, Horsham, and Dorking, and was noted at Chiswick to be “accompanied with unusually little rain.” The hail which I am about to describe was confined to this

western extremity, and its ravages did not extend more than about a mile and a quarter wide, whilst in length they reached about twenty miles, viz. from Arundel to the vicinity of Horsham. Hail fell, I believe, further on, quite into Surrey, but the fall of large stones was limited to the space above mentioned. We had been watching the rise, and dissolution into the expanding body of the nimbus of many heavy cumuli from the south and south-west, with some grand explosions of thunder and lightning, when we observed a dense mass approach us in that direction from the Arundel quarter, accompanied by a rushing or rather roaring sound, clearly to be distinguished from the thunder, and attended with a pretty sharp blast of wind. In a few moments hail of the ordinary kind began to fall copiously, and this in a few moments more was intermixed with stones of an enormous size, the slapping of which could be clearly distinguished from the roaring of the mass of other hail on the slated roof of the summer-house in which I and my family had taken shelter. Very little rain fell, and the duration of the hail-storm was about ten minutes, only five of which was occupied by the fall of the largest hail-stones. On its clearing off, the ground was observed to be whitened by the hail, amongst which the large stones lay like tennis-balls amongst marbles; and on measuring some of them, after they had lain several minutes melting on the ground, we found many five, six, and seven inches in circumference. These large stones were more compact in their structure than the smaller ones, and were all of the flattened spheroidal form, and likened by many of the common people in size and shape to their thick old-fashioned watches. A dead calm succeeded to the passage of the storm, and the atmosphere continued to be encumbered with dark clouds, but without any more rain during the night.

The congelation of large drops of rain at the moment of aggregation, and the formation of ordinary hail, and even a considerable accretion of more ice to the original globule in its passage downwards, do not seem to be very difficult of comprehension and explanation. But there is only one way in which I can suppose such masses of ice as these can be suspended long enough in the atmosphere to grow to such enormous sizes, and that is by the assistance of a nubilar whirlwind or water-spout (*Trombe aërienne*) with sufficient power to keep them in its whorl, and to resist the earth's attraction, whilst the concretive action is going on, till their momentum overcomes the suspending power, or till they are thrown beyond the range of its intensity. That such operations

are amongst the reciprocal electrical phænomena of the clouds, distinct from, though allied to the water-spout, is, perhaps, well-known; and I was myself once witness to an appearance of this sort, between a higher and a lower cloud, that had the strongly electric aspect before they had resolved themselves into nimbus. It was a bent narrow column of dark vapour, which I could distinctly observe to be in rapid rotatory motion, passing from one cloud to the other, continuing for some minutes, and then gradually disappearing. During this time it emitted no sound, and had no visible connexion with the earth whatever.

The above theory of hail-stones will be further corroborated if we consider the form of the stones in this instance, viz. a sphere flattened at its poles, as the result of a rotatory motion; especially if it be a law, as perhaps it is, *that all solids in rapid gyration acquire per seipsos a rotation on their own axes.*

I am, dear Sir, yours, &c.

Pulborough, Dec. 16, 1839.

P. J. MARTIN.

XV. *Notice of certain Meteorological Phænomena observed at Swansea.* By J. W. G. GURCH, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THINKING that the following notice may prove interesting to some of your meteorological readers, I forward it for insertion in your valuable publication.

On the morning of the 20th of November, an unusual rise in the barometer was observable, as sudden as it was great. At 5 p.m. on the 19th, my barometer stood at 29·75; at 9 a.m. on the 20th at 29·99, being a rise of 0·24. At 9 a.m. on the 21st it sunk to 29·99, being a fall of 0·70; and so sudden a rise and fall I have not had occasion to record in my registry, now kept for the last four years. The wind during the whole period was a dead calm; the weather cloudy and hazy, with occasional light showers. A similar phænomenon was observed by my friend Mr. Addison of Malvern. The sudden *fall* of the barometer was noticed by that gentleman on the 20th, and the rise on the 21st, and like mine unaccompanied with wind, and at Malvern no rain fell. The explanation of this sudden rise and fall I am yet to learn, and should be glad if any of your correspondents could elucidate the subject.

On Wednesday, Nov. 6, the most brilliant meteor occurred that has been observed here for a great length of time, illu-

minating perfectly the principal street of the town: this occurred about 10 p.m.

On Monday, Nov. 10, at 1 p.m., the wind, which had been *perfectly calm* all the morning, suddenly rose, and with great violence blew for a minute or two at a pressure of four pounds to the square foot, and veered at the same moment direct from E., at which the vane had been standing all the morning, to W. continuing from that quarter for the remainder of the day, and immediately on so doing subsiding again to a complete calm.

This morning, Dec. 18, we were visited with a gale of wind surpassed only in violence by that of the 8th of May last. I send you the following table, drawn up from the actual markings of my self-registering anemometer and pluviometer.

Date.	Hour.	Pressure in Pounds on Square Foot.	Amount of Rain in 100dth of an Inch Cistern or Receiver 1 foot square.
Tuesday, Dec. 17.	10 a.m.	$\frac{1}{2}$	
	11	1	
	11 30 min.	3	
	12	3	
	1	$1\frac{1}{2}$	
	2	4	
	3	3	
	4	$1\frac{3}{4}$	
	5	5	
	6	2	
	7	2	
	8	6	
	9	6	From 9 to 10 0·03
	10	5	
	11	5	11 to 12 0·03
	11 30 min.	7	12 to 1 0·02
	12	9	1 to 2 0·03
	1	9	2 to 3 0·03
	2	10	3 to 4 0·02
	3	9	4 to 5 0·01
	4	10	5 to 6 0·02
	5	13 !!	6 to 7 0·01
	6	8	
	7	7	
	8	8	
	9	6	9 to 10 0·01
	10	4	10 to 11 0·01
	11	3	11 to 12 0·01
	12	4	

Being a total of 0·21 of rain fallen from 10 a.m. on Tuesday to 12 p.m. on Wednesday, the wind the whole time blowing from the S.E., only twice for a minute or two getting to the N. of E. viz. at 3 a.m. and 8 a.m. of December 18.

It will be my endeavour to ascertain the other localities visited by this storm, as the former ones registered by my anemometer, and also by that used by Mr. Osler of Birmingham, would appear to have occurred at the time that might have been expected for each locality within the circle of the storm, thereby bearing out most fully Col. Reid's ingenious theory. The storm of Tuesday last was at its maximum at about a quarter to 5 a.m. of the 18th, being then at the pressure of 13 pounds on the square foot, or according to Dr. Hutton's table, travelling at the rate of about 60 miles per hour. The barometer on Tuesday at 9 a.m. stood at 29·82, it fell at 5 p.m. to 29·58; at 9 a.m. on Wednesday (to-day) to 29·27, and at 5 p.m. to 29·19, being a fall of 0·73 in the twenty-four hours.

Should the above remarks be worth your acceptance, they are most perfectly at your service.

I remain, Gentlemen, yours, &c.

Swansea, Dec. 18, 1839.

J. W. G. GUTCH.

XVI. *The Bakerian Lecture.—On the Theory of the Astronomical Refractions.* By JAMES IVORY, K.H., M.A., F.R.S. L. & E., *Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Götting. Corresp.*

[Continued from vol. xv. p. 507, and concluded.]

12. WE next proceed to inquire into the influence which the term multiplied by f' , before omitted, may have on the refractions.

Investigation of the integral Q_3 .

The expression of this integral is,

$$Q_3 = \int_0^m \frac{e \, dx}{\Delta} \left(8 c^{-2x} - 8 c^{-x} + 7 x c^{-x} - 2 x^2 c^{-x} + \frac{x^3}{6} c^{-x} \right),$$

which is a negative quantity, as appears from the valuation of it in § 9: it will therefore contribute to distinctness if its sign be changed, in which case it will be thus written,

$$Q_3 = \int_0^m \frac{e \, dx}{\Delta} \left(-8 c^{-2x} + 8 c^{-x} - 7 x c^{-x} + 2 x^2 c^{-x} - \frac{x^3}{6} c^{-x} \right);$$

and the formula for refractions will now be,

$$\delta \theta = \sin \theta \times \frac{\alpha (1 + \alpha)}{\sqrt{5i}} \left(Q_0 + \lambda Q_1 - f Q_2 - f' Q_3 \right).$$

Suppressing the tedious operations of reducing, we may

put the integral Q_3 , taken indefinitely, in the following form, which it is not difficult to verify by differentiating :

$$\begin{aligned}\varepsilon &= \frac{1-e^2}{e}, \\ Q_3 &= -4 \int \frac{e \cdot 2 dx c^{-2x}}{\Delta} + \frac{91}{16} \int \frac{e dx c^{-x}}{\Delta} \\ &\quad + \left(\frac{215}{16} \varepsilon^2 + \frac{175}{16} \varepsilon^4 + \frac{125}{48} \varepsilon^6 \right) \cdot \int \frac{e dx c^{-x}}{\Delta} \\ &\quad + \frac{c^{-x} \Delta}{e} \left(\frac{185}{16} + \frac{125}{12} \varepsilon^2 + \frac{125}{48} \varepsilon^4 \right) \\ &\quad - \varepsilon \left(\frac{185}{16} + \frac{125}{12} \varepsilon^2 + \frac{125}{48} \varepsilon^4 \right) \\ &\quad - \frac{c^{-x} \Delta}{e} \left(\frac{95}{24} x - \frac{5}{12} x^2 + \frac{25}{24} x \cdot \varepsilon^2 \right).\end{aligned}$$

This being the indefinite integral, the value of Q_3 in the formula for the refractions will be obtained by putting $x = m = 10$; which gives

$$\frac{c^{-x} \Delta}{e} = \frac{1+e^2}{e} \cdot c^{-m};$$

and this value, as well as that of ε , being substituted, the quantity sought will be expressed as follows :

$$\begin{aligned}Q_3 &= -4 \int \frac{e \cdot 2 dx c^{-2x}}{\Delta} + \frac{91}{16} \int \frac{e dx c^{-x}}{\Delta} \\ &\quad + \left\{ \frac{215}{16} \left(\frac{1-e^2}{e} \right)^2 + \frac{175}{16} \left(\frac{1-e^2}{e} \right)^4 + \frac{125}{48} \left(\frac{1-e^2}{e} \right)^6 \right\} \int \frac{e dx c^{-x}}{\Delta} \\ &\quad + c^{-m} \left(\frac{125}{48} \frac{1}{e^5} - \frac{125}{16} \frac{1}{e} + \frac{905}{48} \cdot \frac{1}{e} + \frac{905}{48} e - \frac{125}{16} e^3 + \frac{127}{48} e^5 \right) \\ &\quad - \frac{125}{48} \cdot \frac{1}{e^5} + \frac{125}{48} \frac{1}{e^3} - \frac{305}{48} \frac{1}{e} + \frac{305}{48} e - \frac{125}{48} e^3 + \frac{125}{48} e^5.\end{aligned}$$

The series equivalent to the integrals must now be substituted, in order to express the quantity sought in terms containing the powers of e .

In the first place we have these three terms, each of which is zero when the exact values of A_1 , A_3 , &c. are substituted, viz.

$$\begin{aligned}&\frac{125}{48} (A_1 - 1 + c^{-m}) \cdot \frac{1}{e} \\ &+ \left\{ \frac{175}{16} A_1 + \frac{125}{48} (A_3 - 6 A_1) + \frac{125}{48} - \frac{125}{16} c^{-m} \right\} \cdot \frac{1}{e^3}\end{aligned}$$

$$+ \left\{ \frac{215}{16} A_1 + \frac{175}{16} (A_3 - 4 A_1) + \frac{125}{48} (A_5 - 6 A_3 + 15 A_1) \right. \\ \left. - \frac{305}{48} + \frac{905}{48} c^{-m} \right\} \times \frac{1}{e^5}.$$

The next three terms are as follows:

$$\left\{ -4 a_1 + \frac{91}{16} A_1 + \frac{215}{16} (A_3 - 2 A_1) + \frac{175}{16} (A_5 - 4 A_3 + 6 A_1) \right. \\ \left. + \frac{125}{48} (A_7 - 6 A_5 + 15 A_3 - 20 A_1) + \frac{305}{48} + \frac{905}{48} c^{-m} \right\} \cdot e \\ + \left\{ -4 a_3 + \frac{91}{16} A_3 + \frac{215}{16} (A_5 - 2 A_3 + A_1) \right. \\ \left. + \frac{175}{16} (A_7 - 4 A_5 + 6 A_3 - 4 A_1) \right. \\ \left. + \frac{125}{48} (A_9 - 6 A_7 + 15 A_5 - 20 A_3 + 15 A_1) \right. \\ \left. - \frac{125}{48} - \frac{125}{16} c^{-m} \right\} \cdot e^3 \\ + \left\{ -4 a_5 + \frac{91}{16} A_5 + \frac{215}{16} (A_7 - 2 A_5 + A_3) \right. \\ \left. + \frac{175}{16} (A_9 - 4 A_7 + 6 A_5 - 4 A_3 + A_1) \right. \\ \left. + \frac{125}{48} (A_{11} - 6 A_9 + 15 A_7 - 20 A_5 \right. \\ \left. + 15 A_3 - 6 A_1) \right. \\ \left. + \frac{125}{48} + \frac{125}{48} c^{-m} \right\} \cdot e^5.$$

On substituting the exact values of A_1 , A_3 , &c., these three terms will come out as follows:

$$+ \frac{158}{3} c^{-m} \cdot e, \quad \text{or} + \cdot 00239 \cdot e \\ - \frac{348}{5} c^{-m} \cdot e^3, \quad \text{or} - \cdot 00316 \cdot e^3 \\ + \frac{8891}{75} c^{-m} \cdot e^5, \quad \text{or} + \cdot 00538 \cdot e^5.$$

These three terms are the part of the refraction that depends on the height of the atmosphere: at the horizon, or when $e = 1$, their amount is greatest and equal to

$$f' \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \cdot 00461 = f' \times 726'' \cdot 7 \times \cdot 00461 = f' \times 3'' \cdot 3,$$

which, on account of the smallness of f' , will be a minute fraction of a second.

Rejecting the six foregoing terms, we may assume

$$Q_8 = H_7 e^7 + H_9 e^9 + H_{11} e^{11} + \&c.:$$

and, having computed the differences in the following table,

	Δ^2	Δ^4	Δ^6
A_1	+·0278859
A_3	-·0595755	-·0175110
A_5	+·0873930	-·0177390	-·0199864
A_7	+·0748672	+·0065865	-·0079396
A_9	+·0446024	+·0109256	-·0002312
A_{11}	+·0209241	+·0073251	+·0016762
A_{13}	+·0081714	+·0034934	+·0012515
A_{15}	+·0027438	+·0013379	+·0005925
A_{17}	+·0008096	+·0004339	+·0001891
A_{19}	+·0002133	+·0001224
A_{21}	+·0000509

we shall have

$$H_7 = -4 a_7 + \frac{91}{16} A_7 + \frac{215}{16} \Delta^2 A_5 + \frac{175}{16} \Delta^4 A_3 + \frac{125}{48} \Delta^6 A_1 = \cdot 04861$$

$$H_9 = -4 a_9 + \frac{91}{16} A_9 + \frac{215}{16} \Delta^2 A_7 + \frac{175}{16} \Delta^4 A_5 + \frac{125}{48} \Delta^6 A_3 = \cdot 07091$$

$$H_{11} = -4 a_{11} + \frac{91}{16} A_{11} + \frac{215}{16} \Delta^2 A_9 + \frac{175}{16} \Delta^4 A_7 + \frac{125}{48} \Delta^6 A_5 = \cdot 04469$$

$$H_{13} = -4 a_{13} + \frac{91}{16} A_{13} + \frac{215}{16} \Delta^2 A_{11} + \frac{175}{16} \Delta^4 A_9 + \frac{125}{48} \Delta^6 A_7 = \cdot 00249$$

$$H_{15} = -4 a_{15} + \frac{91}{16} A_{15} + \frac{215}{16} \Delta^2 A_{13} + \frac{175}{16} \Delta^4 A_{11} + \frac{125}{48} \Delta^6 A_9 = -\cdot 02230$$

$$H_{17} = -4 a_{17} + \frac{91}{16} A_{17} + \frac{215}{16} \Delta^2 A_{15} + \frac{175}{16} \Delta^4 A_{13} + \frac{125}{48} \Delta^6 A_{11} = -\cdot 02558$$

$$H_{19} = -4 a_{19} + \frac{91}{16} A_{19} + \frac{215}{16} \Delta^2 A_{17} + \frac{175}{16} \Delta^4 A_{15} + \frac{125}{48} \Delta^6 A_{13} = -\cdot 01835$$

$$H_{21} = -4 a_{21} + \frac{91}{16} A_{21} + \frac{215}{16} \Delta^2 A_{19} + \frac{175}{16} \Delta^4 A_{17} + \frac{125}{48} \Delta^6 A_{15} = -\cdot 01023$$

$$H_{23} = -4 a_{23} + \frac{91}{16} A_{23} + \frac{215}{16} \Delta^2 A_{21} + \frac{175}{16} \Delta^4 A_{19} + \frac{125}{48} \Delta^6 A_{17} = -\cdot 00487.$$

The coefficients of the assumed series being found, and being expressed in seconds of a degree, the part of the refractions depending on Q_8 will be as follows:

$$f' \times \sin \theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3 = f' \times \sin \theta \times \left\{ \begin{array}{l} e^7 \times 35.324, 1.54807^{\log.} \\ + e^9 \times 51.529, 1.71205 \\ + e^{11} \times 32.476, 1.51156 \\ + e^{13} \times 1.809, 0.25755 \\ - e^{15} \times 16.205, 1.20965 \\ - e^{17} \times 18.588, 1.26925 \\ - e^{19} \times 13.334, 1.12498 \\ - e^{21} \times 7.427, 0.87080 \\ - e^{23} \times 3.480, 0.54158 \end{array} \right\}$$

The amount of this expression at the horizon, or when $e = 1$, is $f' \times 62''.1$, almost the same with $f' \times 62''.4$, which, as is shown in § 9, is the limit of the integral when it is extended from $x = 0$ to $x = \infty$. It is thus proved that the error of the series is of no account. This part of the refraction cannot be computed because f' is unknown. But although the precise value of f' is uncertain, it is probably very considerably less than f , or $\frac{2}{9}$; so that the effect on the refraction

cannot exceed a few seconds even at the horizon. We shall be better able to form a just notion with respect to this point, when the Theoretical Table in this paper is compared with observations.

13. It remains to investigate the corrections that must be made in the practical application for the deviations indicated by the meteorological instruments from the mean constants used in constructing the table.

For this purpose we have

$$\begin{aligned} \delta \theta &= \sin \theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times S, \\ S &= Q_0 + \lambda Q_1 - f Q_2, \\ \frac{\sqrt{5i}}{\cos \theta} &= \frac{e}{1-e^2}, \\ \lambda &= \frac{\alpha}{i}. \end{aligned}$$

The quantities e and λ depend only upon α and i : α varies both with the barometer and thermometer, and i , with the thermometer only: the quantity f does not seem liable to change in our climate. Admitting that the prefix d refers only to variations of the barometer and thermometer, we shall have

$$\delta\theta + d.\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \left\{ \left(1 + \frac{d\alpha}{\alpha} - \frac{1}{2} \cdot \frac{di}{i}\right) \cdot S \right. \\ \left. + \frac{de}{e} \cdot \frac{dS}{de} e \right. \\ \left. + \frac{d\lambda}{\lambda} \cdot \lambda Q_1 \right\}.$$

Now

$$\frac{de}{e} = \frac{1}{2} \cdot \frac{di}{i} \cdot \frac{1-e^2}{1+e^2},$$

$$\frac{d\lambda}{\lambda} = \frac{d\alpha}{\alpha} - \frac{di}{i};$$

wherefore,

$$\delta\theta + d.\delta\theta = \delta\theta \left(1 + \frac{d\alpha}{\alpha}\right) \\ - \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot \frac{di}{i} \left(\frac{S}{2} - \frac{1}{2} \cdot \frac{1-e^2}{1+e^2} \cdot \frac{dS}{de} e \right) \\ + \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot \left(\frac{d\alpha}{\alpha} - \frac{di}{i} \right) \cdot \lambda Q_1.$$

If p denote the observed height of the barometer, reduced to the fixed temperature of 50° Fahr.; and τ the temperature of the air on the same scale; then $\beta = \frac{1}{480}$,

$$1 + \frac{d\alpha}{\alpha} = \frac{1}{1 + \beta(\tau-50)} \cdot \frac{p}{30},$$

$$\frac{d\alpha}{\alpha} = - \frac{\tau-50}{480} - \frac{30-p}{30},$$

$$\frac{di}{i} = + \frac{\tau-50}{480},$$

$$\frac{d\alpha}{\alpha} - \frac{di}{i} = - 2 \times \frac{\tau-50}{480} - \frac{30-p}{30}.$$

These values being found, if we put

$$T = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \frac{1}{480} \times \left(\frac{(1+e^2)S - (1-e^2) \frac{dS}{de} e}{2(1+e^2)} + 2\lambda Q_1 \right)$$

$$b = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \frac{2\lambda Q_1}{30};$$

the expression of the mean refraction with its correction will be as follows,

$$\delta\theta + d.\delta\theta = \frac{\delta\theta}{1 - \beta(\tau - 50)} \cdot \frac{p}{30} - T \cdot (\tau - 50) - b(30 - p).$$

The first term of this expression is the mean refraction corrected in the manner usually practised by astronomers. If we assume that the temperature of the mercury in the barometer is the same with that of the air, this term will be equal to

$$\frac{1}{1 + \beta(\tau - 50)} \cdot \frac{1}{1 + \frac{\tau - 50}{10000}} \cdot \frac{p}{30} = \frac{1}{1 + c(\tau - 50)} \cdot \frac{p}{30},$$

$$c = \cdot 002183,$$

the new factor being added to compensate the expansion of the mercury. Two subsidiary tables are given for computing this part: Table II. contains the logarithms of

$\frac{1}{1 + c(\tau - 50)}$ for 30° on either side of the mean temperature 50° , negative indices being avoided by substituting the arithmetical complements; and Table III. contains the logarithms, or the arithmetical complements, for all values of p from 31 to 28.

The coefficients, T and b , of the other two terms vary with the distance from the zenith; and they can be computed in no other way than by reducing them to series of the powers of e . By substituting for λQ_1 , the equivalent series already known, we immediately obtain

$$b = \sin\theta \cdot \frac{\alpha(1 + \alpha)}{\sqrt{5i}} \cdot \frac{1}{30} \cdot \left\{ B_3 e^3 + B_5 e^5 + B_7 e^7 + \&c. \right\}.$$

Further, by expanding S and its differential, the expression of T will take this form,

$$T = \sin\theta \cdot \frac{\alpha(1 + \alpha)}{\sqrt{5i}} \cdot \frac{1}{480} \cdot \left\{ G_3 e^3 + G_7 e^7 + G_9 e^9 + \&c. \right\};$$

and we shall have

$$G_3 = A_1 - A_3 + 2 B_3 = 0\cdot 2436$$

$$G_5 = -A_1 + 3 A_3 - 2 A_5 + 2 B_5 = 0\cdot 4523$$

$$G_7 = A_1 - 3 A_3 + 5 A_5 - 3 A_7 + 2 B_7 = 0\cdot 4705$$

$$G_9 = -A_1 + 3 A_3 - 5 A_5 + 7 A_7 - 4 A_9 + 2 B_9 = 0\cdot 3502$$

$$G_{11} = A_1 - 3 A_3 + 5 A_5 - 7 A_7 + 9 A_9 - 5 A_{11} + 2 B_{11} = 0\cdot 2092$$

$$G_{13} = -A_1 + 3 A_3 - 5 A_5 + 7 A_7 - 9 A_9 + {}^{11}A_{11} - 6 A_{13} + 2 B_{13} \\ = 0\cdot 1050.$$

The series for T and b being now known, the coefficients of

the terms must next be expressed in seconds of a degree, which being done, the following final results will be obtained.

$$T = \sin \theta \times \begin{cases} e^3 \cdot 0''369, & -1.5668, \\ +e^5 \cdot 0.685, & -1.8356 \\ +e^7 \cdot 0.712, & -1.8526 \\ +e^9 \cdot 0.530, & -1.7263 \\ +e^{11} \cdot 0.317, & -1.5006 \\ +e^{13} \cdot 0.159, & -1.2013 \end{cases} \quad b = \sin \theta \times \begin{cases} e^3 \cdot 0.530, & -1.7240 \\ +e^5 \cdot 1.113, & 0.0465 \\ +e^7 \cdot 1.350, & 0.1306 \\ +e^9 \cdot 1.207, & 0.0817 \\ +e^{11} \cdot 0.873, & 1.9412 \\ +e^{13} \cdot 0.539, & -1.7313 \end{cases}$$

The values of T and b are added in separate columns of the annexed table for altitudes less than 10° : for greater altitudes they are omitted as of no account. The application for finding the corrected refraction from the formula

$$\delta\theta + d \cdot \delta\theta = \frac{\delta\theta}{1 + c(\tau - 50)} \cdot \frac{p}{20} - T(\tau - 50) - b(30 - p),$$

will best be explained by the examples afterwards given.

14. The Theoretical Table of refractions which has been computed by the foregoing formulas, and which is deduced solely from the phænomena of the atmosphere without arbitrary assumptions, is next to be compared with the tables most esteemed by astronomers. Two tables more eminently deserve this character; namely, Bessel's table with its supplement in the *Tabulæ Regiomontanæ*, which may be considered as the result of observations, and as being nearly exact to 88° or $88\frac{1}{2}^\circ$ from the zenith; and the table published annually in the *Connaissance des Temps*. As all the tables are supposed to contain the same series of refractions, the numbers corresponding to the same altitude should have constantly the same proportion: so that taking the number α which answers to the zenith-distance θ in Bessel's table, the logarithm of the refraction at the same zenith distance in the new table should be equal to

$$\log \alpha + \log \tan \theta + .00507,$$

the number .00507 being the difference of the logarithms of the refractions at the altitude of 45° in the two tables: but, in the supplemental table, which contains the logarithms of the refractions, it is sufficient to add .00507 to obtain the logarithms in the new table. With regard to the refractions in the *Conn. des Temps*, it is more convenient to use the table in the *Tables Astronomiques*, published by the French Board of Longitude: for the logarithms in this table with the addition of .0011, should agree respectively with the logarithms of the new table. According to these directions the following comparative view has been drawn up.

Zenith dist.	Reflections.		
	New Table.	Tab. Reg.	Conn. des Temps.
0°	"	"	"
10	10.30	10.30	10.30
20	21.26	21.26	21.26
30	33.72	33.72	33.72
40	48.99	48.99	48.99
45	58.36	58.36	58.36
50	69.52	69.52	69.52
55	83.25	83.24	83.25
60	100.85	100.85	100.86
65	124.65	124.62	124.65
70	159.16	159.11	159.22
75	214.70	214.58	214.83
80	320.19	319.88	320.63
81	353.79	353.38	354.33
82	394.68	394.20	395.37
83	445.42	444.86	445.87
84	509.86	509.23	511.22
85	593.96	593.38	595.80
85½	646.21	647.10	648.34
86	707.43	707.15	710.07
86½	779.92	777.36	783.07
87	866.76	864.59	870.37
87½	971.93	972.21	975.89
88	1101.35	1101.40	1105.1
88½	1262.6	1265.5	1265.0
89	1466.8	1481.8	1464.9
89½	1729.5	1764.9	1716.4

From this view it appears that the three tables agree within less than 1" as far as 80° from the zenith; the new table is in accordance with Bessel's, with slight discrepancies, to 88° or 88½° from the zenith; from 80° to 88° of zenith distance the numbers in the French table exceed those in Bessel's, the excess being 2" at 84°, and 4" at 88°. But when the distance from the zenith is greater than 80°, the accuracy of the French table is questionable, both on account of the hypothetical law of the densities, and because the quantity assumed for the horizontal refraction is uncertain.

A few examples are subjoined, as well for explaining the use of the new table as for affording some indications of its accuracy at low altitudes. The two first instances are taken from the *Tables Astronomiques*, and are likewise published yearly in the *Conn. des Temps*.

EXAMPLE 1.		EXAMPLE 2.	
$\theta = 86^{\circ} 14' 42''$		$\theta = 86^{\circ} 15' 20''$	
Therm. $8^{\circ}.75$ cent. = $47^{\circ}.75$ F.		Therm. $8\frac{1}{2}^{\circ}$ cent. = $46^{\circ}.9$ F.	
Barom. $0^m.741 = 29.17$ in.		Barom. $0^m.766 = 30.16$ in.	
$86^{\circ} 10'$	2.86345	$86^{\circ} 10'$	2.86345
$4\ 42''$	664	$5\ 20''$	753
	2.87009		2.87098
Therm.00214	Therm.00276
Barom.	9.98781	Barom.00232
Log $\delta \theta$	2.86004	Log $\delta \theta$	2.87606
$\delta \theta$	724.5	$\delta \theta$	751.7
$-.25 \times -.2\frac{1}{4}$	+ .5	$-.25 \times -.3.1$	+ .8
$.4 \times .8$	-.3	$-.4 \times -.16$	+ .6
Corrected refraction	12' 4".7	Corrected refraction	12' 33".1
Observed refraction	12 4 .2	Observed refraction	12 32 .5
EXAMPLE 3.		EXAMPLE 4.	
Mean of 42 sub-polar observations of α Lyræ by Dr. BRINKLEY. Irish Transactions, 1815.		Mean of 10 observations of Capella, from a memoir of M. PLANA. Acad. de Turin, tom. 32.	
$\theta = 87^{\circ} 42' 10''$		$\theta = 88^{\circ} 24' 9''.7$	
Therm. 35°		Therm. $47^{\circ}.75$	
Barom. 29.5		Barom. 29.75	
$87^{\circ} 40'$	3.00522	$88^{\circ} 20'$	3.08087
$2\ 10''$	392	$4\ 9''.4$	847
	3.00914		3.08934
Therm.01444	Therm.00214
Barom.	9.99270	Barom.	9.99607
Log $\delta \theta$	3.01628	Log $\delta \theta$	3.08755
$\delta \theta$	1038".2	$\delta \theta$	1223".3
$-.6 \times -.15$	+ 9.0	$-.95 \times -.2\frac{1}{4}$	+ 2.1
$-1.13 \times \frac{1}{2}$	- 0.6	$-1.6 \times .27$	- 0.4
Corrected refraction	17' 26".6	Corrected refraction	20' 25"
Observed refraction	17 26 .5	Observed refraction	20 24 .3

We may now inquire how far the refractions are likely to be affected by the term which it was found necessary to leave out, because the present state of our knowledge of the phenomena of the atmosphere made it impossible to determine the coefficient f'' by which it is multiplied. For this purpose the term alluded to, viz.

$$\sin \theta \times f' \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3,$$

which may be shortly denoted by $f' \times \chi(\theta)$, has been computed by means of the equivalent series, for every half degree between 85° and 88° , the results being as follows:

θ	$f' \times \chi(\theta)$
85°	$f' \times 1.5$
$85\frac{1}{2}$	$f' \times 2.0$
86	$f' \times 3.3$
$86\frac{1}{2}$	$f' \times 4.9$
87	$f' \times 7.4$
$87\frac{1}{2}$	$f' \times 11.2$
88	$f' \times 17.0$

From this view it appears that f' , although considerably less than f or $\frac{2}{9}$ may still have some influence on the refractions at very low altitudes. The mean refraction in Bessel's table, and in the new table, can hardly be supposed to differ $2''$ from the true quantity, which would limit f' to be less than $\frac{1}{10}$. It is a matter of some importance to obtain a near value of f' : and it is probable that this can be accomplished in no other way but by searching out such values of f and f' as will best represent many good observed refractions at altitudes less than 5° . If such values were found, our knowledge of the decrease of heat in ascending in the atmosphere would be improved, and the measurement of heights by the barometer would be made more perfect.

April 25, 1838.

TABLE I.

Mean Refractions for the Temperature 50° Fahrenheit, and the barometric Pressure 30 inches.

Zenith dist.	$\delta \theta$	Log $\delta \theta$	Diff.	T.	C.	Zenith dist.	$\delta \theta$	Log $\delta \theta$	Diff.	T.	C.
1°	$0' 1.02$	0.0085				7°	$0' 7.17$	0.8557			
2	2.04	0.3097	3012			8	8.21	0.9144	587		
3	3.06	0.4860	1763			9	9.25	0.9663	519		
4	4.08	0.6112	1252			10	10.30	1.0129	466		
5	5.11	0.7086	974			11	11.35	1.0553	424		
6	6.14	0.7882	796			12	12.42	1.0941	388		
7	7.17	0.8557	675			13	13.49	1.1300	359		

TABLE I. (continued).

Zenith dist.	$\delta \theta$.	Log $\delta \theta$.	Diff.	T.	C.	Zenith dist.	$\delta \theta$.	Log $\delta \theta$.	Diff.	T.	C.
13	0' 13.49	1.1300				57	1' 29.73	1.95291			
14	14.57	1.1634	334			58	33.23	1.96955	1664		
15	15.65	1.1947	313			59	36.93	1.98646	1691		
16	16.75	1.2241	294			60	40.85	2.00368	1722		
17	17.86	1.2519	278			61	45.01	2.02124	1756		
18	18.98	1.2784	265			62	49.44	2.03918	1794		
19	20.11	1.3036	252			63	54.17	2.05754	1836		
20	21.26	1.3277	241			64	59.23	2.07635	1881		
21	22.42	1.3507	230			65	2 4.65	2.09567	1932		
22	23.60	1.3729	222			66	10.48	2.11555	1988		
23	24.80	1.3944	215			67	16.78	2.13603	2048		
24	26.01	1.4151	207			68	23.61	2.15719	2116		
25	27.24	1.4352	201			69	31.04	2.17910	2191		
26	28.49	1.4547	195			70	00 39.16	2.20186	2275		
27	29.75	1.4736	189			10	40.59	2.20573	388		
28	31.05	1.4921	185			20	42.04	2.20963	390		
29	32.38	1.5102	181			30	43.52	2.21356	393		
30	33.72	1.5279	177			40	45.02	2.21752	396		
31	35.09	1.5452	173			50	46.53	2.22150	398		
32	36.49	1.5622	170			71	00 48.08	2.22552	402		
33	37.93	1.5790	168			10	49.65	2.22956	404		
34	39.39	1.5954	164			20	51.25	2.23363	407		
35	40.89	1.6115	162			30	52.87	2.23773	410		
36	42.42	1.6276	160			40	54.53	2.24186	413		
37	44.00	1.6435	159			50	56.21	2.24603	417		
38	45.61	1.6591	156			72	00 57.92	2.25022	419		
39	47.27	1.6746	155			10	59.66	2.25445	423		
40	48.99	1.6901	155			20	3 1.43	2.25870	425		
41	50.75	1.7055	154			30	3.23	2.26299	429		
42	52.57	1.7207	152			40	5.06	2.26732	433		
43	54.43	1.7358	151			50	6.93	2.27168	436		
44	56.35	1.7510	152			73	00 8.83	2.27608	440		
45	58.36	1.76611	151			10	10.77	2.28051	443		
46	1 0.43	1.78123	1512			20	12.74	2.28498	447		
47	2.57	1.79637	1514			30	14.75	2.28948	450		
48	4.80	1.81155	1518			40	16.80	2.29402	454		
49	7.11	1.82678	1523			50	18.88	2.29860	458		
50	9.52	1.84208	1530			74	00 21.01	2.30322	462		
51	12.02	1.85747	1539			10	23.18	2.30789	467		
52	14.64	1.87298	1551			20	25.39	2.31259	470		
53	17.38	1.88863	1565			30	27.66	2.31734	475		
54	20.24	1.90440	1577			40	29.95	2.32213	479		
55	23.25	1.92036	1596			50	32.30	2.32696	483		
56	26.41	1.93653	1617			75	00 34.70	2.33184	488		
57	29.73	1.95291	1638			10	37.16	2.33677	493		

TABLE I. (continued).

Zenith dist.	$\hat{c} \theta$	Log $\hat{c} \theta$	Diff.	T.	C.	Zenith dist.	$\hat{c} \theta$	Log $\hat{c} \theta$	Diff.	T.	C.
5 10 3	37.16	2.33677	497			82 40	7 7.19	2.63062	899		
20	39.65	2.34174	502			50	16.13	2.63961	916	.07	.11
30	42.21	2.34676	507			83 00	25.42	2.64877	833		
40	44.82	2.35183	512			10	35.09	2.65810	948		
50	47.48	2.35695	517			20	45.14	2.66758	970		
6 00	50.21	2.36212	523			30	55.64	2.67728	985		
10	53.00	2.36735	528			40	8 6.55	2.68713	1005		
20	55.85	2.37263	533			50	17.95	2.69718	1028		
30	58.76	2.37796	538			84 00	29.86	2.70746	1047	.10	.16
40 4	1.74	2.38334	545			10	42.31	2.71793	1069		
50	4.79	2.38879	551			20	55.33	2.72862	1092		
7 00	7.91	2.39430	557			30	9 8.96	2.73954	1116		
10	11.11	2.39987	563			40	23.25	2.75070	1140		
20	14.39	2.40550	569			50	38.23	2.76210	1166		
30	17.74	2.41119	576			85 00	53.96	2.77376	1194	.15	.24
40	21.19	2.41695	583			10 10	10.52	2.78570	1219		
50	24.72	2.42278	589			20	27.90	2.79789	1248		
8 00	28.33	2.42867	596			30	46.21	2.81037	1281		
10	32.04	2.43463	603			40 11	5.55	2.82318	1312		
20	35.84	2.44066	611			50	25.90	2.83626	1342		
30	39.75	2.44677	618			86 00	47.43	2.84968	1377	.24	.39
40	43.76	2.45295	626			10 12	10.21	2.86345	1412		
50	47.88	2.45921	635			20	34.34	2.87757	1448		
9 00	52.12	2.46556	642			30	59.92	2.89205	1488	.31	.51
10	56.47	2.47198	650			40 13	27.11	2.90693	1627		
20 5	0.94	2.47848	659			50	55.99	2.92220	1570		
30	5.54	2.48507	669			87 00 14	26.76	2.93790	1612	.39	.67
40	10.28	2.49176	677			10	59.54	2.95402	1658	.43	.75
50	15.16	2.49853	688	.03	.04	20 15	34.55	2.97060	1704	.47	.83
0 00	20.19	2.50541	696			30 16	11.93	2.98764	1758	.52	.91
10	25.36	2.51237	707			40	52.10	3.00522	1808	.58	1.01
20	30.70	2.51944	716			50 17	35.12	3.02330	1862	.63	1.13
30	36.20	2.52660	727			88 00 18	21.35	3.04192	1918	.69	1.24
40	41.88	2.53387	738			10 19	11.07	3.06110	1977	.78	1.41
50	47.74	2.54125	749	.04	.05	20 20	4.68	3.08087	2040	.87	1.58
1 00	53.79	2.54874	761			30 21	2.60	3.10127	2102	.96	1.75
10 6	0.04	2.55635	772			40 22	5.22	3.12229	2169	1.07	2.00
20	6.50	2.56407	785			50 23	13.11	3.14398	2239	1.19	2.24
30	13.18	2.57192	797			89 00 24	26.8	3.16637	2316	1.32	2.48
40	20.09	2.57989	811			10 25	46.8	3.18943	2388	1.52	2.91
50	27.26	2.58800	824	.05	.08	20 27	14.2	3.21331	2461	1.72	3.34
2 00	34.68	2.59624	838			30 28	49.5	3.23792	2529	1.92	3.77
10	42.37	2.60462	851			40 30	33.2	3.26321	2573	2.20	4.34
20	50.33	2.61313	866			50 32	15.1	3.28894		2.48	5.00
30	58.59	2.62179	883			90 00 34	32				
40 7	7.19	2.63062									

TABLE II.

Thermometer.					
	Log.	Diff.		Log.	Diff.
50	0.00000		50	0.00000	
49	0.00094		51	9.99906	
48	0.00190		52	9.99811	
47	0.00285		53	9.99717	
46	0.00380		54	9.99623	
45	0.00476	96	55	9.99529	
44	0.00572		56	9.99434	94
43	0.00668		57	9.99341	
42	0.00764		58	9.99248	
41	0.00861		59	9.99154	
40	0.00957		60	9.99061	
39	0.01053		61	9.98969	
38	0.01151	98	62	9.98875	
37	0.01248		63	9.98783	92
36	0.01346		64	9.98690	
35	0.01444		65	9.98598	
34	0.01541		66	9.98506	
33	0.01640		67	9.98414	
32	0.01738		68	9.98323	
31	0.01837		69	9.98231	
30	0.01935		70	9.98140	
29	0.02033		71	9.98049	
28	0.02133	100	72	9.97958	
27	0.02232		73	9.97867	
26	0.02331		74	9.97777	90
25	0.02432		75	9.97686	
24	0.02531		76	9.97596	
23	0.02630		77	9.97506	
22	0.02730		78	9.97416	
21	0.02832	102	79	9.97326	
20	0.02933		80	9.97237	

TABLE III.

Barometer.		
	Log.	Diff.
In.		
31	0.01424	
30.9	0.01248	
8	0.01143	
7	0.01002	142
6	0.00860	
5	0.00718	
4	0.00575	
3	0.00432	144
2	0.00289	
1	0.00145	
30.0	0.00000	
29.9	9.99855	
8	9.99709	
7	9.99563	
6	9.99417	146
5	9.99270	
4	9.99123	
3	9.98975	148
2	9.98826	
1	9.98677	
29.0	9.98528	150
28.9	9.98378	
8	9.98227	
7	9.98076	
6	9.97924	
5	9.97772	152
4	9.97620	
3	9.97466	
2	9.97313	
1	9.97158	
28.0	9.97004	154

XVII. *Letter to Richard Taylor, Esq., with reference to two Papers in the Philosophical Magazine for January, 1840.*
By JAMES D. FORBES, Esq., F.R.S.S., L. & Ed., &c.

MY DEAR SIR,

IN order that your readers may be aware why I do not answer Mr. Potter's animadversions in the January Number of the Philosophical Magazine, I would request you to insert the following letter which I addressed to him after reading his communication.

"MY DEAR SIR,

"Edinburgh, Jan. 8th, 1840.

"I have read with some pain, and at least equal surprise, some observations by you on photometry, stated to have been

occasioned by reading a brief notice of some experiments of mine, with respect to *heat*, which I thought might, perhaps, bear some analogy to the case of *light*. It is evident, however, that as it would be impossible to predicate beforehand that heat and light are reflected according to the same law, any verification of Fresnel's law for light obtained in this way could only be an analogical one, and therefore acceptable only whilst photometric methods are so very imperfect as I still consider them to be, however dexterously employed.

"It was a matter, as I say, of equal surprise and pain to me to find that you should have so gratuitously misinterpreted my sentiments towards you, which I deliberately declare to you were solely those of entire friendship and respect. The object of this letter is simply to assure you of this, and if I could do it in stronger terms, I would.

"It seems to be strange and almost incredible that one whose experiments I have so often quoted with respect, whose results I have made known, and whose originality in the matter of metallic reflection I have so often vindicated at home and abroad, in private conversation and in public lectures, should take a pleasure in misinterpreting my expressions. I am persuaded that at some future time you will do me justice, and in the mean time I will rather run the risk of sustaining any prejudice which your letter may excite against my experiments until they appear to speak for themselves, than enter into a public disputation about statements and expressions, to the certain loss both of time and temper. I mean to write to Mr. Taylor to this effect, and shall perhaps communicate to him the substance of this letter.

"I am, my dear Sir, yours very truly,
"JAMES D. FORBES."

Now, Sir, after this statement it is not my intention to enter into any defence of the "memorandum" inserted in your Journal for December (*L. & E. Phil. Mag.* vol. xv. p. 479.). I shall correct neither Mr. Potter's statements nor his inferences, which so far as they relate to myself are certainly unfounded. Had I been at all aware of the extreme importance which Mr. Potter attributes to the particular experiments on photometry to which I alluded, I should certainly have done so with far greater caution. I imagined that Mr. Potter probably considered (as I think I would have done under the circumstances) his earlier contributions to physical science as subject to the revision of his own maturer skill and judgement, and, until they had received that revision, as open to some doubt; the subject being one of such difficulty, that if Mr. Potter failed, he failed

only in common with perhaps every other person who had attempted it. In this supposition it appears that I was mistaken. I have since learned from Mr. Potter himself that he considers the true measure of light as more attainable than that of heat. A proposition so startling, and which is at variance with all that I have ever heard expressed, or should have been disposed to conclude upon the subject, I could not be expected to anticipate, and therefore a collision of opinion, though to be regretted, was unavoidable.

Before concluding, I have a single observation to offer upon Mr. Warington's interesting communication on Nobili's coloured rings. If Mr. Warington will refer to your Number for July last, page 27, *note*, (L. & E. Phil. Mag. vol. xv.) he will find the following remark: "The explanation of these colours, by supposing with the philosopher of Reggio (if I understand him aright) that they are produced by thin plates of *adhering oxygen gas*, is too evidently founded in error to require any notice." I may now add the consideration which seemed to me so conclusive, which is not a chemical but an optical one. The colours of thin plates are on all hands admitted to be produced by the interference of the light reflected at their first and second surfaces. In the present case the first surface would be the common boundary of air and oxygen gas, which can neither be considered as a sharp mathematical surface, nor if it could, would there be any appreciable quantity of light reflected from the boundary of substances having scarcely an appreciable difference of refractive power, much less could such intensely vivid colours be the result. This is but one of many palpable oversights in a paper, which, whatever may be its value to artists, seems unworthy of the scientific reputation usually given to Nobili, and in which notwithstanding, he speaks with very little respect of the reasonings of Newton and Berzelius.

I am, my dear Sir, yours very truly,

Edinburgh, Jan. 21, 1840.

JAMES D. FORBES.

☞ We regret to find that we have incurred blame on account of some expressions in Mr. Potter's paper on Photometry in our preceding number, implying a charge of unfairness in the treatment of scientific questions against the Cambridge Philosophical Society, and which are complained of as being, "under the form of a scientific communication, an irrelevant and most unjust attack upon a public body." We freely admit the justice of the remark of a correspondent, that the editors of a scientific journal should avoid giving currency to imputations of this kind; and can only state, that had the nature of the charge, and the tone of some other expressions, caught our attention, we should have objected to its admission in the form in which we received it. We can safely appeal to the spirit in which our work has long been conducted in proof of our wish not to occupy its pages with personal imputations, or with the remonstrances to which they necessarily give rise.

—EDIT.

XVIII. *Observations on the Blood Corpuscles, or Red Particles, of the Mammiferous Animals.* By GEORGE GULLIVER, F.R.S., Assistant Surgeon to the Royal Regiment of Horse Guards. No. II.*

IN my former communication† I omitted to mention that in many of the observations an achromatic object glass was used, of one tenth of an inch focal length, made by Powell, and adapted to the same eye-piece as the excellent object-glass by Ross. They both perform admirably, and the additional power gained by the former one is not only of considerable advantage, but it affords an opportunity of instituting comparative trials, so as to diminish the chances of error. Both these glasses will therefore be employed in the succeeding observations, and I shall avail myself of opportunities of testing the measurements previously given, and of recording the results when they seem to be of any consequence. The magnifying power of Powell's glass with the micrometer eye-piece is as nearly as possible nine hundred and eighty diameters, and the object is very distinctly defined.

After repeating very carefully numerous observations on the corpuscles in their own serum, as compared with specimens dried in the manner formerly described, it appears that the latter are almost always a little larger and more accurately defined in the outline than the former. This is particularly observable at the margins of the dried preparations, where the corpuscles are very thinly spread, and where desiccation takes place instantaneously when they are applied warm from the wound to the glass. Towards the centre, as the particles are more thickly aggregated, they do not dry so quickly, but have time to contract a little, and accordingly correspond in diameter pretty accurately with those observed in their own serum. I have noted instances in which they manifestly shrunk while under examination in the serum, as if they were acted on by the glasses between which they were placed, the edges of the disks becoming more rounded, occasionally granulated, and not unfrequently puckered or swollen, so that the central concavity in many of them was very remarkable, and often more or less misshapen from the bulging of the edges towards the centre, a triangular depression with concavity of the margins being thus occasionally produced on the surface of the corpuscles.

Though saline solutions are useful in diluting the blood for comparative observations, measurements from corpuscles so preserved are not worthy of much reliance. The shrinking,

* Communicated by the Author. † L. & E. Phil. Mag. for January 1840.

or the alteration of form in the disks, may generally be observed even in the course of a minute or two after the mixture. Nor is the serum of one animal always proper to dilute the blood of another; for I could seldom get the corpuscles of the carnivora, or even of some ruminants, to mix well with the serum of the horse.

If obtained from the body a day or two after death, the disks are generally so clustered together as to be seen very indefinitely in the wet state, although some of the smallest detached from the masses are often tolerably distinct. The corpuscles are mostly very irregular in size, approaching more to the spherical shape, and even more susceptible of alteration from any of the common methods of dilution, than in blood procured from the living animal. By drying, however, a tolerably clear outline of the disks from the carcass may in most instances be procured, although every method sometimes fails, as I experienced a short time since in some blood from the Sloth Bear and from the Malay Sun Bear. Though the bodies of these animals were perfectly fresh, and the masses of fibrine in the heart and great vessels firmly coagulated, the particles of the blood were so much conglomerated, and their size so singularly variable, that it was impossible either by drying or any method of dilution to obtain even an approximation to their average diameter; and yet some corpuscles procured from the living Sloth Bear did not exhibit such irregularities.

Of the accidental circumstances by which the particles are liable to become enlarged, besides incipient putrefaction, the moisture of the atmosphere, of the breath, or of the hand are the most frequent. Dried or drying specimens are thus instantly injured or destroyed, the disks being more or less altered in shape and deprived of their colouring matter. But much depends on the degree in which these causes may have acted; for a diminution in the magnitude of the corpuscles may be the consequence. If, for example, some water be mixed with blood, the disks immediately become much enlarged and spherical, quickly losing their colouring matter; and yet if the whole of this be thus removed, after a while the outlines of the disks, very faint indeed, may frequently be recognised, diminished considerably in diameter and apparently quite flat. They may always be clearly seen by treating them with a strong solution of corrosive sublimate. The human blood corpuscles, thus enlarged at first, and then deprived of their colouring matter, and reduced in size, generally present a diameter of about $\frac{1}{4800}$ th of an inch, whether detected in the pure water or rendered more apparent by the sublimate.

They have a very characteristic appearance, being remarkably flat and pellucid, several generally touching at their edges so as to form groups, scarcely ever turning over or even moving in the fluid. It is obvious from the size, shape, and general appearance of these particles, that they are not identical with those which have been usually described as the nuclei of the blood corpuscles. The average diameter of the disks in the first instance was 1-3429th of an inch.

Besides the precaution concerning the preservation of the blood, it is equally necessary to be careful as to how it is obtained. The corpuscles are more or less modified very quickly after extravasation; and if some delay occur, and the drop of blood, effused into the subcutaneous tissue, require pressure to determine it to the surface, the corpuscles in that blood may be expected to be irregular in form and magnitude, many of them particularly presenting a granulated appearance. Hence the glass should not be pressed upon the wound, but merely lightly touched on a drop of the blood, however small, that has appeared freely immediately after the puncture. A specimen thus procured, and dried as before noticed, will be excellent, although in all cases where it is practicable a small incision directly into a superficial vein will be preferable.

The granulated particles are almost uniformly smaller than the common disks, and it is not improbable that some of the former may be produced by the irregular shrinking of the latter. In some instances I could not detect any of the granulated corpuscles in the blood immediately after it was taken from the animal, although they were to be seen abundantly after a few hours' exposure in the serum to the atmosphere, the temperature ranging between 45° and 50° . In one observation some of the extremely minute spherules, which are not uncommon in the blood, were observed to attach themselves to a few of the smaller disks, so as to produce the granulated appearance.

The blood corpuscles therefore are so singularly susceptible of variation in size and form from the operation of very slight agency, that there is probably no other microscopic object of equal delicacy, or that requires so much experience in its management. Hence it is not surprising that cursory observers should have committed remarkable errors, and that the history of the blood corpuscles, even after the labours of more careful inquirers, should have been so much obscured by discrepancies, particularly when it is considered how the inherent difficulties of the subject have been increased by the imperfection of instruments. Hewson indeed, whose obser-

ventions apart from his hypotheses are generally remarkable for their accuracy, laboured under these disadvantages, but there is reason to believe that his results were not obtained without the most devoted and diligent inquiry.

The discrepancies just alluded to are frequently adduced as instances of how little credit should be attached to microscopical anatomy generally. There does not appear to me to be any reason in the objection; first, because it is seldom if ever urged by those who are sufficiently acquainted with the instrument to enable them to judge fairly of its use; and secondly, the same objection might in like manner be made to the use of the unassisted vision in minute anatomy. Whoever has attended to the history of the elementary structure of different parts in the animal œconomy, as given by anatomists who have not employed glasses, must be acquainted with differences in the observations, just as remarkable as those which have resulted from the use of the microscope. But no one ever yet ventured to suggest that the imperfection of our senses was a reason why they should be dispensed with. The intimate structure of the bones, of the cellular or adipose tissues, are, among others, singularly obscured by false observations, notable for their number as well as for their disagreement. Yet the microscope has made these things infinitely more simple. No anatomist would now require to found the distinctions between the cellular and adipose tissues chiefly on remote physiological phænomena; no one would doubt the difference who had once seen the vesicles of the latter by the aid of the microscope. Haller could thus have immediately seen and shown to others the proof of that great discovery, which required so much labour to demonstrate. A curious collection might be given of the errors of anatomical observations made by the unaided vision as compared with those which have arisen from the use of the microscope*. But this subject is foreign to my purpose, and I have only to repeat the conviction which I have elsewhere expressed†, that the minute anatomy of the fluids, both healthy and diseased, is of the utmost importance, and that the steady and successful pursuit of this object will ultimately be the foundation of a new æra both in physiology and pathology.

76. Orang-outang, (*Pithecus Satyrus*,) a female, about a third grown. All the following sizes very common: 1-3552nd,

* On the limits of vision with the best instruments there is an ingenious paper by Ehrenberg, of which we are indebted to Mr. Francis for an excellent English version. See Taylor's Scientific Memoirs, Part iv.

† On the Softening of Fibrine, Med. Chir. Trans., vol. xxii.

1-3429th, 1-3368th, 1-3309th, and 1-3200th. Extreme diameters 1-4000th and 1-3000th, though a very few considerably smaller might be seen in the serum. Blood from a vein of the fore arm.

77. Hoolock Gibbon, (*Hylobates scyrites*,) nearly full-grown female. Most frequent size of the corpuscles 1-3200th, but very variable from 1-4570th to 1-2782nd of an inch. Blood from the left ventricle, dried, three days after death.

78. White Whiskered Gibbon, (*Hylobates* ——— ?) a male, about two-thirds grown. Most frequent diameters of corpuscles 1-3428th and 1-3200th. Extreme sizes 1-4570th and 1-2900th. In serum 1-4000th common; in weak saline solutions still smaller, and many of the edges swelled and punctured, so as to form a very remarkable triangular or quadrangular depression in the centre of the disks. Blood from the left ventricle of the heart.

I am informed by Mr. Ogilby that this is a new species of *Hylobates*.

79. Magot or Barbary Ape, (*Papio sylvanus*), 1-3428th and 1-3200th most frequently. Extreme sizes 1-4570th and 1-2900th. Blood from the aorta and vena cava.

80. Rhesus Monkey, (*Macacus rhesus*), a young male. Corpuscles very variable in size, 1-3200th very common; extreme diameters 1-4000th and 1-2666th of an inch. Blood from left ventricle of the heart.

81. Toque or Chinese Bonnet Monkey, (*Macacus radiatus*), a male, about two-thirds grown. 1-3600th and 1-3200th common size of the corpuscles. Blood from the left ventricle.

82. Black Ape, (*Macacus niger*), full-grown male. Most common diameter of the disks 1-3554th of an inch; extreme sizes 1-4572nd and 1-2965th. Blood from right ventricle.

83. Hare-lipped Monkey, (*Macacus cynomolgus*), full-grown male. Most common diameter of the disks 1-3429th of an inch; extreme sizes 1-4500th and 1-2666th. Blood from the renal vein.

84. Pigtailed Monkey, (*Papio nemestrinus*), a female two-thirds grown. Most common diameter of disks 1-3329th and 1-3555th. Extreme sizes 1-4570th and 1-2900th of an inch. Blood from the portal vein, from the coronary veins, and from the right ventricle.

85. Jacchus Monkey, (*Jacchus vulgaris*), adult male. Four measurements of three or four in a row, gave the following diameters of each disk: 1-3552nd, 1-3554th, 1-3693rd, and 1-3555th. Extreme diameters 1-4570th and 1-3000th. Blood from a prick of the tail.

In the venous blood of the monkey tribe, besides lymph globules of the common size and appearance, there are generally spherical bodies of a very white colour, and frequently of semi-fluid consistence, as may be inferred from their being seen occasionally to alter in shape, like a drop of any viscid matter subjected to currents of particles on its margins. The white round bodies vary in diameter from 1-4000th to 1-1777th of an inch. Their number is often very great, and they may be observed very remarkably in blood from the mesenteric veins. In the blood of the right ventricle they are also extremely common, though the semi-fluid appearance is more frequently seen in the former. These observations were made on monkeys, apes, and baboons, dead of various diseases, chiefly tubercular phthisis. Tubercle seems in these animals to be as common in the spleen as in the lungs; indeed, this deposit is not unfrequent in the former organ when none can be found in the latter.

86. European Brown Bear, (*Ursus Arctos*,) female, hardly a third grown. Most frequent diameters of corpuscles 1-3600th, 1-3692nd, 1-3750th, 1-3790th, and 1-4000th. Extreme sizes 1-4570th and 1-3048th. Blood from a prick of the upper lip.

87. Black Bear, (*Ursus Americanus*,) a male, nearly same size and apparently of the same age as the European Bear. All the following diameters common: 1-3600th, 1-3693rd, 1-3790th, 1-3840th. Extreme sizes 1-4570th and 1-3000th of an inch. Blood from a prick of the upper lip.

88. Cinnamon or Chocolate Bear, (*Ursus Americanus*, var.?) a male, from North America, apparently of the same age as the Black and European Bears. 1-3693rd, 1-3790th, 1-3840th, 1-4000th, all common sizes. Extreme diameters 1-4800th and 1-3000th. Blood from a prick of the upper lip.

89. Polar Bear, (*Ursus Maritimus*,) an old female. The following sizes very common: 1-3600th, 1-3693rd, 1-3764th, and 1-3840th. Extreme diameters 1-4570th and 1-3048th. There were seen, though rarely, some as small as 1-5333rd of an inch. Blood from a prick of the upper lip.

90. Sloth Bear (*Ursus labiatus*,) an aged female and a full-grown male. Most common diameter of corpuscles from 1-4000th to 1-3555th. Extreme sizes 1-4800th and 1-3000th of an inch. Blood from the different cavities of the heart of the female; from a prick of the lip of the male, which was a healthy animal. The unsatisfactory result of the examination of the corpuscles in the female has been already noticed; the specimen from the male was not a very good one.

91. Raccoon, (*Procyon lotor*,) nearly full-grown male. In the dried corpuscles, the following sizes all very common:

1-4500th, 1-4572nd, and 1-4800th. Extreme diameters, 1-6000th and 1-4000th. In the serum, a great many of the disks 1-6000th to 1-5333rd of an inch. Blood from a vein of the fore foot.

92. Wolf, (*Canis Lupus*,) adult male. 1-3554th, 1-3635th, 1-3692nd, all very frequent diameters. Extreme sizes 1-4570th and 1-3000th. Blood from a prick of the ear.

93. Jackal, (*Canis mesomelas*,) adult female. The following diameters very common: 1-3552nd, 1-3600th, 1-3693rd, 1-3790th. Extreme sizes 1-4570th, 1-3000th. Blood from a prick of the ear.

94. Two Spotted Paradoxure, (*Paradoxurus binotatus*,) a female nearly full-grown, from Western Africa. The following sizes frequent: 1-4572nd, 1-4800th, and 1-5052nd of an inch. Extreme diameters 1-6000th and 1-3555th. Blood from a prick of the tail.

95. Striped Hyæna, (*Hyæna vulgaris*,) female not quite full-grown. 1-4000th, 1-3764th and 1-3552nd, common diameters. Extreme sizes 1-4800th and 1-3000th. Blood from a vein of the ear.

96. Lion, (*Felis Leo*,) from Africa, nearly full-grown. The most common diameters 1-4500th and 1-4365th of an inch. Extreme sizes 1-5800th and 1-3554th. Blood from a prick of the ear.

a. Lioness, about two-thirds grown. Some corpuscles obtained from the cutaneous vessels of the leg gave the same measurements.

97. Puma or Silver Lion, (*Felis concolor*,) from South America, full-grown male, 1-4572nd, 1-4500th and 1-4440th, the most frequent diameters of the disks. Extreme sizes 1-5800th and 1-3554th. Blood from a prick of the ear.

98. Tiger, (*Felis Tigris*,) from India, a female, full-grown. Common sizes 1-4440th, 1-4210th, and 1-4268th. Several also of 1-4000th. Extreme diameters 1-5333rd and 1-3428th of an inch. Blood from a vein of the ear.

The size therefore of the blood corpuscles of these larger species of the genus *Felis* is very nearly alike*. In some of the smaller species, as the Cat (27.), Serval (28.), and Lynx (30.), the disks have much the same diameter, as I infer from frequent examinations. Mr. Siddall too, before he was acquainted with the result of my observations, came to the same conclusion, from several trials with the blood of the cat, as compared with one specimen of that of the tiger. In some blood obtained during life from the femoral vein and from the femoral artery of a cat, about a third-grown, the disks most

* See Dublin Medical Press, No. 52.

commonly presented the following diameters, 1-4365th and 1-4752nd of an inch; and there were several 1-4000th, the thickness of the edges of the disks being 1-16,000th. The blood was examined quickly after it was obtained, and no appreciable difference was seen between the arterial and venous corpuscles. They are certainly but very slightly smaller than in the tiger.

99. A Kangaroo, (*Macropus Eugenii*?) a female, 1-3554th, 1-3432nd, and 1-3200th, a common diameter of the disks, and 1-4000th and 1-3000th rather frequent sizes. The edges of the corpuscles generally from 1-12,000th to 1-10,000th of an inch thick. Blood from a prick of the tail.

100. The Coypu, (*Myopotamus Coypus*), an adult. Most common diameters of corpuscles 1-3500th and 1-3200th of an inch. Small disks, 1-4000th; large, 1-2666th, in the dry state. Thickness of the edges of the corpuscles 1-12000th to 1-9600th. In the serum, the corpuscles were more variable in size, 1-4572nd and 1-3000th of an inch being very frequent. Blood from a prick near the buttocks.

101. Gray Squirrel, (*Sciurus cinereus*), adult male. Common diameters 1-4266th, 1-4000th, 1-3840th, and 1-3600th. Extreme sizes 1-6000th and 1-3000th. Magnitude of corpuscles very irregular; a few not more than 1-6400th. Blood from a prick of the nose for the first examination, and from the ear a few days subsequently for another trial.

102. Capistrated Squirrel, (*Sciurus capistratus*), adult male. Common diameters 1-4000th, 1-3790th, and 1-3693rd. Extreme sizes 1-5333rd and 1-3000th. A few of the very small corpuscles less than 1-6400th of an inch in diameter. Blood from a prick of the upper lip.

103. Black Squirrel, (*Sciurus niger*), an adult. All the following sizes frequent, 1-3600th, 1-3692nd, 1-3790th, and 1-3840th. Extreme diameters 1-5333rd and 1-3000th. A few of the very small corpuscles were seen.

The size of the corpuscles, as far as I have observed, is very irregular in the genus *Sciurus*. There are circular particles, though not in large numbers, yet very remarkable for their regularity and diminutive size in regard to the common blood disks. For this reason the very small corpuscles should be further examined. I have certainly seldom seen them so remarkably in any other genus, though I think they appeared in the blood of the female Sloth Bear. In the Palm Squirrel (72.) there were several about 1-7000th of an inch.

104. Wild Boar, (*Sus Scrofa*), from Asia, male, nearly full-grown. 1-4266th, 1-4365th, and 1-4000th, most frequent

diameters of the disks. Extreme sizes 1-1533rd and 1-3555th. Blood from a prick of the nose.

105. Collared Peccari, (*Dicotyles torquatus*), from Mexico. Full-grown female. 1-4173rd, 1-4500th, 1-4572nd, and 1-4800th very common diameters of disks. Extreme sizes 1-6000th and 1-3555th. Much more irregular in size than in the Wild Boar, and from two examinations appearing to be smaller. Blood from a prick of the upper lip.

106. Zebra, (*Equus Burchellii*), full-grown female. Average-sized disks 1-4500th; and 1-4800th, 1-4365th and 1-4000th, not uncommon. Extreme sizes 1-5800th and 1-3368th. Blood from a prick of the nose.

107. Dshikketai or Wild Ass, (*Equus Hemionus*), a female, as large as a common Ass. The most frequent sizes of corpuscles 1-4572nd and 1-4800th. Several as large as 1-4000th. Extreme diameters 1-5800th and 1-3555th of an inch. Blood from a prick of the nose.

With reference to the blood of the Horse (see No. 34 in the former paper) Mr. Siddall obtained the following measurements of the common-sized corpuscles from a cart stallion, aged 12, suddenly killed: 1-4360th, 1-4208th, and 1-4362nd. The blood, from a vein of the testicle, examined soon after the death of the animal.

108. Axis Deer, (*Cervus Axis*), adult male. Most frequent sizes 1-4924th and 1-5333rd of an inch. Extreme diameters 1-6000th and 1-4365th. Altogether very irregular in size. Blood from a vein of the ear.

109. A Deer, (*Cervus macrourus*?). Most frequent diameters 1-5142nd and 1-5333rd. Extreme sizes 1-6400th and 1-4000th. An adult male and female; the blood from the renal vein of the former, and from the ear of the latter.

These animals were shipped at Honduras, and brought home with the Mexican Deer.

110. Reeves's Muntjac, (*Cervus Reevesii*, Ogilby). The corpuscles belonging to the same class as those of the Porcine and Mexican Deer. They will be all fully described together on a future occasion.

111. Sing Sing Antelope, (*Antilope Sing Sing*), adult female. Corpuscles very variable from 1-6000th to 1-4000th of an inch; 1-4800th was very common, and 1-5333rd common. Blood from a prick of the nose.

112. Nyl-ghau, (*Antilope picta*), a young male, hardly half-grown. 1-4924th, 1-4800th and 1-4572nd, all common diameters. Extreme sizes 1-6000th and 1-4365th of an inch. Blood from a vein of the ear.

113. Cervine Antelope, (*Antilope bubalis*), adult male. Cor-
Phil. Mag. S. 3. Vol. 16. No. 101. Feb. 1840. I

puscles remarkably variable in size, and differing much whether examined dry or in their serum. In the former state 1-5333rd and 1-6000th most common diameters, and extreme sizes 1-6400th and 1-4562nd. In the serum 1-6856th the most frequent size. Blood obtained for the first examination from a prick of the nose, and for the second some weeks afterwards from an incision of the ear. The animal was diseased.

114. Buffalo, from Manilla, (*Bos Bubalus*,) adult female, 1-5142nd, 1-4800th, and 1-4500th common diameters. Extreme sizes 1-5333rd and 1-3600th. Average thickness of the edges of the disks 1-14,000th of an inch. Blood from a vein of the ear.

115. Cape Buffalo, (*Bos Caffre*,) full-grown male. 1-5142nd and 1-4800th, most frequent sizes in the dried specimens. Extreme diameters 1-6000th and 1-3554th. The corpuscles in their serum were commonly 1-5333rd, or even as small as 1-6000th of an inch. Blood from a vein of the ear.

From two trials it appeared that disks were slightly smaller than in *Bos Bubalus*.

Napu Musk Deer (49.) An adult female having lately died at the Zoological Gardens, I availed myself of an opportunity of examining some blood from the different cavities of the heart, as well as from the cava, portal, and mesenteric veins. The result fully confirms the accuracy of my former observations*, especially that the blood corpuscles of the Musk Deer are smaller than any previously described in the mammalia. In relation to the parts from which the blood was obtained, there was no appreciable difference in the disks. The average diameter of those procured from the dead animal was 1-13,400th of an inch.

In an animal with blood corpuscles so remarkably minute, it was interesting to ascertain the comparative magnitude of the lymph globules. The latter were therefore carefully examined with this view; and their size, appearance, and chemical characters found to be identical with the lymph globules of many mammals with large blood particles. The lymph granules in the Napu Musk Deer varied in diameter from 1-5000th to 1-3500th of an inch. Hewson inferred from his observations that there was a relation between the particles of the lymph and blood in the same animal, and a difference in the size and shape of the former in different animals.

I have recently examined the blood of the Vicugna (37.), and compared it again with that of the Dromedary (36.). In

* Dublin Medical Press, Nov. 27, 1839. L. and E. Phil. Mag. Dec. 1839, and Jan. 1840.

the former the following measurements of the corpuscles were taken. Long diameters, 1-4000th very common, many 1-3555th, and a few, not commonly seen, 1-2666th. Several considerably shorter than 1-4000th of an inch, for instance, 1-5333rd; and even 1-6000th very rarely. Short diameters most frequently 1-6400th and 1-7110th. The shorter corpuscles are generally broader in proportion than the others, some nearly circular, but in this observation none perfectly so. Mr. Siddall's measurements agree as nearly as possible with mine, and the corpuscles in the Vicugna appear to be a little smaller than those of the Dromedary. Though taken from a vein of the ear, the blood was of a bright brick-red colour, as it appeared from the puncture. It would be singular if the venous blood of any of the mammals with oval particles should not have the usual dark colour.

The Rhinoceros (52, p. 32.) 1-2554th is a misprint for 1-3554th.

The Red American Fox (24) is the *Canis fulvus*, not a variety of *C. Vulpes*.

XIX. *On Galvanic Series formed of Zinc and Inactive Iron.*
By Mr. THOMAS HAWKINS.

To the Editors of the Philosophical Magazine and Journal.
GENTLEMEN,

ONE of Professor Schoenbein's experiments described at p. 429 of vol. x. of the L. & E. Phil. Mag., by which he obtained an electric current of high intensity with wires "having one of their ends coated with peroxide of lead, and each end put into a separate vessel filled with nitric acid, a hundred times diluted," interested me so much, that I was led, at the time of its publication, to make some experiments, with the view of establishing the possibility of employing iron in the *peculiar condition* as the negative metal in voltaic batteries. After some failures I succeeded in forming a galvanic circle with a fine iron wire put into nitric acid of sp. gr. 1.5 contained in a tube of plaster of Paris, and associated with amalgamated zinc in diluted sulphuric acid, which electrolyzed water; and two of such circles, in series, liberated the gases rapidly, continuing in action for seven hours, when sudden effervescence of the acid occurred and the wires were dissolved. Efficient circles, but of less permanency, were constructed of iron wires alone, having one of their ends in the porous cells of nitric acid, the other in diluted sulphuric or even very diluted nitric acid. The liability of the iron to be suddenly attacked by the nitric acid was prevented, so far as my experience went, by dissolving mercury in the acid, in accordance with the Pro-

fessor's observation of the effect of nitrate of mercury in preserving the inactive state; but it was also necessary to protect the wires at and above the surface of the acid from the corroding action of its fumes by a coating of wax or glass. By these means the inactive condition of the iron was maintained; but another obstacle then arose in the crystallization of nitrate of mercury, by which the cells after twenty or thirty hours' use were generally broken; and this could not be surmounted but at the sacrifice of some power, by diluting the acid, which the presence of the nitrate permitted, and the substitution for the cells of pipe-clay of others made of wood.

I cannot give comparative results obtained with iron and platina batteries, but I may mention that with an arrangement of six cylinders of sheet iron, each containing thirty-six square inches in strong nitric acid, without mercury, and associated with zinc plates of half their size, a current was evolved which for several hours ignited charcoal, or the whole of a strip of platina six inches in length by one-eighth of an inch in width. The construction of this battery was imperfect in several respects, particularly in the porous vessels being much too thick; there is, therefore, reason to suppose that an equal power might have been obtained from a less number of cells. As some progress in advance of the Professor's experiment in which the current ceased with the solution of the film of peroxide of lead, this communication may possibly be deemed worthy of being recorded: but the importance of my results in relation to the proposed object of the experiments is, I think, materially affected by the discovery of Mr. Cooper, as given in your last Number, of the application of charcoal and other forms of carbon as a substitute for platina in the voltaic arrangement of Mr. Grove.

I have the honour to be, Gentlemen, &c.

52, King's Road, Brighton,
Jan. 10th, 1840.

THOMAS HAWKINS.

XX. *On Lightning Conductors, and the Effects of Lightning on Her Majesty's Ship Rodney and certain other Ships of the British Navy: being a further examination of Mr. Sturgeon's Memoir on Marine Lightning Conductors. By W. SNOW HARRIS, Esq., F.R.S., &c.*

[Illustrated by Plate I.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

1. **I**N my former communication (L. and E. Phil. Mag. vol. xiv. p. 461.) I considered the nature of a well-known

phænomenon in electricity, termed by Cavallo, Priestley, and others the lateral explosion, and showed that it did not apply to the state of a metallic rod in the act of transmitting a vanishing electrical accumulation between two opposed electrified surfaces, as insisted on by Mr. Sturgeon in a recent number of his *Annals of Electricity*. I will now proceed to examine the general character and effect of ordinary electrical discharges, whether produced on the great scale of nature, or artificially, with a view of further showing, that such lateral explosions do not occur at the instant of the passing of a shock of lightning through a metallic conductor, as also with a view of meeting certain other objections which have been advanced at different times to the use of lightning rods in ships.

2. I should not have felt myself called upon to notice further Mr. Sturgeon's memoir, did I not consider the statements it contains, although superficial and inconclusive, likely to mislead the public upon many important points connected with the effectual protection of shipping against the destructive effects of lightning, and convey false views of the nature of electrical action. Under these impressions I have little hesitation in noticing what he has advanced under the following heads:—

1st. Examination of the observed effects produced on shipping by lightning.

2nd. A comparison of the observed effects of lightning and the probable effects which lightning would produce by the application of Mr. Harris's conductors to shipping.

3. The first contains an excellent, and I have no doubt, an accurate statement, by an intelligent officer of the *Rodney*, of the destructive effects of lightning lately experienced in that ship, together with notices of two cases in which ships fitted with my conductors were struck by lightning without any attendant ill consequence. In the second, it is the author's object to prove, from the effects of lightning in the *Rodney*, that my system is inadmissible; since the discharge of lightning, he observes, which struck the *Rodney*, "would have been powerful enough to have rendered even the thickest part of Mr. Harris's conductors sufficiently hot to ignite gunpowder."

Considering the boldness of this assertion, and the high pretension of the memoir, we should expect, on examining the author's researches, to find him in possession of a copious induction of facts from well-authenticated cases of damage by lightning on ship-board, illustrating clearly the views he so strenuously insists on,—cases in which continuous or other

metallic conductors have been from any cause placed along the masts or rigging, and in which the electric agency found its way through the hull to the sea. We should further expect from him, something like an examination of the general nature and effects of electrical discharges, since it is clear, before any accurate estimate can be arrived at, of the relative quantity of electricity likely to be discharged from a thunder-cloud, and the probable effects on metallic rods, or other conductors set up with a view of directing it in any given course, such information is quite indispensable.

4. Now it is to be particularly observed, that Mr. Sturgeon's memoir is really deficient in such information; a few clumsy experiments in illustration of a well-known fact in electricity, deceptively associated, by means of a vague hypothesis, with some of the ordinary effects of lightning, on a ship *not having* any regular conductor, and with some every-day phænomena of the electrical kite, is virtually the amount of all that the author has advanced, under the imposing title of "Theoretical and Experimental Researches."

5. In illustration of the careless way in which he meets this question, it may not be out of place to notice the following specimen of his inductive philosophy, — being the very outset of the comparison he has proposed, of the observed effects of lightning, and the probable effects on my conductors*.

In the account given of the damage recently sustained by H.M. Ship Rodney, it appears, that the shock of lightning which shivered the top-gallant-mast, damaged the top-mast, &c., &c., fell on a small brass sheave in the truck for signal halliards, and *slightly* fused it. This sheave weighed about 4 ounces; it was only about an inch and a half diameter, hollowed except at the centre and rim, where it was somewhere about half of an inch in thickness. The lightning also fell on the copper funnel for top-gallant rigging, being a hollow cylinder of 16 inches in length, 10 inches in diameter, and not quite a quarter of an inch thick. This funnel was not anywhere fused. It fell also on other metallic masses, such as the iron-bound tie-block, on the top-sail-yard, &c., &c., the iron hoops of the mast, &c., on which no calorific effect was apparent.

6. Now we have here something like evidence what was really the *actual power* of the charge. We see, for example, that it *did not* fuse a copper funnel, 16 inches long, 10 inches in diameter, and about $\frac{1}{5}$ th of an inch thick. In the face of which fact Mr. Sturgeon insists, that had the charge fallen on

* Sturgeon's Memoir, sec. 204.

my conductor, the thickest part of it would have become red-hot. His reasoning, in fact, amounts to this ; an explosion of lightning having *slightly* fused a small brass sheave, weighing 4 ounces, and having failed to fuse a short copper funnel, therefore had it fallen on a rod of copper of one inch in diameter, and 200 feet long*, that rod would have been rendered red-hot.

This, it must be allowed, is a somewhat amusing kind of special pleading, quite unprecedented, I believe, in any paper on science.

7. The author wishes to strengthen his deduction, such as it is, by adverting in a foot-note to the case of a small brig struck by lightning, in which some part of a chain conductor is *supposed* to have been fused ; how much is not known, “as the lower part fell overboard.” The statement is given without any quoted authority, and is altogether deficient in the very information most required, viz. the *size of the chain, and how much of it was fused*. Let us, however, take it upon the author’s own ground, and suppose the conductor to have been such as is commonly used in the merchant service,—that is to say, links of iron wire of about one-fourth of an inch in diameter, united by rings, a kind of conductor very easily disjointed and fused at the points of junction by lightning ;—the reasoning then stands thus : because a shock of lightning fused and disjointed some unknown portion of a lightning chain in a merchant brig, therefore the same shock, had it fallen on a solid copper rod of one inch in diameter and 100 feet long, would have rendered that rod red-hot.

7. The fallacy and entire worthlessness of such reasoning, seems not altogether to have escaped Mr. Sturgeon’s notice, as appears by his amplification of the above effects ; thus on entering upon the comparison of the effects of lightning, he resorts to a sort of wholesale dealing, and leads the reader to conclude that the *entire* sheave in the *Rodney* and *all* the brigs’ conductor underwent fusion. But even if it were so, no such conclusion as that above mentioned is admissible†, especially in reference to a continuous and massive conductor termina-

* This is the equivalent of my conductor on the main-mast of such a ship as the *Rodney*, taking it at its least value.

† “ Were there no other data than those of the *fusion of the metallic sheave in the Rodney and the fusion of the chain-conductor in the brig Jane*,” &c. &c.

“ The impressions which these facts convey to the mind are too definite to be easily misunderstood ; they clearly imply that either of the discharges which struck the *Rodney* or *Jane* would have rendered the thickest part of Mr. Harris’s conductors sufficiently hot to ignite gunpowder,” &c. &c.—*Sturgeon’s Memoir*, sec. 204.

ting in a point, and equalizing with inconceivable rapidity the disturbed electrical state of the sea and clouds.

8. The manifest deficiency of sound practical information in Mr. Sturgeon's memoir, imposes upon me the necessity of adverting to the general character and operation of common electrical discharges, whether produced by artificial means or on the great scale of nature. In doing this I have no desire to excuse myself, in case I should not have written clearly and explicitly on the subject, since in no department of physics is the field of observation so fertile, and the path of experiment so sure and easy. We have before us the experience of nearly a century, during which time lightning-rods have been employed; a great number of instances have occurred of shocks of lightning falling on ships under a variety of different circumstances, in some cases where lightning conductors have been present, in others where absent; in many instances where ships have been near each other and exposed to the same storm, some *having* conductors, others *not*. The general laws of the discharge are traceable in them all, and the effects on metallic bodies distinctly shown. On the other hand, we can on a minor scale, imitate successfully the great operations of nature, and examine experimentally every possible contingency attendant on the operation of a shock of lightning in a ship. It is our own fault, therefore, if we do not treat the subject scientifically, and arrive at complete practical solutions of such questions as these: Is a lightning conductor desirable in a ship? Will it cause by attraction a shock of lightning to fall on a ship when otherwise such would not take place? If so, can it cause damage by its inability to get rid of the lightning which falls on it? What is the *best* form and dimensions of a lightning conductor for a ship? What is the greatest probable force of lightning to which it may become exposed? Is it liable to cause damage by any lateral operation of the charge passing through it? I say, if such questions as these cannot now be reasonably determined they probably never can; and, therefore, any one who writes or reasons obscurely about them, and without due regard to a good induction of facts, can have no claim to be considered as a sound reasoner in experimental science; for, as beautifully observed by Lord Bacon, "Man, who is the servant of nature, can act and understand no further than he has, either in operation or in contemplation, observed of the method and order of nature." Under these impressions I proceed to examine the general character and effects of electrical discharges as exhibited artificially, and on the great scale of nature.

9. Although some theoretical differences may have arisen

concerning the precise nature of electricity, yet the following explanation runs sufficiently parallel with facts to entitle it to our confidence, and put us in possession of one of the great advantages of *every* theory, viz. a classification and connexion of observed effects; the province of human knowledge, being, as justly observed by a most intellectual and accomplished writer, “to observe facts, and trace what their relations are*.”

General principles:—

10. There is an invisible agency in the material world intimately associated with common matter, termed electricity.

11. Lightning, thunder, and a variety of analogous phenomena of a minor kind, artificially produced, result from discharges of this agency between bodies differently affected by it.

12. In every case of electrical discharge there are two surfaces of action; one existing on some substance eager to throw off redundant electricity, being, according to Dr. Franklin, overcharged with it; the other existing in some other substance eager to receive electricity, being, according to the same philosopher, deficient of it, or undercharged.

13. Two opposed bodies, when placed in these opposite electrical states, have a sort of exclusive action on each other, either *directly* through any intervening substance, whether a conductor of the electrical principle or not, or otherwise *indirectly* through any lateral circuit.

Thus two metallic surfaces *A B* (fig. 1.) pasted on the opposite sides of a square of glass *cd*, have, when the square is said to be charged, an exclusive action on each other, either through the intervening glass, or otherwise through any conductor, *A o B*, connecting them.

Now we have only to suppose these planes placed further apart, as in fig. 2, to have a discharging conductor, *mn*, of greater or less extent between them, to be greatly increased in size, to be separated by air instead of glass, and to consist of free vapour or water, and we have a pretty faithful representation of the conditions, under which a discharge of lightning takes place, when passing partly through the air, and partly through a discharging conductor, *mn*, or any other body, *cd*, placed on the plane *B*†.

* Abercrombie on the Intellectual Powers.

† The thickness of the intervening air, and the amount of free electricity in the clouds, has led Professor Henry to question in some measure, the perfect analogy of a discharge of lightning, with that of a Leyden jar; but I think upon *mature* consideration this circumstance will not be found in any way subversive of the general principle. Thus whether electricity

14. Any continuous metallic rod or other body, $m n$ (fig. 2.), connected with the lower plane, must be considered merely as a passive way of access for the charge so far as it goes; the electrical agency being observed to seize upon substances best adapted and in a position to facilitate its progress, or otherwise to fall with destructive effect upon such as resist it.

15. It is easy to perceive here, that the presence of a conducting rod, $m n$ (fig. 2), or other conducting body, has nothing whatever to do with the great natural action set up between the planes $A B$. It is in fact to be considered merely as a point in one of them. The original accumulation of electricity and subsequent discharge, would necessarily go on whether such a rod were present or not, as is completely shown by experience. When present, its operation is confined to the transmission, so far as it extends, of that portion of the charge which happens to fall upon it; and since it is quite impossible to avoid the presence of conducting bodies in the construction of ships, it is the more important to understand clearly in what way damage by lightning occurs to the general mass, and how it may be best avoided.

16. When discharges of lightning fall upon a ship in the way above stated, as being a heterogeneous mass fortuitously placed between the charged surfaces $A B$ (fig. 3.), the course of the discharge is always determined through a certain line or lines, which upon the whole least resist its progress. The interposed air between the ship and the clouds first gives way in some particular point, probably the weakest,—suppose at A , fig. 3;—the electrical agency then meeting with continued resistance from the non-conducting particles of air, is often turned into a tortuous course. Suppose it arrives in this way at some point, m , in the vicinity of a ship at k , the

be accumulated on thick glass or on thin, the result is the same; it is merely the intensity as indicated by the electrometer which changes.

Now the term free electricity, applies to the greater or lesser influence of the opposed coating in respect of other bodies. In the case of the opposed surfaces of the clouds and earth, all the charge is necessarily free electricity, since there exists no other point upon which it can tend to discharge. In the same way the electricity of the jar, when the coatings are very near, is nearly all redundant, or free electricity, in respect of the action between them, although latent in respect of other bodies. Hence with a moderate accumulation, the electrometer exhibits but a small intensity, if any. The only difference at the time of the discharge, is in the position of the discharging circuit, which in the case of the clouds and sea, is directly in the interval of separation; and as we find the principle of induction always active in cases of lightning, the thickness of the stratum has evidently no influence on the conditions of the accumulation, especially when we consider the great extent of the opposed surfaces, which may possibly be 20,000 or more square acres. Dr. Faraday has shown that no distance excludes the inductive action.

question whether it would strike upon the mast at y would be determined by the resistance in the direction of $m y k$, as compared with that in any other direction m, B ; whether, in fact, it would be easier to break down the remaining air in the direction $M B$, or otherwise the air in the direction $m y$, supposing the ship's mast to facilitate the progress in that direction.

17. Let the charge however strike in the direction $m y$, and so fall upon the mast,—then in proceeding to its ultimate destination, viz. the plane of the sea B , its course is still determined by the same general principles; that is to say, it seizes upon all those bodies which tend to assist its progress, and which at the same time happen to be placed in certain relative positions, *and upon no others*, falling with destructive effect upon intervening bad conductors, and exhibiting in non-conducting intervals all the effects of a powerful expansive force. If we examine carefully the course of discharges of lightning on ships in some hundred instances in which damage has ensued, we shall find this effect invariable. The damage has always occurred where good conductors cease to be continued, and the destructive consequences most apparent are those usually produced by expansion. The calorific effects, except as depending on this cause, are really inconsiderable; there are comparatively few instances in which metallic bodies have been fused, and no instance in which a bolt or chain of any considerable magnitude has been even much heated.

The following experimental and natural illustrations of these facts will be found conclusive and interesting.

Exp. 1. Lay some small detached pieces of leaf-gold a, b, c, d , &c. on a piece of paper, as represented in fig. 4; pass a dense shock of electricity over these, from the commencement at A to the termination at B , so as to destroy the gold; the line which the discharge has taken will be thus shown by the blackened parts; the result will be as in fig. 5, in which we perceive the course of the discharge has been in the dotted line a, b, d, e, f, g, h, l , being the least resisting line; and it is particularly worthy of remark, that not only are the pieces c, k untouched, being from their positions of no use in facilitating the progress of the charge, but even portions of other pieces, which have so operated, are left perfect, as in the transverse piece i and portions of a, b, d, e , and f ; so little is there any tendency to a lateral discharge, even up to the point of dispersion of the metallic circuit in which the charge has proceeded; indeed, so completely is the effect confined to the line of least resistance, that percussion powder may be placed with impunity in the interval between the portions c, d .

Now the separate pieces of leaf-gold thus placed, may be taken to represent detached conducting masses fortuitously placed along the mast and hull of a ship.

Exp. 2. Let a thin continuous line, m, n , be passed through the separated pieces, and a dense accumulation discharged over the whole, as in the preceding case. The effect will be as represented in fig. 6.: the discharge will be confined to the line of least resistance; and we may perceive in this, as in the former case, that those pieces, or parts of pieces, out of the track of the discharge, are not affected; thus a part only of the piece g is destroyed, also of the piece i , whilst other pieces, b, d, e, f, l , which in the former case, where the continuous line, a, b , was not present, were blackened by the discharge, remain here perfect.

Exp. 3. If the continuous line A, B (figs. 7, 8) be assisted by other comparatively short collateral branches, as $d e, d c$, having one common connexion at B , then a discharge which would destroy the line A, B , will divide upon these auxiliary lines, and the part d, B will either escape, or the whole will suffer together.

Exp. 4. Pass a discharge over a strip of gold-leaf, as A , fig. 2; every part of it, as indicated by the last experiment, will participate in the shock; and if it be of uniform density and thickness it will be everywhere equally affected, so that one portion will not be destroyed without the whole. This result will be readily distinguished from that represented at d and i , fig. 5, where the masses lie across the track of the discharge.

The diagrams here referred to, are copied from the actual effects of the electrical discharge in the way above mentioned.

18. These experiments are instructive. They evidently prove, that an electrical explosion will not leave a good conductor, constituting an efficient line of action, to fall upon bodies out of that line. Mr. Sturgeon's assertion that a conductor on a ship's mast would operate on the magazine is therefore quite unwarranted. Besides, we have many instances of the masts having been shivered by lightning into the step, whilst acting as partial conductors, without any such consequence; as happened in the *Mignonne* in the West Indies, the *Thetis* at Rio, the *London*, *Gibraltar*, *Goliath*, and many others. Instead, therefore, of a conductor on the mast being dangerous, it is absolutely requisite as a source of safety to the ship, by confining the discharge to a given line and leading it to the sea.

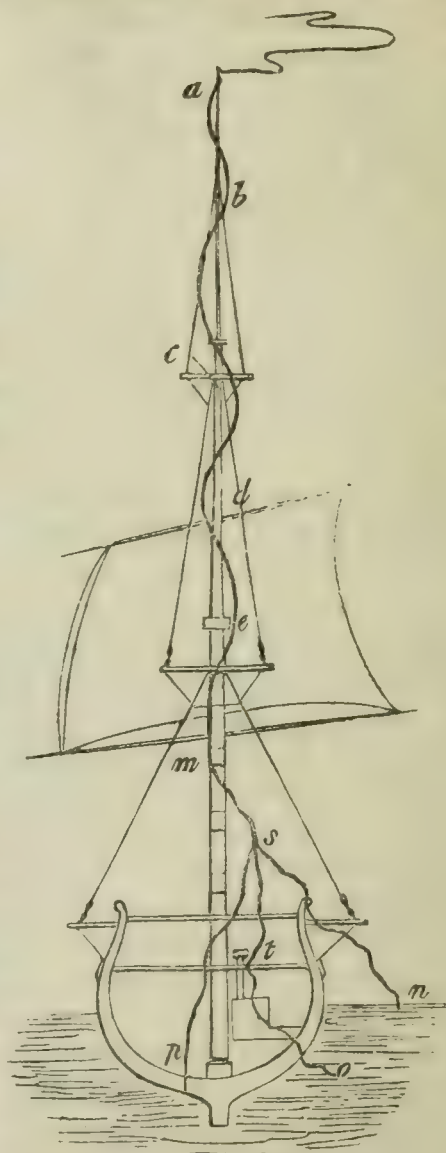
19. It was from a careful consideration of the common effects of lightning, and from such experimental facts as those above mentioned, that I was led to suggest the propriety of

fitting continuous conductors of lightning of great capacity in the masts of ships, linking them by efficient communications, together with the principal detached metallic bodies in the hull, into one general continuous system, and finally connecting the whole with the sea. These conductors consist of two laminæ of copper-sheet, varying from one inch and a half to five inches wide, and being together nearly one-fourth of an inch thick; they are inlaid so as to be fair with the surface of the mast, and form a series of shut-joints; they are otherwise so constructed as to present an uninterrupted line of action from the highest point to the sea. The method has been partially used in the British navy for several years, and has been proved in every way efficient. In no case has any of the vessels fitted with them received the slightest damage, although frequently exposed to severe thunderstorms, and in some instances actually struck by heavy discharges similar to that which fell on the *Rodney* in December, 1838*.

20. If we consider attentively the effects of this shock, we shall find them in complete accordance with the principles just stated. The attendant phænomena were of the simplest kind, and such as have always occurred in cases of ships struck by lightning not having a continuous conductor: e.g. the electrical discharge, in forcing its way between the sea and clouds, over resisting intervals, and between discontinuous metallic masses, was productive of a violent expansive effect in these intervals; causing at the same time a considerable evolution of heat. There was really nothing particularly remarkable in this instance; the course of the discharge was a very simple affair, being, according to the law of electrical action just exemplified (Exp. 2), in the line or lines of least resistance from the highest point to the sea: thus the course of the discharge was, as represented in the annexed diagram, along the masts and rigging, upon the general mass of the hull and sea. The vane-spindle *a*, upon which the accumulation was first concentrated, was of course severely dealt with. From this, being probably assisted by the moisture on the surface of the wood, it glanced over the royal pole to the head of the top-gallant mast at *b*, where it found intermediate metallic assistance in the copper funnel for the top-gallant rigging: from this, the resistance in the mass of the wood appears to have been less than that on its surface, probably from the long interval of air between the funnel and conducting bodies about the cap below, the mast was therefore split open as far as the cap at *c*. Here again it was enabled to strike over the surface of the

* See a letter in the *Nautical Magazine* for December 1839, by Lieut. Sullivan, R.N., who witnessed these effects.

mast upon the metals about the parrel of the top-sail-yard at *d*, where the accumulation became again concentrated, producing a powerful expansion and heating effect so far as the lower cap at *e*; and thus it passed along *per saltum* over the lower mast *m*, from one metallic mass to another, until within a striking distance *s* of the sea and hull, it divided upon the hull and sea in convenient directions *s n*, *s o*, *s p*. In this course, as indicated by the waving black line *a, b, c, d, &c.*, it evidently sought assistance from all the conducting matter it could seize upon; such as the wet ropes, the copper funnel for top-gallant rigging at *b*, the iron work and other bodies about the topmast cap at *c*, as also the men in the top-gallant crosstrees at *c*. The charge evidently divided upon them in proportion to the assistance each could afford as a small auxiliary circuit, as in Exp. 3; the men nearest the mast would be necessarily in the more direct course of the discharge, the others would be more or less so according to their respective positions; that these poor fellows who were killed suffered in this way as being conductors to parts of the charge is evident from the appearance of the bodies.



Mr. Sturgeon calls especial attention to the circumstance of the men being thrown in opposite directions, and thinks it remarkable: but how could it be otherwise? the intervening air being caused to expand violently from a central point, would necessarily operate as a central force; surely there is nothing very new in this. About the parrel of the topsail-yard at *d*, we should expect again powerful effects; for here again the charge became concentrated, and set the sail, &c., on fire. This is quite in accordance with the known laws of electrical

action; thus we find the points of ingress and egress of an artificial charge, when caused to fall on a slip of gold leaf or other matter, are always those in which the most powerful effect arises; and when we desire to fire inflammable matter by electricity we place it directly between detached metallic points.

21. The circumstance of the lightning striking over portions of the wet mast without damage, is precisely the same effect as observed in certain cases of artificial electrical discharges. Thus a very slight film of moisture will allow a jar intensely charged to discharge a luminous ball over a long strip of glass. Dr. Franklin found he could destroy a dry rat by an electrical shock when he failed to hurt a wet one. If we continue to follow the discharge we find similar expansive and destructive effects; such as the bursting of the hoops on the mast, &c., &c., which will sometimes occur and sometimes not.

22. There is really nothing in all this to call for especial remark, except we may observe, as shown by the experiments already described, that if a good capacious conductor had been incorporated with the mast from the truck to the metallic masses in the hull and to the sea, then these *expansive* and destructive effects could not possibly have occurred; since the interrupted circuit would have been avoided, and the intense electrical action have vanished, or nearly so, at the mast-head, for it would have no longer been driven to force its way in a dense explosive form to the hull and sea: of this we have the most complete evidence from experience, particularly in the cases of the ships struck by lightning having such conductors as those just alluded to, curiously enough quoted by Mr. Sturgeon as evidence to the contrary. It seems a strange way of disproving a fact to quote those who, having been eye-witnesses, insist upon its truth. That the electric matter finally distributed itself upon the hull as *well* as on the sea, is evident from the circumstance of the casing of Hearle's pump at *t*, which led through the side under water being shivered; from the vivid electrical sparks below, and from the usual smell of sulphur in the well, and appearance of smoke in the orlop-deck.

23. The interrupted circuit therefore to be traced here, is first from the vase-spindle to the copper funnel of top-gallant rigging; 2nd, from this to the conducting bodies at the heel of the top-gallant mast; 3rd, thence to the metallic masses about the parrel of topsail-yard; 4th, between this and the metallic bodies about the head of lower mast; 5th, from this over the detached metallic bodies on lower mast; finally, from lower mast to the hull and sea. The effect of this shock of lightning appears to have been somewhat palliated by heavy rain.

Although Mr. Sturgeon has gone far out of his way to twist these phenomena into an accordance with certain theoretical views, and sets them up as being of an extraordinary kind, they are nevertheless of a very simple character, and are merely illustrative of a few well-known laws of electrical action.

[To be continued.]

XXI. *On a new Compound of Ferrocyanide of Potassium, with Cyanide of Mercury.* By ROBERT KANE, M.D., M.R.I.A.*

IT had frequently occurred to me to notice that, in the process for obtaining cyanide of mercury by the action of ferrocyanide of potassium on sulphate of mercury, it was necessary to observe accurately the equivalent proportions of these substances, in order to ensure success. If any ferrocyanide of potassium were present in excess, a corresponding deficiency in the quantity of the cyanide of mercury always occurred. I at last traced this circumstance to the fact, that the ferrocyanide of potassium in excess combines with the cyanide of mercury, to form a new substance so similar in appearance to the former of the two, as to be very easily confounded with it and thus rejected in the crystallizations.

This new salt is most easily prepared by dissolving together in a moderate quantity of water about one part of ferrocyanide of potassium in crystals with two of cyanide of mercury. On cooling, the new salt separates in the form of rhomboidal plates of a rich yellow colour, almost as deep as that of ferrocyanide of potassium. When heated, these crystals lose some water and become whitish and opake, then blacken and yield cyanogen and mercury; the usual products of the decomposition of ferrocyanide of potassium remaining behind.

With a protosalt of iron, a solution of this new compound yields Prussian blue, and indeed, every reagent which acts on either constituent gives its characteristic reaction with this new body.

For its analysis very simple methods were sufficient.

Forty grains dried at 300° Fahr. lost 2.31 of water or 5.78 per cent.

The remaining 37.69 grains were dissolved in water, and then decomposed by a stream of sulphuretted hydrogen. The sulphuret of mercury was collected and dried. It weighed

* Communicated by the Author.

23·5 grains corresponding to 58·75 per cent. containing 50·13 mercury, equivalent to 63·05 of cyanide.

The liquor from which the mercury had been thus separated, was evaporated carefully to dryness, and the salt obtained was deprived of all water of crystallization by exposure to a temperature of 300°, until it ceased to lose weight. It then weighed 31·32. It was pure dry ferrocyanide of potassium.

A quantity of the new salt equal to 50·4 grains was ignited and incinerated; the residue then treated by muriatic acid, and the iron thrown down by ammonia added in excess. The oxide of iron weighed contained 2·23 of metallic iron, corresponding to 4·47 per cent.

The liquor after the separation of the iron was evaporated to dryness, and ignited; there remained chloride of potassium, equal to 12·3, containing 6·51 potassium or 12·91 per cent.

This new salt, therefore, contained

Mercury.....	50·13
Iron	4·47
Potassium	12·91
Water.....	5·78
Loss and cyanogen	26·71
	<hr/>
	100·00

The relation is exactly such that the mercury employs half of the cyanogen to form cyanide of mercury, and the remaining half forms with the potassium and the iron common ferrocyanide of potassium: the result as calculated should be

3 atoms mercury	304·2	50·26
2 ——— potassium	79·4	13·11
1 ——— iron.....	28·0	4·62
6 ——— cyanogen.....	157·8	26·07
4 ——— water.....	36·0	5·94
	<hr/>	<hr/>
	605·4	100·00

The existence of this salt is of considerable practical importance, as it shows the necessity of avoiding any excess of ferrocyanide of potassium in preparing cyanide of mercury; an error into which, from motives of œconomy, the manufacturing operator would be peculiarly liable to fall.

23, Gloucester Street, Dublin, Dec. 23, 1839.

XXII. *On the Decomposition of the Neutral Sulphate of the Peroxide of Iron by boiling its Solution.* By TH. SCHEERER.*

A CONCENTRATED solution of the neutral sulphate of iron may be heated to boiling without becoming opaque, but if one part of this salt be dissolved in 40 parts of water, continued boiling precipitates traces of a basic salt, which increase so as to form a considerable precipitate the more the solution is diluted with water.

This salt is a combination of sulphuric acid, peroxide of iron and water, in the following proportions:

74·70 peroxide of iron.
12·57 sulphuric acid.
12·70 water.

99·97

Thus it consists of 6 atoms of the peroxide of iron, 2 atoms of sulphuric acid, and 9 atoms of water; the theoretical composition, therefore, would be

74·46
12·71
12·83——100·00

and accordingly the formula of Berzelius is $2 \text{Fe}^3\ddot{\text{S}} + 9\text{H}$,
or $2(\text{Fe}\ddot{\text{S}} + 8\ddot{\text{Fe}}) + 27\text{H}$.

According to the nomenclature of Berzelius, this salt may be called the eight-fold basic sulphate of the peroxide of iron. The oxygen of the water amounts to the half of that in the oxide, being quite analogous to the five-fold basic salt which is produced by the oxidation of a solution of sulphate of iron in the open air.

Dried at 212° Fahr., this salt forms a dark orange yellow powder, its colour being lighter in proportion as the solution is previously diluted, and the less it is boiled. It is not dissolved by water, but pretty readily by acids. At a temperature below a dull red heat, it loses its water and becomes of a dark brown colour. At a red heat the sulphuric acid is expelled and the peroxide of iron is left behind.

Experiments were made to discover how much of the sulphate of iron was decomposed by various degrees of dilu-

* Communicated by the Author, to whom we beg to return our kind thanks. The present extract forms the substance of two distinct articles, published in Poggendorff's *Annalen*, vol. xlii. p. 104, and vol. xlv. p. 453.—EDIT.

tion, which may be determined by the quantity of the salt precipitated. The results were as follows: dissolved in

100 parts of water, 0.309 precipitated.				Opacity commenced at 203°F.		
200	—	—	0.558	—	—	158
400	—	—	0.749	—	—	137
800	—	—	0.806	—	—	122
1000	—	—	0.912	—	—	117.

If 1 part of the sulphate of iron was dissolved in 10.000 parts of water, the solution became opaque even at the temperature of about 63° F. which was that of the water employed; but if it was afterwards heated to boiling, not a trace of iron could be detected in the solution filtered from the precipitate either by ammonia or by tincture of galls. The above quantities are mere approximations to accuracy, as in the first place, during the boiling of the solution, more or less water is evaporated, by which the degree of dilution is altered; 2ndly, the water which is condensed in the upper part of the tube again falling down, causes a momentary increased dilution, and consequently an increased quantity of the precipitate; and 3rdly, the boiling point is heightened the greater the quantity of salt of iron dissolved in the water. Nevertheless the result of the experiments is sufficiently accurate to allow of our establishing the following law with respect to the relative quantities of the peroxide of iron remaining in solution after boiling: *With a 200-fold and greater dilution, the quantities of the peroxide of iron remaining in solution are in inverse ratio to the dilution.* Indeed if these quantities are calculated according to the above-mentioned proportions, we find approximatively that,

With a 200-fold dilution,		$\frac{1}{2}$	of the peroxide of iron remains dissolved.	
—	400	—	$\frac{1}{4}$	—
—	800	—	$\frac{1}{8}$	—
—	1000	—	$\frac{1}{10}$	—

This progression, however, is not generally exact, for instance by a 100-fold dilution $\frac{2}{5}$ ds of the iron should remain dissolved. The law therefore is approximative only for the central members.

If a solution of the neutral sulphate of the peroxide of iron is mixed with a solution of the neutral sulphate of potash, the same basic salt is precipitated without any part of the potash entering into the combination. The properties of the solution of sulphate of iron now described may be employed to separate the peroxide of iron from some salts. The neutral sulphates of manganese, nickel and cobalt have no acid reaction upon litmus paper like the neutral sulphate of the per-

oxide of iron. If therefore one of these sulphates is mixed with the latter in solution, by far the greater portion of the iron may be precipitated by saturating, as nearly as possible, the solution with potash, and the remaining portion of iron may be thrown down by dilution and boiling. It remains only to be observed that no other acid than sulphuric acid must be present, and that the solution nearly saturated must be diluted with at least twice or three times its quantity of water.

This method may not only be employed with advantage in preparing pure oxide of cobalt, but also in analysis. When no error has occurred, the iron is perfectly free from cobalt, although the cobalt may sometimes contain a slight trace of iron.

During the preparation of pure oxide of cobalt from the roasted ores, arsenious or arsenic acid is constantly present. This need not first be separated by sulphuretted hydrogen, for it is precipitated, on treating it in the manner above described, as arseniate or arsenite of iron. It is, however, better in this case to add to the solution previously to saturation a quantity of the sulphate of the peroxide of iron, as otherwise there might not be a sufficient quantity of iron present to take up the whole of the arsenious acid, and then arseniate or arsenite of cobalt would also be thrown down.

XXIII. *A Method of determining by mere Inspection the derivatives from two Equations of any degree.* By J. J. SYLVESTER, F.R.S. and R.A.S., Professor of Natural Philosophy in University College, London.*

LET there be two equations, one of the n th, the other of the m th degree in x ; let the coefficients of the first equation be $a_n \ a_{n-1} \ a_{n-2} \ \dots \ a_0$, each power of x having a coefficient attached to it, a_n belonging to x^n and a_0 to the constant term.

In like manner let

$b_m \ b_{m-1} \ \dots \ b_0$ be the coefficients of the second equation.

I begin with

A Rule for absolutely eliminating (x) .

Form out of the (a) progression of coefficients (m) lines, and in like manner out of the (b) progression of coefficients form (n) lines in the following manner:

* Communicated by the Author. See the December and January Numbers of this Magazine,

1. (a). Attach $(m-1)$ zeros all to the *right* of the terms in the (a) progression; next attach $(m-2)$ zeros to the right and carry over to the left; next attach $(m-3)$ zeros to the right and carry over 2 to the left. Proceed in like manner until all the $(m-1)$ zeros are carried over to the left and none remain on the right.

The (m) lines thus formed are to be written under one another.

1. (b) Proceed in like manner to form n lines out of the (b) progression by scattering $(n-1)$ zeros between the right and left.

2. If we write these (n) lines under the (m) lines last obtained, we shall have a solid square $(m+n)$ terms *deep* and $(m+n)$ terms *broad*.

3. Denote the lines of this square by arbitrary characters, which write down in vertical order and permute in every possible way, but separate the permutations that can be derived from one another by an even number of interchanges (effected between *contiguous* terms) from the rest; there will thus be half of one kind and half of another.

4. Now arrange the $(m+n)$ lines accordingly, so as to obtain $\frac{1}{2}(m+n \cdot m+n-1 \dots 2 \cdot 1)$ squares of one kind which shall be called positive squares, and an equal number of the opposite kind which shall be called negative.

Draw diagonals in the same direction in all the squares; multiply the coefficients that stand in any diagonal line together: take the sum of the diagonal products of the *positive* squares, and the sum of the diagonal products of the *negative* squares; the difference between these two sums is the prime derivative of the zero degree, i. e. is the result of elimination between the two given equations reduced to its ultimate state of simplicity, there will be no irrelevant factors to reject, and no terms which mutually destroy.

Example. To *eliminate* between

$$a x^2 + b x + c = 0$$

$$l x^2 + m x + n = 0$$

I write down

$$\left. \begin{array}{l} a \ b \ c \ o \\ o \ a \ b \ c \\ l \ m \ n \ o \\ o \ l \ m \ n \end{array} \right\} \begin{array}{l} \dots (1) \\ \dots (2) \\ \dots (3) \\ \dots (4) \end{array}$$

I permute the four characters (1) (2) (3) (4) distinguishing them into positive and negative; thus I write together

Positive Permutations.

1	2	3	1	2	3	2	1	3	4	4	4
2	3	1	4	4	4	1	3	2	2	1	3
3	1	2	2	3	1	4	4	4	1	3	2
4	4	4	3	1	2	3	2	1	3	2	1

and again

Negative Permutations.

1	2	3	4	4	4	2	1	3	2	1	3
2	3	1	1	2	3	4	4	4	1	3	2
4	4	4	2	3	1	1	3	2	3	2	1
3	1	2	3	1	2	3	2	1	4	4	4

I reject from the permutations of each species all those where 1 or 3 or both appear in the 4th place, and also those where 2 or 4 or both appear in the 1st place, for these will be presently seen to give rise to diagonal products which are zero.

The permutations remaining are

Positive effectual permutations.

1	3	3	1
2	1	4	3
3	2	1	4
4	4	2	2

Negative effectual permutations.

3	1	1	3
1	4	3	2
4	3	2	1
2	2	4	4

I now accordingly form four positive squares, which are

$a b c o$	$l m n o$	$l m n o$	$a b c o$
$o a b c$	$a b c o$	$o l m n$	$l m n o$
$l m n o$	$o a b c$	$a b c a$	$o l m n$
$o l m n$	$o l m n$	$o a b c$	$o a b c$

Drawing diagonal lines from left to right, and taking the sum of the diagonal products, I obtain $a^2 n^2 + lb^2 n + l^2 c^2 + am^2 c$. Again, the four negative squares

$l m n o$	$a b c o$	$a b c o$	$l m n o$
$a b c o$	$o l m n$	$l m n o$	$o a b c$
$o l m n$	$l m n o$	$o a b c$	$a b c o$
$o a b c$	$o a b c$	$o l m n$	$o l m n$

give as the sum of the diagonal products

$$l b m c + a l n c + a m b n + l a c n$$

be

$$i. e. l b m c + a m b n + 2 a c l n.$$

Thus the result of eliminating between $a x^2 + b x + c = 0$
 $l x^2 + m x + a = 0$

ought to, and is

$$a^2 n^2 + l^2 c^2 - 2 a c l n + l b^2 n + a m^2 c - l b m c - a m b n = 0.$$

Rule for finding the prime derivative of the 1st degree, which is of the form $A x - B$.

1. Begin as before, only attach one zero less to each progression; we shall thus obtain *not* a square, but an oblong broader than it is deep, containing $(m+n-2)$ rows, and $(m+n-1)$ terms in each row: in a word, $(m+n-2)$ rows, and $(m+n-1)$ columns.

To find (A) reject the column at the extreme right, we thus recover a square arrangement $(m+n-2)$ terms, broad and deep.

Proceed with this new square as with the former one; the difference between the sums of the positive and negative diagonal products will give A.

To find B, do just the same thing, with the exception of striking off not the last column, but the last but *one*.

Rule for finding the prime derivative of any degree, say the r th, viz. $A_r \cdot x^r - A_{r-1} x^{r-1} + \dots \pm A_0$.

Begin with adding zeros as before, but the number to be added to the (a) progression is $(m-r)$ and to the (b) progression $(n-r)$.

There will thus be formed an oblong containing $(m+n-2r)$ rows, and $(m+n-r)$ terms in each row, and therefore the same number of columns.

To find any coefficient as A_s , strike off all the last $(r+1)$ columns except that which is (s) places distant from the extreme right, and proceed with the resulting squares as before.

Through the well-known ingenuity and kindly proffered help of a distinguished friend, I trust to be able to get a machine made for working Sturm's theorem, and indeed all problems of derivation, after the method here expounded; on which subject I have a great deal more yet to say, than can be inferred from this or my preceding papers.

University College, London, Jan. 16, 1840.

[To be continued.]

XXVI. *New Researches on the true nature of the Boetian Contractions, especially with reference to the Explanation given by M. Chasles.* By J. O. HALLIWELL, Esq., F.R.S., F.S.A., F.R.A.S., &c.*

I HAVE the pleasure of placing before the readers of the Philosophical Magazine a complete explanation of the first tract in No. 343 of the Arundel MSS. in the British Museum; and that I have been able to accomplish this desideratum affords me the greater gratification, because in so doing I am fulfilling the wish of the patriarch of English Literature†.

The manuscript referred to, sometimes called the *Mentz Manuscript*, is a small quarto of the twelfth century, on vellum; and the first tract, entitled *de Arte Numerandi*, consists of four leaves only, unfortunately being imperfect at the end. A fragment from the recto of the first folio is lithographed in the appendix to the *Rara Mathematica*, which serves to show the style of the manuscript and the forms and names of the contractions.

The treatise itself commences with an explanation of the increasing value of igin, andras, &c., in the different abacal compartments; in point of fact, a definition of abacal numeration dependent upon the principle of local decimal value. It is important to notice that, after this explanation, the compiler gives the usual definitions of *digiti* and *articuli*, clearly showing by that his comprehension of their future value. It is remarkable that everything stated is subservient to multiplication and division, no notice whatever being taken of addition, subtraction, duplation, or mediation;—a plain proof, if any were needed, that when the boundaries were abolished, and when an attempt at a generalization of the local system was made, artificial methods were adopted to come to the same conclusions. Now I would ask M. Libri, or any one who agrees with him, how he can possibly account for such a clumsy, primitive, yet most ingenious, method of avoiding *abacal* difficulties, if we suppose that the writers of the thirteenth and following centuries derived their arithmetical knowledge direct from the Arabs?

And now for the *modus operandi*: and in order to render it intelligible to every reader, let us take the first example in multiplication, on account of its great simplicity:—

“Sint ergo iiij. pedes equi, unusquisque habens vj. clavos.”

Arbas is to be placed in the lower part of the singular arc

* Communicated by the Author.

† Hallam's Introduction to the Literature of Europe during the Middle Ages, vol. i. p. 151.

(arcus singularis), and in the upper part of the same arc is placed chalcus "quasi fundamentum multiplicationis." But in the actual multiplication recourse is had to the common Roman notation, and the result of the multiplication of arbas and chalcus in the singular arc is xxiii. Then the system of *articuli* comes into operation, and the *articulus* of this number (24) is andras, which, *by the principle of local position and of no other*, is placed in the *decenal* arc. Now I would ask M. Libri, in reply to every one of his arguments, how can we possibly suppose a rule of this nature with its full explanation to exist, without allowing its author to have possessed the knowledge of the value of local position? The decenal arc is made use of in a simple but masterly manner, and the articulate system is invented to avoid the principal difficulty. The digit arbas, it is almost unnecessary to observe, is placed in the singular arc, and thus we have the complete number represented.

In higher numbers the centenal, millenal, and other arcs come into use. The following rule is a fair specimen of the methods employed:—

"Cum autem per decenum multiplicabis singularem, dices hanc regulam deceni;—Decenus quemcunque arcum multiplicat, in secundo ab eo pone digitum in ulteriori articulum," fol. 2, r^o , the reason of which is obvious. Thus, in the MS., the operation for finding the square of twelve is as follows:—

	2	
1		4
	2	

		6
1	4	4
	2	4

proceeding in a most complicated manner, but merely using the simple formula

$$m \cdot (np) = mnp, \text{ or, } 12 \times 12 = 12 \times 2 \times 6 = 24 \times 6.$$

in which latter case the above rule is applicable. This rule is afterwards generalized.

"His patefactis, oculus mentis aperiatur ad subtilitatem divisionis;" but as the same system is carried out, precisely similar to the methods of Johannes de Sacro-Bosco, I do not consider it at all necessary to repeat them.

Gerbert uses the Boetian fractional notation*, and I consider this fact a grand argument for his acquaintance with the Boetian contractions, if, indeed, the passage in the geometry

* Pezii Thesaurus, tom. i. pars ii. col. 13. "Quod abacistæ facillimum est." *Ibid.* col. 30.

of Boetius was not introduced by him. It does not appear to me that much authority ought to be given to the well-known passage of William of Malmesbury,* as far as it is supposed to prove that Gerbert brought the knowledge of the abacus from Spain: and, as Professor Peacock† so well observes, “the passage of this historian contains no certain intimation of the knowledge of the notation by nine figures and zero, as the rules which would be thence derived, would tend rather to relieve than increase the labours of the sweating calculators,”—*quæ a sudantibus abacistis vix intelliguntur*. Now had the question of the Boetian contractions been broached when Professor Peacock composed his history of arithmetic, he would immediately have seen how evidently this passage refers to them, and this supposition would have explained his doubts in the remaining part of his argument.

In the treatise of Berhelinus in the Bodleian library‡, the Boetian contractions occur explained by *Greek* numerals,—a most singular and important fact, and one which affords a very strong argument for what M. Chasles has stated at p. 474 of his *Aperçu Historique*. *En passant*, this is also an argument for the antiquity of this artificial abacal system.

Again, what difference is there between the system of the Greeks, the system in the Mentz Manuscript, the system in the passage in Boetius as satisfactorily explained by M. Chasles, and the Arabic method? I mean with regard to first principles§. All, in fact, are contained in the following formula, which is the general expression for any finite number:—

$$N = a_m 10^m + a_{m-1} 10^{m-1} + a_{m-2} 10^{m-2} + \dots + a_1 10 + a_0,$$

where $a_0, a_1, a_2, \dots, a_m$ are digits, or integers less than the radix 10.

XXV. *On the Permeability of various bodies to the Chemical Rays.* By ROBERT HUNT.||

HAVING many years since repeated, with much interest, the experiments of Wedgwood, Davy, and Wollaston on the chemical influence of light, it was with much pleasure

* Wright's Essay on Anglo-Saxon Literature (p. 66).

† History of Arithmetic, p. 415.

‡ I possess a transcript of this manuscript, but, having mislaid it, am compelled to defer any commentary on it till M. Chasles has published his edition.

§ M. Chasles, *Aperçu Historique*, p. 474.

|| Communicated by the Author.

that I read Mr. Talbot's paper on photographic drawing, which opened to me new views, and pointed out paths rich in the promise of important results.

The vast sum of delight which the pursuit of this subject during the past year has afforded me, makes me a large debtor to that erudite gentleman, which I thus humbly, but sincerely, acknowledge.

My first endeavours in the photographic art were directed to restoring the natural order of light and shadow; and I fortunately succeeded in effecting this very early in the summer of 1839. My next were to improve the camera for photographic purposes, in which object I was most materially assisted by Mr. John Towson, of this town, who, having directed much of his attention to optics, furnished me with information and instruments by which my progress was greatly accelerated*.

Having, in conjunction with this gentleman, while trying a variety of lenses, been often perplexed by the dissimilar results obtained on the same paper from different kinds of glass, I was induced to commence a series of experiments on the interference of transparent bodies to the permeation of chemical light.

The same subject has, I am informed, engaged the attention of two scientific inquirers on the continent; but beyond a brief notice of M. Edmond Becquerel's experiments in the *Athenæum*, No. 621, I am perfectly unacquainted with the methods or results of their observations.

Being anxious to obtain a measure of the interference of the various bodies I was about to examine, I constructed a very delicate galvanometer—the coil being of ribbon copper and the needles of French watch-spring. To this instrument I connected, by platina wires, a U tube, as suggested by M. Becquerel in his *Traité d'Electricité*, which held in one arm a solution of nitrate of silver, and in the other a solution of iodide of potassium. Every part of the tube was screened from light, except the lowest point, at which the fluids met. On this point, by means of a powerful lens, a concentrated pencil of light was thrown, which was made to pass through the bodies to be examined. The force of electro-chemical action being dependent on the quantity of chemical light impinging on the exposed portion of the fluids, led me naturally to conclude that the deflections of the needle would furnish very accurate comparative results. I have also tried the plan M. E. Becquerel adopts, of floating one photometric fluid upon

* See L. and E. Phil. Mag. for November last, vol. xv. p. 381.—EDIT.

another. Experience has, however, convinced me that the galvanometer, although capable of being made in the hands of a skilful manipulator a very accurate measurer of the diurnal variations of the quantity of chemical rays in the solar beam*, cannot be depended on where a series of nice comparisons are required. I have never yet been enabled to arrive at precisely the same results by this instrument in any two sets of observations; every thin cloud or the lightest smoke materially altering the deflections. I have, however, found it of use in giving me near approximations to a correct arrangement. I proceed thus: having by the galvanometer tabulated a number of bodies, I select those whose interference seems to approach near each other, and place them in regular order, under the same circumstances, upon a sheet of highly sensitive photographic paper in a dark room; then opening the window-shutters, expose it for three minutes to the direct influence of the solar rays, or for twice that time to diffused daylight; again darkening the apartment I examine the tints at which the paper has arrived under each body, and mark their correspondence or otherwise with the observations by the galvanometer. By carefully repeating many times each set of experiments, I am enabled to correct small errors of observation.

I use yet another method to test the correctness of the foregoing processes, which consists in filling a camera with the fluid or gas to be examined, or interposing the solid body and receiving the sun's image on a disc of silvered copper, prepared according to the principles of the Daguerreotype.

As many simple contrivances will suggest themselves to those who are desirous of repeating the experiments, it may be sufficient for me to state, that my apparatus is simply one cylinder sliding within another for the purpose of adjusting the focus to the different densities of the bodies, and that the photographic disc is protected from the fluid or gas by a piece of tested plate glass well greased around the edges, as are also the cylinders, throughout their length.

This plan may appear open to some of the objections I have urged against the galvanometer; but, as from the sensitiveness of the preparation an exposure of thirty seconds is sufficient, you are enabled to select your moments of observation, and

* Long prior to the publication of the speech of M. Arago on the report of the Commission on the Daguerreotype, both Mr. Towson and myself had remarked that the light of morning acted more powerfully on photographic preparations than the evening light. The paper which at nine in the morning became in ten minutes a rich purple bronze, took almost twice that time to reach the same hue at three in the afternoon.

using the galvanometer at the same time to mark the intensity of light, try every substance under precisely the same circumstances. Having completed the exposure of a series, I place all the discs in the mercurial vapour-box together, and the instant the impression appears the strongest, remove them and carefully compare effects.

The following are the results I have arrived at by these means. At the head of each series I have placed the mean permanent deflection of the galvanometer needle, from ten careful experiments with each of the bodies included within it. By this means a comparative view is given of one series with another.

Series 1.—*Deflection* $22^{\circ} 30''$.

Nitrogen
Atmospheric air
Oxygen
Hydrogen
Carbonic Acid
Carbonic Oxide
Steam (invisible)
Nitrous Oxide
Water
Alcohol (absolute)
Æther (sulphuric)

Series 2.—*Deflection* 20° .

British Plate Glass
Iceland Spar
Carbonate of Soda
Nitrate of Potassa
Ditto, *fused and opaque*
Camphor
Sulphuric Acid
Hydrocyanic Acid (Scheele's)
Nitric Acid

Series 3.—*Deflection* $18^{\circ} 80''$.

Crown Glass
Flint Glass
Mica
French Plate Glass
Alum
Gum Arabic
B. Plate and Crown Glass

German Plate (with a pink shade)

Two pieces of Crown Glass
Purple Fluor Spar
Nitrous Acid Gas
Iodine Vapour

Series 4.—*Deflection* $17^{\circ} 15''$.

B. Plate and Flint Glass
Flint and Crown Glass
Three pieces of Crown Glass
Three laminæ of Mica
Creosote

Oil of Aniseed (German)
——- Peppermint (English)
——- Rosemary
——- Savine

Four pieces of Crown Glass
Amber

Green Fluor Spar
Oil of Lavender

——- Caraways
——- Cloves

Canada Balsam

Series 5.—*Deflection* $4^{\circ} 35''$.

Green Bottle Glass*

Chlorine

Protoxide of Chlorine

Bromine (vapour)

——- (liquid)

Lightly-smoked Glass

* I have been much surprised at some of the manufacturing chemists in London sending out their hydrocyanic acid and other easily decomposable preparations in bottles of *purple* glass, which offers no interruption to the chemical rays. Dark green glass should be substituted.

It is necessary I should state that the results are likely to be differently recorded by different observers, unless the same photographic preparation is used in all cases. I have been in the habit of using a paper washed with a solution of the muriate of baryta and nitrate of silver, while it is yet damp. The sensitiveness of this preparation may be shown by the fact of its being acted on very decidedly in five minutes by a gas flame from a ten-holed Argand burner. On this paper the tints are blue under greenish glasses, while under those inclining to a yellow they are reddish. If a paper prepared with a solution of the chloruret of soda be used, the tints are reddish under the green, and bluish under the pure white or yellowish glasses.

The above list does not contain all the bodies I have examined, but they are all I am satisfied to place in a determinate position.

Not having deduced any fixed principles from my observations, I may appear to act prematurely; but as it is probable the same subject may be engaging those whose minds and means are superior to my own, and as it is possible even my humble experiments may be of service to such persons, I feel myself excused from the charge of obtrusiveness.

12, Cornwall Street, Devonport, January 2, 1840.

XXVI. *On an anomalous Electric Condition of Iron.* By
MARTYN J. ROBERTS, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT is now some months since, that while prosecuting a series of novel galvanic experiments, I discovered a singular anomaly in the electric condition of iron, which is, that although iron if associated with copper as a galvanic pair is highly positive to the copper, yet when associated with zinc, it is more highly negative to the zinc than copper would be under similar circumstances; or in other words, that although copper and iron form a galvanic combination, in which the iron is in the same relation to the copper that a zinc plate would be, yet that iron and zinc form a galvanic pair that has a greater power of generating electric action than a similar sized pair of copper and zinc. This singular phænomenon will, I believe, lead the way to some important discoveries; but not to occupy too much space in your valuable Journal, I will without further comment give extracts from my notebook of some experiments made by me on this subject.

Jan. 1st, 1839. A galvanic combination of iron and zinc was put in communication with two poles of a differential galvanometer; a like-sized combination (or gal. pair) of copper and zinc was connected with the other two poles of the diff. galvanometer: Deviation of needle in favour of the iron and zinc pair = 25 degrees.

Feb. 27, 1839. Experiments made on the comparative power of two galvanic batteries fitted up on Wollaston's plan (that is with the negative plate opposed to both surfaces of the zinc or positive plate.) The size of the plates was the same in both batteries, viz. zinc plate $2\frac{1}{8}$ inch. by $2\frac{1}{2}$ inch. The number of pairs in each battery was ten. The only difference between the two batteries was, that the negative plates in one were of copper and in the other of iron.

The exciting solution was dilute sulphuric acid, which was not renewed during the experiment, but the experiment was continued until the acid was exhausted. The power of each battery was applied to the decomposition of water, and the gas collected was the measure of the power.

RESULTS.

Battery of Copper and Zinc.

The first cubic inch of gas obtained in 33 minutes.
Then one half cubic inch ... in 92 minutes.

Acid was now exhausted. $1\frac{1}{2}$ cubic inch in 125 minutes.

Battery of Iron and Zinc.

First cubic inch obtained	in	7 minutes.
Second — — —	in	9 minutes.
Third — — —	in	26 minutes.
Fourth — — —	in	62 minutes.

Acid now exhausted. 4 cubic inch. in 104 minutes.

Iron battery with a measured quantity of acid gave 4 cubic inches in 104 minutes.

Copper battery with a like quantity of acid gave only $1\frac{1}{2}$ cubic inch in 125 minutes.

I have the honour to remain,
Gentlemen, yours, &c.

Dec. 16, 1839.

MARTYN J. ROBERTS.

XXVII. *Notices respecting New Books.**Curtis's British Entomology.*

WE are happy to announce the publication of the Preface, General Indexes, &c. to this beautiful work, which is now *completed*, after sixteen years of almost unremitting application on the part of the author. The sixteen volumes which, if arranged systematically as proposed by Mr. Curtis, will form eight, contain illustrations of all the Genera of Linnæus, Fabricius, and Latreille that are recorded as native groups, as well as most of the interesting discoveries that have been made for many years past, comprising 770 copper-plates, giving faithful figures of our wild flowers, as well as the insects, beautifully coloured and finished with the greatest care. The letter-press, amounting to nearly 1700 pages, although scientific, contains concise accounts of the history and œconomy of every group that is interesting and familiar to us, such as the Hive-Bee, Wasps, Cockroaches, Molecrickets &c.; and there are two thick volumes of the Butterflies and Moths.

XXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xv. p. 544.]

Nov. 6. A NOTICE of showers of ashes which fell on board the Roxburgh, at sea, off the Cape de Verd islands, February, 1839, by the Rev. W. B. Clarke, F.G.S., was first read.

The object of this communication is to register an interesting occurrence, though the author possesses no direct evidence of its probable cause.

On February 2, when the Roxburgh was in latitude $21^{\circ} 14' N.$, long. $25^{\circ} 6' W.$, the wind, which had blown from the north-east during the passage from Plymouth, changed to the east and south-east, and was accompanied with a thick haze of a peculiar kind. The same description of weather prevailed on the 3rd, when the ship was off St. Anthony, one of the Cape de Verd islands.

On Feb. 4, the latitude at noon was $14^{\circ} 31' N.$, long. $25^{\circ} 16' W.$ The sky was overcast, and the weather was thicker than before and insufferably oppressive, though the thermometer was only 72° . At 3 P.M. the wind suddenly lulled into a calm, then rose from the south-west, and was accompanied with rain, and the air appeared to be filled with dust, which affected the eyes of the passengers and crew. At $10\frac{1}{2}$ P.M. the wind returned to the east and blew strongly. During the continuance of the haze, which was as thick as a November fog, and extended all around the horizon, dust was gradually deposited on every part of the ship that offered a lodgement. At noon, on the 5th of February, the Roxburgh was in lat. $12^{\circ} 36' N.$, long. $24^{\circ} 13' W.$, thermometer 72° , barometer 30° , the height at which it had stood during the voyage from England. The volcanic island Fogo,

one of the Cape de Verds, was about 45 miles distant. The weather was clear and fine, but the sails were found to be covered with an impalpable reddish-brown powder, or a kind of triturated pumice, which Mr. Clarke says resembled many of the ashes ejected from Vesuvius, and was evidently not sand blown from the African desert. On the 6th the wind returned to the south-east, and the weather afterwards resumed its ordinary characters.

The circumstances connected with these atmospherical changes induced the author to infer that they were due to an eruption in the Cape de Verd group.

In June, 1822, the ship *Kingston*, of Bristol, bound to Jamaica, while passing near Fogo, had her sails covered with a similar brownish triturated pumice, which it is stated smelt strongly of sulphur.

Mr. Clarke also mentions the following instances of similar phenomena on the authority of the officers of the *Roxburgh* :—

In the lat. of the Canaries, and long. 35° W., showers of ashes have been noticed two or three times. At Bombay the decks of the vessels were on one occasion covered to the depth of an inch with dust, which was supposed to have been blown from Arabia.

In January, 1838, dust was noticed by the crew of a vessel navigating the China sea, a considerable distance from the Bashee islands, one of which had been previously seen in eruption*.

In 1812, ashes fell on the deck of a packet bound to the Brazils, and when 1000 miles from land.

Mr. Clarke also mentions the ashes which fell at sea during the eruption of Vesuvius in 1822, and 400 miles from that volcano; likewise the reddish dust which fell in the south of Italy and in Sicily on the 16th May, 1830, as well as in 1807 and 1813, during eruptions of Etna, and at first attributed to those outbursts, but afterwards found to be sand similar to that of the desert of Africa. During the eruption in May, 1830, a caravan perished beneath a whirlwind of sand, and similar storms occurred during the eruptions of 1807 and 1813†.

A letter was then read from Mr. Caldcleugh, dated St. Jago de Chili, 18th February, 1839, containing the following translation of the declaration of the master and crew of the Chilian schooner *Thily*.

“I, the undersigned, Joseph Napoleon Escofier, master of the said vessel, and with the corroborating evidence of my crew, declare as follows :—

“This, the 12th day of February, 1839, at ten minutes past 9 o'clock in the morning, being in lat. $33^{\circ} 32'$ S., and $74^{\circ} 32'$ W. long. of the meridian of Cadiz ($80^{\circ} 51'$ W. of Greenwich), we felt an earthquake, which lasted more than a minute. The noise which accompanied it was similar to that caused by the running out of a heavy chain cable. At fifteen minutes past 7 o'clock of the same evening we saw an island rising out of the sea, of the height of Curauma

* Mr. Clarke believes that this is the first announcement of a volcano in that group.

† Bulletin, Soc. Geol. France.

Point, bearing south 79° W. by compass, distant from six to nine miles. A considerable time afterwards the island divided itself into the form of two pyramids, the most northern one crumbled away diagonally towards the north, and the southern one disappeared in pieces, the base however always remaining above the surface of the sea. At half-past 7 o'clock the same island appeared again, or its size became considerably increased, but shortly afterwards its summit became flattened. At thirty-five minutes past 7 o'clock two other islands appeared to the southward of the first. Of these, the most southern bore south 56° west. The three islands appeared to run in the direction of north and south. The sea broke with violence upon their shores and seemed violently agitated. In the distances between these islands nothing was visible but chains of rocks, among which a great explosion was discernible.

"At eight minutes before 8 o'clock, the most northern island was the only one visible: it appeared much higher than before, and of the shape of a sugar loaf. The darkness of the night prevented us seeing the other two islands.

"The following day, the 13th of the month, at a quarter past 1 o'clock in the morning, the larboard watch and myself saw at intervals a light in the same direction as the islands, south 72° west, which appeared to be caused by a volcano.

Position of the most northern island:

Long. W. of meridian of Cadiz, $70^{\circ} 33'$ ($76^{\circ} 52'$ W. of Greenwich.)

Lat. S..... 33 34.

Position of the most southern island:

Long. W. of meridian of Cadiz, $70^{\circ} 34'$ ($76^{\circ} 53'$ W. of Greenwich.)

Lat. 33 40.

"I consider my longitude to be correct from having sighted Juan Fernandez on the 11th, at 8 o'clock in the morning, and compared its bearings with my latitude by observation.

"Signed, &c. &c. &c."

Mr. Caldcleugh adds, the master of another vessel reported that the islands bore 30 leagues due east of Juan Fernandez; and that a ship had been despatched from Valparaiso to discover whether they remained above water or had crumbled away.

The larger Curauma Point, referred to in the declaration, is a bluff point, about 400 feet in height, and situated to the southward of Valparaiso.

A letter was next read, addressed to Charles Lyell, Esq., V.P.G.S., by John Buddle, Esq., F.G.S., on depressions produced in the surface of the ground by excavating beds of coal.

Subsidence of the surface invariably follows the working of the subjacent beds of coal where sufficient supports are not left, but the extent of the subsidence is governed by the following circumstances:

1st. The depth of the seam of coal below the surface.

2nd. The thickness of the seam or seams removed.

3rd. The nature of the strata between the surface and the seams of coal.

4th. Whether the pillars of coal are wholly or partially worked.

If the depth from the surface does not exceed 30 fathoms, and sandstones form the predominant strata, the subsidence is about equal to the thickness of the seam of coal removed; but if metal-stone constitute the greater portion of the intervening mass, the amount of depression in the surface is less. This rule is considered to hold good at all depths.

The degree of subsidence does not depend so much on the thickness of the bed of coal, as on the entire removal of it; but Mr. Buddle states, that he has had no opportunities of making correct observations on the relative effect produced in the surface. If a considerable portion of the coal be left, although quite inadequate to the support of the superincumbent strata, the subsidence is retarded. This is more particularly the case in the Newcastle system of working, where rectangular pillars are left in the first instance and afterwards removed. In working these pillars, stooks or blocks of coal of considerable strength are left as props to protect the colliers from the exfoliation of the roof; and though a subsidence of the superincumbent strata invariably takes place, yet the extent in the first instance is governed by the degree of resistance of the stooks; and it frequently happens, that a large tract of a coal mine remains for several years only filled in part, and without any perceptible change occurring. In course of time, however, from the exfoliation of the stooks and the operation of the atmosphere, a further subsidence, called a second creep, takes place, and generally continues until the excavation is completely closed.

In the Yorkshire system, by which all the coal is taken out in the first instance, except small pillars, the roof being principally supported by wooden props and stone pillars, the subsidence of the strata takes place immediately after the coal is removed, and there is no second settlement.

It is only where water accumulates on the surface or a railway traverses a coal-field, that the amount of subsidence can be accurately ascertained.

In one instance, mentioned by Mr. Buddle, the excavation of a bed of coal 6 feet thick, one-fourth having been left in "stooks," the depth of the bed from the surface being 100 fathoms, and the overlying strata principally sandstone, the amount of subsidence was shown by the accumulation of a pond of water, to have been rather more than 3 feet deep.

In another instance, it was found necessary to restore the level of a railway three times, in consequence of three distinct sinkings of the surface having followed the successive excavating of three seams of coal. The tract in question is of a quadrangular form and about 23 acres in area, and contains the following five seams of coal:

Coal.	Depth below the surface. fath.	Thickness. ft. in.
1. The three-quarter seam	54 $\frac{1}{2}$	1 8
2. The five-quarter	62	3 6
3. The high main	73	6 3
4. The Maudlin.....	83 $\frac{1}{2}$	5 0
5. The Hutton	107	3 8

The high-main seam was first worked, then the Maudlin, and afterwards the Hutton, and the removal of each was attended with a depression in the line of the railway. The extent of each settlement was not measured, but the whole amount was 5 feet 6 inches, the aggregate thickness of the seams being 14 feet 11 inches. This small effect Mr. Buddle explains by showing, that the railway passes near one end of the excavated tract, and that metal-stone predominates over sandstone in the superincumbent strata. The working of the five-quarter seam is now in progress, and the effects occasioned by the removal of the three lower seams are well exposed. Innumerable vertical cracks pass through the coal, its roof and pavement, but they are perfectly close except around the margin of the settlement. Along this line the strata are bent down, the cracks in the pavement are frequently open, forming considerable fissures, the coal is splintered, and the roof-stone is shattered. In the interior of the settlement the pavement is as level and smooth as if it had never been disturbed, and the cracks are quite close, passing through the seam without splintering it or producing any effect except that of rendering it tougher, or, in the language of the colliers, "woody." This effect, Mr. Buddle conceives, may be attributed to the escape of the gas, and he states that it is sometimes produced by other operations, when the coal is said to be "winded." The smoothness of the pavement, he is of opinion, is due to the direct downward pressure of the superincumbent mass; and he states, that he has never noticed any tendency to a sliding or sideway movement in any subsidence of strata occasioned by the working of the coal, except the slight obliquity occasioned by the offbreak at the sides of the settlement.

[To be continued.]

ROYAL ASTRONOMICAL SOCIETY.

December 14, 1839. The following communications were read:—

On the parallax of Sirius. By Thomas Henderson, Esq. Astronomer Royal for Scotland.

The parallax of Sirius, the brightest star in the heavens, has been several times the subject of investigation among astronomers. From the variations of the zenith distances observed at Paris, the second Cassini inferred a parallax in declination amounting to six seconds of space; and, from similar variations in the observations of La Caille made at the Cape of Good Hope, some astronomers have deduced a parallax in declination of four seconds. Piazzi has also obtained from his observations a parallax of the same amount. On the other hand, La Caille's observations of zenith distances made at Paris, more numerous and certain than those made at the Cape, do not exhibit any sensible parallax; and the observations which have since been made in the observatories of Europe, would appear to lead to the same result, as no parallax has ever been deduced from them. In the *Fundamenta Astronomiæ*, M. Bessel has investigated, from Bradley's Observations of Differences of Right

Ascension of Sirius and α *Lyrae*, the sum of the parallaxes of the two stars, and has found it to be an insensible quantity.

The extensive series of observations of Sirius, made with the mural circle of the Observatory at the Cape of Good Hope, is well adapted for the investigation of the parallax, as the observations possess some advantages over those made in Europe. The star is near the zenith of the Cape, and the temperature is nearly the same when it passes the meridian at noon in June, and at midnight in December, the periods of the greatest parallaxes in declination; so that the irregularities and uncertainties of refraction, which affect observations in Europe, may be supposed to disappear.

From May 1832 to May 1833, ninety-seven observations of Sirius were made by Mr. Henderson with the mural circle at the Cape Observatory, of which sixty-three were made by direct vision, and thirty-four by reflection; and in Mr. Maclear's printed observations of zenith distances, made with the same instrument, there are sixty-seven observations of the double altitude of the star, made between August 1836 and December 1837. Each of these series of observations was made in one position of the telescope upon the circle, so that in each series the similar observations were referred to the same divisions.

The observations made by Mr. Henderson have been reduced in the same manner as were those of α *Centauri*, given in his memoir on the parallax of the latter star. (*Monthly Notice*, Vol. IV. No. 19.) The declinations of Sirius have been determined by comparisons with such of the principal or standard stars as were observed on the same day; and it is consequently assumed that, in the observations of the stars of comparison, any errors which may arise from supposing their parallaxes to be insensible, and the coefficient of aberration to be correctly assumed, neutralize each other. The mean declinations of the standard stars of comparison have been taken from the catalogue annexed to the author's "Memoir on the Declinations of the Principal Stars:" the absolute places of the stars are not required, but only their relative positions with regard to each other.

Mr. Maclear observed the double altitude of Sirius, or the angular distances between the star seen by direct vision and by reflexion at the same transit over the meridian. These are independent of the observations and assumed positions of other stars, and are affected by twice the amount of the parallax in declination. Observations of this description appear to be the best adapted of any which can be made with the mural circle for the investigation of small variations in the declination of a star.

In the reductions of Mr. Henderson's series the constant of aberration has been assumed = $20''.50$; of Mr. Maclear's, = $20''.36$; in the reductions of both the coefficient of lunar nutation has been assumed = $9''.25$, and the annual precession and proper motion have been taken from the *Tabulæ Regiomontanæ*.

Mr. Maclear's observations are suitable for determining the constant of aberration; the correction to be applied to it has been

therefore introduced as an unknown quantity into the equations of condition; and, from the value which is obtained, we may judge of the degree of accuracy with which the parallax is determined.

On resolving, by the method of minimum squares, the two sets of equations, and combining the results according to their relative weights, the greatest effect of parallax in declination is found, from the whole of the 231 observations, $= + 0''.15$; and the greatest effect of aberration in declination, $= 13''.07$. These quantities are to the total effect of parallax and aberration in the proportion of $13''.13$ to $20''.50$, whence the final results are—

Parallax of Sirius (or the angle subtended by the radius of
the earth's orbit, at a distance equal to that of the star). $= 0''.23$
Constant of aberration $= 20.41$

The error of this determination of the parallax may be estimated not to exceed a quarter of a second, as it is almost certain that the constant of aberration is not in error to a greater amount. On the whole, it may be concluded that the parallax of Sirius is not greater than half a second of space, and that it is probably much less.

A Catalogue of Twenty-seven Stars of the Pleiades. By M. Bessel, Director of the Observatory Königsberg.

The catalogue was computed by M. Bessel from meridian observations made by himself and his assistant, Dr. Busche. It contains the positions, annual precession, and its secular variations in R and declination, together with the proper motions, and a comparison with Piazzi's catalogue.

In a letter addressed to Mr. Baily, containing the above catalogue, M. Bessel announces, that the observations respecting the parallax of 61 *Cygni** have been continued through a second year; and that the result of this new series will agree very nearly with that of the first. The publication of the observations will be delayed for a few months, in order to obtain a more certain determination of the proper motions which the two small stars compared seem to possess; and he adds, that although the *weight* of the former result was sufficiently great to leave no doubt about the real existence of the parallax, it is gratifying to see its quantity so very nearly confirmed by a second series of observations.

A Letter from M. Valz, Director of the Observatory at Marseilles, to the President, Sir J. F. W. Herschel, Bart., relative to the Variation of the Apparent Diameter of Encke's Comet.

After adverting to some objections suggested by Sir John Herschel (*Memoirs of the Royal Astronomical Society*, vol. vi. p. 102†) to the theory by which M. Valz explains the changes observed in the apparent diameters of some comets, when near their perihelia, namely, the condensation of volume produced by the pressure of an ethereal medium growing more dense in the vicinity of the sun, the

* Abstracts of M. Bessel's observations on the parallax of the star 61 *Cygni* will be found in Lond. and Edinb. Phil. Mag. vol. xiv. p. 68. 226.—ED.

† An abstract of Sir J. Herschel's paper appeared in Lond. and Edinb. Phil. Mag. vol. ii. p. 222.—EDIT.

author proceeds to give his own observations on Encke's comet, at the time of its last perihelion passage in 1838, when it appeared under circumstances favourable for observing the nebosity. He states that he was able to follow the comet till the evening before the perihelion passage; that he observed it to diminish rapidly, and, after being prodigiously reduced, to melt away, as it were, under his eyes, disappearing only in consequence of its extreme smallness, inasmuch as its brilliancy should, from its position, have continued to increase. The observations are as follows:—

On the 9th and 10th of October, the nebosity subtended an angle of $20'$, but it diminished continually after that time. On the 15th of October, he first remarked it to be elongated in the direction of the sun; and the elongation continued to increase until the 25th of October, when the greater diameter appeared to be double the smaller, after which it began to diminish. The most luminous part was not at the centre, but at the point opposite the sun. On the 25th of October, the nebosity was reduced to $15'$, and the real volume was then eighteen times smaller than on the 10th. On the 6th of November, the nebosity was $13'$, and the volume reduced to 1–40th. On the 13th of November, the nebosity was $11'$; on the 16th, between $8'$ and $9'$; on the 20th between $6'$ and $7'$; on the 23rd, $4'$; on the 24th, $3'$, and the real volume, 826 times less than on the 10th of October. On the 29th of November, the comet could no longer be seen in the evening twilight, but it reappeared on the morning of the 7th of December*. On the 12th of December it appeared as a star of the fifth magnitude; and its diameter was less than $20''$, being entirely covered by a wire of that thickness. The volume deduced from this apparent magnitude would be 80,242 times less than on the 10th of October. On the 14th of December it appeared feebler, and equal to a star of the sixth magnitude, with which it was compared; its diameter was then estimated at $15''$. On the 16th the comet appeared as a star of the seventh magnitude, and its apparent diameter was from $10''$ to $12''$. On the 17th it was reduced to the eighth magnitude at most, and its apparent diameter was from $7''$ to $8''$. On the 18th of December it was entirely invisible, although stars of the seventh and eighth magnitudes were seen in its neighbourhood. From these comparisons it appears that the real diameter must have undergone a diminution from the 10th of December*, when it was first observed in the morning, until the 18th, when it finally disappeared.

A Letter from Professor Schumacher, to Francis Baily, Esq., announcing the Discovery of a Comet by M. Galle, assistant in the Berlin Observatory.

The comet was discovered on the 2nd of the present month, $17^h 45^m$ mean time (Berlin), in the constellation *Virgo*. Comparing it by the great refractor, with a star of the tenth magnitude (which star was immediately compared with γ *Virginis*), M. Galle obtained the following positions:—

* There must be some error here in the dates.—EDIT.

Sidereal Time, Berlin.			A.R. of Comet.			Declination of Comet.		
h	m	s	h	m	s			
11	1	14	12	38	25.18	—°	10	22.8
11	9	42	12	38	28.26	—2	10	13.9
11	21	45	12	38	32.38	—2	9	57.3
11	40	39	12	38	39.63			

These observations give its daily motion in \mathcal{R} , $+ 2^{\circ} 12'$, in decl. $+ 0^{\circ} 19'$. It has a well-defined point, as a nucleus, within the uniform nebula, which, opposite to the sun, expands in the form of a tail.

Tables for the Calculation of Precession, for the year 1825, of Stars observed by M. Bessel in the several Zones, from $- 15^{\circ}$ to $+ 15^{\circ}$ Declination. By Dr. Max. Weisse, Director of the Observatory at Cracow.

Observations of Moon and Moon-culminating Stars, Eclipses of Jupiter's Satellites, and Occultations of Fixed Stars by the Moon, made at the Observatory of Paramatta, in New South Wales, in the year 1838, by Mr. Dunlop. Communicated by Sir Thomas Macdougall Brisbane, Bart.

XXIX. Intelligence and Miscellaneous Articles.

THEORY OF SUBSTITUTIONS.—ACETIC AND CHLOROACETIC ACIDS.

M. DUMAS has lately read the following note on these subjects to the Royal Academy.

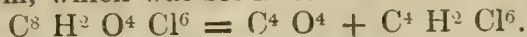
“In a previous memoir I have shown that chlorine decomposes acetic acid under solar influence, and that it gives rise to a new acid, which I call chloroacetic acid. On this occasion I expressed my opinion that acetic acid and chloroacetic acid belong to the same chemical type, one being represented by $\text{C}^8\text{H}^8\text{O}^4$; and the other by $\text{C}^8\text{H}^2\text{O}^4\text{Cl}^6$. I endeavoured to generalize this view, and to explain how these types might serve to group organic bodies into well-characterized classes.

“M. Berzelius, not admitting the theory of substitutions, has given, as soon as he became acquainted with my memoir, a refutation of the views announced in it. He considers acetic acid and chloroacetic acid as very different from each other, because they have not the same density, nor the same boiling point, nor the same odour, &c.*

“M. Berzelius has certainly not understood what I call the *fundamental properties* of a body, for I have long known that by replacing the hydrogen of a compound by chlorine it is rendered more dense and less volatile, and at the same time the density of its vapour is increased. It is therefore perfectly clear to me that the objections made by M. Berzelius are not at all directed to the views which I would really express. In order therefore to avoid any fresh misunderstanding, I shall endeavour to illustrate my opinion by an example. In causing chloroacetic acid to act upon an alkali, I observed a very remarkable reaction. The acid was converted into two new bodies, namely, carbonic acid, which combined with the alkali,

* See our last Number, p. 1.

and chloroform, which was set free. We have therefore



"I was convinced, and I had to a certain extent announced it in my memoir, that acetic acid would produce an analogous reaction; that is to say, under the influence of an excess of base it would change into carbonic acid and a carburet of hydrogen, the formula of which would be $C^4 H^8$. After some trials I perfectly succeeded in producing this remarkable reaction. It is only necessary to mix 10 grammes of crystallized acetate of soda with 30 or 40 grammes of caustic barytes, and to heat the mixture very slightly in a retort, to effect the conversion of the acetic acid into carbonic acid and a gas, the formula of which is $C^4 H^8$.

"This decomposition is quite perfect: the residue is perfectly white: not the slightest trace of oil or of pyroacetic spirit is disengaged, nor any vapour, except the water which accompanies the gas. The analysis of this gas by the eudiometer proved that it was formed, as is commonly stated, of one volume of vapour of carbon and two volumes of hydrogen. This is precisely the composition of a gas which chemists have never been able to produce, I mean light carburetted hydrogen (*gaz des marais*). It is impossible not to observe the connexion which exists between light carburetted hydrogen, produced by the spontaneous decomposition of vegetable substances, and that resulting from the final decomposition of acetic acid, which has been itself produced by the destructive distillation of wood.

"I intend to perform a complete examination of this gas, and to follow out an examination of reactions analogous to that which caused its discovery. At present I confine myself to announcing in a distinct manner that the gas $C^4 H^8$, corresponding to chloroform $C^4 H^2 Cl^6$, according to the theory of substitutions, has been produced by chloroacetic acid; that is to say, that acetic acid and chloroacetic acid possess the same fundamental properties as I had determined, and belong to the same organic type."—*L'Institut*, No. 313.

MYRONIN, MYRONIC ACID.—ESSENTIAL OIL OF MUSTARD.

It results from the experiments of M. Bussy that there exist in the farina of mustard seed two principles, the reaction of which, under the influence of water, gives rise to an essential oil. One of these is a peculiar acid, which M. Bussy calls *myronic acid* (*μύρον* essence), and the other is a substance which has great analogy with albumen, and which he calls *myronin*.

The properties of these substances are as follows:

Myronic acid is inodorous, it exists in mustard combined with potash. Myronate of potash is a salt which is soluble in water, perfectly crystallizable, inodorous, colourless, of a bitter taste, and decomposable by heat. The myronic acid, which may be isolated, combines also with soda, barytes, ammonia, and yields salts, which like the myronate of potash develop essential oil under the influence of myronin.

Myronin is a substance soluble in water, coagulable like albumen

by heat, alcohol, and acids; it has great analogy with emulsin, but neither albumen, emulsin, nor the synoptase of M. Robiquet can replace it for the production of essential oil of mustard. When put into contact with a solution of myronate of potash, it develops the odour of mustard, and the liquor submitted to distillation yields essential oil. It exists in black mustard, together with myronate of potash; but yellow mustard on the contrary contains myronin, but no myronate of potash.

It appears that M. M. Boutron and Fremy had simultaneously with M. Bussy discovered the above-described facts.—*L'Institut*, No. 313.

POLYCHROMATIC ACID.

M. Boutin has presented to the French Academy of Sciences a memoir on a new substance resulting from the action of nitric acid upon socotrine aloes. This product, which he calls *polychromatic acid*, is, in the opinion of the author, of considerable importance in dyeing and calico printing. By varying the mordants, it yields an infinite number of tints, all of them finer and more permanent than can be obtained by the usual processes. It has the appearance of a reddish brown powder, is very slightly soluble in water, but still sufficiently so to colour a large quantity of it at common temperatures; it is more soluble in alcohol, and in dyeing possesses the double advantage of yielding, in small quantities, much more colour than the substances usually employed.

This acid is susceptible of combining with metallic oxides, and of forming salts of different degrees of solubility, and all of different colours. Those which the author presented to the Academy were the salts of potash and silver.—*L'Institut*, No. 313.

CYANIL.

M. Boutin also gave an account to the Academy of a new substance which is formed by the action of nitric acid upon aloes, or rather upon polychromatic acid. This product is liquid and colourless when it has been purified by distillation from chloride of calcium, and has so great an analogy, on account of its poisonous properties, with hydrocyanic acid, that it is natural to conclude that they are isomeric bodies. It is so deleterious, that one or two drops in an eight-ounce bottle, half-filled, are sufficient to impart to the air which occupies the remainder of the bottle the power of immediately killing a bird which is made to breathe it; a capillary tube, slightly impregnated with this liquid, and put into the eye of a bird, produces also the same sudden effect.—*L'Institut*, No. 313.

ACTION OF ALBUMEN ON METALLIC SALTS.

M. Lassaigne has presented to the Academy of Sciences researches on the chemical action exerted by metallic salts on albumen and on certain animal tissues.

According to the author the experiments detailed in this memoir prove :

1st. That albumen has the property of combining with a great number of metallic salts without decomposing them, and forming with them compounds, which are insoluble in water, when they are united in certain proportions, but susceptible of dissolving in them severally when there is either an excess of albumen or of the metallic solution combined with it.

2nd. That these compounds, which may be designated by the name of *albuminates*, appear to result from the union of several atoms of albumen with one atom of metallic salt, as shown by the analyses which have been performed.

3rd. That these compounds possess the singular property of dissolving, without undergoing any immediate alteration, in the solutions of alkaline salts, which decompose the metallic salts when taken by themselves, and they remain dissolved for a shorter or longer period, according to the temperature.

4th. That it is extremely probable that when metallic salts are externally exhibited, there takes place in the œconomy, effected by absorption, an analogous combination between these salts, the tissues and the albumen contained in the various animal fluids, and that it is probable they are conveyed in the humours, and their medicinal effect is thus most commonly produced.

5th. That it would be interesting if physicians would ascertain the therapeutic effects of these compounds of albumen and metallic salts.

6th. That in the action of a metallic salt on any tissue, a combination is effected between these two bodies, which must modify its vital properties and effect a change in its functions.

7th. That the property of certain metallic salts of combining either with albumen or with the base of various tissues of our organs, generalizes what has been already stated with respect to bichloride of mercury and the same substances.—*L'Institut*, No. 313.

HAYDENITE.

M. Lévy has read the following notice respecting this mineral to the Academy of Sciences :—

“Cleaveland, in the second edition of his *Treatise on Mineralogy and Geology*, published at Boston [U. S.] in 1822, has given the name of haydenite to a mineral which had then been recently discovered by Dr. Hayden of Baltimore [U. S.]. He gives the following description of this mineral substance.—It is found in small crystals of a reddish colour, the form of which is cubic or slightly rhombic, and the surface of the faces vary from $\frac{1}{8}$ th to $\frac{1}{4}$ th of an inch square. It appears susceptible of ready decomposition, and becomes porous and spongy, but always retains its form. Before the blowpipe, it fuses with some difficulty into a yellow enamel; it is soluble in hot sulphuric acid, and the solution deposits small white needles. It has also been found accompanied with zeolite and carbonate of iron, in the fissures of gneiss at a mile and a half from Baltimore.

“The authors who have since mentioned this substance have merely repeated what has been stated by Cleaveland. Mr. Brooke, in his article on Mineralogy in the *Encyclopædia Metropolitana*, classes, without assigning any reason, haydenite with heulandite. I will add what appears to me singular, which is, that in a work recently published in the United States, entitled a *System of Mineralogy*, by James Dana, and printed at Newhaven in 1837, no mention is made of this species, though in other respects the work appears to be pretty complete.

“The cause of our ignorance respecting the nature of haydenite, may be explained by the small number of specimens which have been brought to Europe. M. Lévy then goes on to state that he had seen only three specimens of haydenite, and the account which he gives of it is as follows :—Haydenite is regularly crystallized ; the crystals have the form of a small oblique prism with rhombic bases, in which the incidence of the lateral faces is $98^{\circ} 22'$, and the incidence of the base, on each of the lateral faces, is $96^{\circ} 5'$. The crystals are frequently maced. The axis of revolution, around which one of the two crystals forming the macle is supposed to have turned 180° , is perpendicular to the base of the primitive form, and the face by which the two crystals are united is parallel to the same base. The crystals are thickly grouped together, and a small portion only of each is isolated. I have observed no modification either upon the edges or angles, so that the relation between the sides of the base and the lateral edges remains undetermined. The crystals cleave with the same facility on every face of the primary form. The cleavage faces sometimes present an uneven surface on account of small dark spots, as if the substance had suffered incipient decomposition. The crystals are usually covered by a thin layer of hydrate of iron, which is readily detached by the knife, and the faces of the crystals thus exposed are sufficiently brilliant to be measured by the reflective goniometer. The colour of haydenite is brownish yellow or greenish yellow ; the crystals are translucent and sometimes transparent, they are easily scratched by the knife, readily friable ; the hardness is nearly the same as that of fluor spar. The quantity detached was too small to admit of its specific gravity being taken.”—*L'Institut*.

BEAUMONTITE, A NEW MINERAL.

This is a new mineral discovered by M. Lévy. It accompanies haydenite, and is named in honour of M. Elie de Beaumont. It occurs in small brilliant crystals of a pearly lustre. Their form is that of a prism with square bases, terminated by obtuse pyramids. The summits of all the crystals are closely grouped. The incidences of the faces of the terminal pyramids, measured with Wollaston's goniometer, are $132^{\circ} 20'$ of the two faces, the intersection of which is parallel to one of the edges of the base of the primary form, and $147^{\circ} 18'$ of the two faces above, whose intersection is inclined to this base. One of these angles is a necessary consequence of the other. By calculating from the first, the second is $147^{\circ} 28'$, instead of $147^{\circ} 18'$, as determined by experiment. The primary form of the beau-

montite may therefore be taken as a right prism with square bases, in which the relation between the sides of the base and the height is nearly as the numbers 23 and 10, and then the faces of the pyramid have b^1 as a crystallographical sign. The crystals cleave readily parallel to the lateral faces of the primary form, but more easily parallel to one of the faces than the other, and this greater facility corresponds with the pearly lustre peculiar to them. There are also some indications of cleavage, parallel to the diagonal planes of the primary form, the crystallographical sign of which is g^1 . The colour of the crystals is whitish yellow; they are translucent; their hardness is greater than that of haydenite, and is almost equal to that of phosphate of lime.

The crystals of beaumontite and haydenite form a crystalline layer, the brilliant portions of which belong to the first-mentioned substance, and the parts covered with brownish hydrate of iron to the second. This layer covers a granular rock composed principally of grains of quartz and haydenite. The other face of the specimen is covered with small flat elongated prisms of green amphibole.—*Ibid.*

AN ACCOUNT OF THE EXPERIMENT PROPOSED BY M. ARAGO AS A TEST OF THE ACCURACY OF THE UNDULATORY HYPOTHESIS OF LIGHT.*

M. Arago proposes to avail himself of Prof. Wheatstone's revolving mirror, used by that gentleman in his researches on the velocity of electricity whilst traversing good conductors, in determining experimentally the accuracy of one or other of the present hypotheses of light. The principle of the proposed experiment is readily understood. A ray of light incident on the surface of a plane mirror is reflected in the ordinary manner, the reflected and incident beams forming equal angles with a line perpendicular to the point of incidence of the ray. If the mirror be supposed to revolve around this point to an extent expressed by the quantity a , and to augment by this quantity the original angle of incidence, the former angle of reflexion will become lessened to an extent corresponding to $2a$, which must be added to this new angle to render it equal to the first. Consequently, if the incident ray remain the same, an angular movement of the mirror of a will produce an angular motion of the reflected ray equal to $2a$.

If then two perfectly parallel rays be incident in the same vertical line on a mirror revolving round the point of incidence, their parallelism will be preserved after reflexion, providing they both impinge upon the mirror at the same instant of time, and two luminous points situated exactly vertically over each other will be seen; but if the rays impinge upon the mirror at different instants, so that one will be somewhat later than the other, the reflected images will no longer preserve their original position in the same vertical line—one appearing to the right or left of the other.

* The Editors are obliged to Dr. Golding Bird for this account.

According to the Newtonian hypothesis, or corpuscular theory of light, a ray moves in a fluid of higher refractive power, as water, quicker than in air, in a ratio expressed by the *direct relation* of the sine of the angle of incidence to the sine of refraction; whilst on the undulatory theory, light traverses a liquid much slower than air, and in the *inverse ratio* of these sines. To test the truth of either of these hypotheses, all that is therefore necessary, is to cause two rays of light to be incident on a rapidly-revolving mirror, in the same vertical line, the lowest beam traversing air only, whilst the upper one passes through a tube filled with water, or other refracting liquid. Under these circumstances, if the corpuscular theory of light be correct, the upper ray will reach the mirror before the lower one, and consequently the reflected images will no longer be in the same vertical line: if the undulatory hypothesis be the true one, then the lower ray will reach the mirror before the upper one, and the vertical position of the reflected images will become deranged. Let us suppose that the mirror revolves in a direction from right to left; then, if the upper reflected image appear to the left of the lower, *light consists of moving corpuscles!* but if the upper image appear to the right of the lower—*light is produced by ethereal undulations!* To submit this proposed experiment to the test of experimental investigation is obviously a difficult matter, for, as tubes of water of any moderate length can but very slightly affect the velocity of a ray of light, it is obvious that the rotation of the revolving mirror must be excessively rapid to produce a deviation of the reflected images sufficient to admit of accurate observation.—M. Arago is inclined to consider that a deviation of a minute of a degree produced by two positions of the reflecting plane inclined half a minute of a degree upon each other will be sufficient for this purpose.

From computations deduced from the known velocity of light, it appears that in $\frac{1}{432,000,000}$ th part of a second (the time during which a revolving mirror moving by mechanism prepared by M. Gambey moves through half a minute of a degree) a ray of light traverses a portion of space corresponding to 7.07 metres, or, in round numbers, 23 English feet. Hence if the mirror perform 2000 rotations in a second of time, the tube of water through which one of the rays passes must be 23 feet in length to produce, on the theory of emission, an angular separation of the reflected images corresponding to one minute. As a velocity of the revolving mirror of the enormous quantity of 2000 turns in a second is extremely difficult to obtain, the angular deviation produced by reflection from one revolving mirror performing 1000 rotations in a second, may be increased by viewing the images afterwards in a second revolving mirror. In this manner, by repeated reflexions from several revolving mirrors, the angular deviation of the images which eventually reach the eye of the observer may be increased to a sensible quantity.

If, as M. Arago has thought probable, an angular separation of the reflected images equal to half a minute is perceptible to the eye, a tube only $11\frac{1}{2}$ feet long, full of water, will be sufficient to produce such an alteration in the velocity of the transmitted ray, as to render the angular separation of the images very obvious, after reflection from

four revolving mirrors, each performing 1000 rotations in a second. By employing a medium of greater refractive power than water, as the essential oils or sulphuret of carbon, a still greater alteration in the velocity of the transmitted ray will be observed, being accelerated on the corpuscular, and retarded on the undulatory hypothesis.

Another mode of testing the truth of the two theories, proposed by M. Arago, is founded upon the different velocities of the coloured rays of the spectrum, whilst traversing refracting media. If, therefore, a beam of white light be made to traverse a long tube filled with a highly dispersive fluid, as sulphuret of carbon, the different coloured rays will not reach the revolving mirror simultaneously, and a spectrum will be produced. So far, the same result occurs on both theories; but on watching the reflected images, a different arrangement of the colours will be observed, and the violet or red band will be observed, in the reflected image, to be situated to the right or to the left, according to the direction of the rotation of the mirror. The order in which the colours appear, the direction of the rotation remaining the same, will be precisely the reverse on the undulatory, to that on the corpuscular theory.

It is of course obvious that great practical difficulties lie in the way of submitting M. Arago's ingenious views to the test of experiment, and hitherto no attempt appears to have been made to overcome the difficulties connected with the construction of the apparatus required for these interesting experiments.

METEOROLOGICAL OBSERVATIONS FOR DEC., 1839.

Chiswick.—Dec. 1—3. Dense fog. 4. Frosty: fine. 5. Slight haze: fine. 6. Foggy. 7. Dense fog: fine: frosty at night. 8—10. Hazy. 11. Fine: hazy: rain. 12. Cloudy and windy. 13. Overcast: heavy rain at night. 14. Fine. 15. Frosty: hazy: rain. 16. Hazy: fine. 17. Fine. 18. Hazy. 19. Cloudy. rain. 20. Rain. 21. Cloudy: rain. 22. Rain: fine. 23. Fine: very mild for the period of the season: rain at night. 24. Boisterous with rain. 25. Very fine. 26. Heavy rain. 27. Rain: hazy. 28. Frosty: fine. 29. Clear and frosty. 30. Frosty and foggy. 31. Overcast: fine.

Boston.—Dec. 1. Fine. 2. Foggy: rain P.M. 3. Cloudy: rain A.M. 4. Foggy. 5. Cloudy: rain P.M. 6, 7. Foggy. 8, 9, 10. Cloudy. 11. Cloudy: rain early A.M.: rain P.M. 12. Fine: rain P.M. 13. Fine. 14. Cloudy. 15. Fine. 16. Foggy. 17. Fine. 18. Stormy: rain P.M. 19, 20. Cloudy: rain early A.M.: rain P.M. 21. Fine: rain P.M. 22. Cloudy: rain P.M. 23. Fine: rain P.M. 24. Cloudy. 25. Fine. 26. Fine: rain P.M. 27. Foggy. 28, 29. Fine. 30. Fine: hail and rain P.M. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Dec. 1. Clear and sunny. 2. Calm and clear: hard frost. 3. Dull: raw day. 4. Dull, but dry. 5. Frost A.M.: thaw P.M. 6. Dull moist day: hoar frost early A.M. 7. Dull and cloudy, but dry: hoar frost A.M. 8. Fine day: slight hoar frost. 9. Quiet and cloudy. 10. Quiet and cloudy: freezing P.M. 11. Fine day: wind rose P.M. 12. Heavy rain in the afternoon. 13. Fine morning: rain afternoon. 14. Moderate day: slight frost preceding night. 15. Foggy: slight frost preceding night. 16. Fine: slight frost early A.M. 17. Raw and cold. 25. Stormy day of wind and rain. 26. Clear day: frosty morning. 27. Snow in the evening, and frost all day. 28. A little more snow: hard frost P.M. 29. Clear and calm and frosty. 30. Looking dull A.M.: sleet and rain P.M. 31. Heavy rain all morning: cleared up P.M.

Sun 20 days. Rain 9 days. Snow 3 days. Frost and hoar frost 13 days.

Wind easterly 13 days. North-east 4 days. Southerly 4 days. Westerly 2 days.

Days of Month. 1839. Dec.	Barometer.			Thermometer.				Wind.				Rain.			Dew point. London: Roy. Soc. 9 a.m.		
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.		Chiswick.	Boston.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.		Dumfries-shire.	
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max. Min.									Max. Min.
1.	29.708	29.810	29.724	29.30	29.51	29.63	37.7	37.6	38.4	37.6	41	29	E.	SE.	calm	ENE.	38
2.	29.882	29.992	29.890	29.54	29.78	29.90	38.8	37.2	39.4	37.2	42	33	E.	NW.	calm	E by N.	36
3.	29.960	29.985	29.962	29.61	29.92	29.89	38.8	35.8	39.4	35.8	39	27	S.	SW.	calm	NE.	38
4.	29.830	29.963	29.835	29.48	29.78	29.82	34.3	32.0	34.6	32.0	38	30	NNW.	N.	calm	E.	34
5.	30.096	30.487	30.124	29.67	29.92	30.06	36.7	33.3	37.2	33.3	37	28	SW.	NW.	calm	NE.	33
6.	30.364	30.508	30.397	29.94	30.19	30.15	36.7	36.0	37.3	36.0	37	32	W.	S.	calm	NE.	35
7.	30.332	30.366	30.239	29.95	30.10	30.05	34.9	34.2	35.3	34.2	42	28	E.	SE.	calm	E.	33
8.	30.096	30.124	30.014	29.83	30.00	29.97	35.4	33.4	35.9	33.4	35	31	E.	E.	calm	E.	32
9.	29.830	29.860	29.712	29.60	29.81	29.65	34.9	34.7	36.0	34.7	37	32	NW.	NE.	calm	ENE.	31
10.	29.606	29.651	29.637	29.35	29.50	29.45	34.2	34.2	36.7	34.2	42	34	E.	E.	SW.	E.	31
11.	29.664	29.598	29.326	29.24	29.45	29.33	42.4	34.3	43.0	34.3	46	39	E.	SE.	calm	ENE.	37
12.	29.248	29.286	29.190	29.2	29.18	29.10	44.8	41.9	45.3	41.9	48	35	E.	S.	E.	E.	39
13.	29.294	29.311	29.277	28.87	29.10	29.09	43.5	41.3	46.4	41.3	48	42	S.	S.	calm	E.	40
14.	29.230	29.400	29.247	28.79	29.07	29.30	44.2	42.3	44.4	42.3	49	30	SW.	SW.	calm	S by W.	39
15.	29.590	29.596	29.427	29.15	29.40	29.42	39.3	38.8	40.0	38.8	43	41	SW.	SE.	calm	S.	36
16.	29.696	30.083	29.713	29.20	29.63	29.89	40.3	38.8	40.5	38.8	44	30	W.	SE.	calm	SSW.	38
17.	30.074	30.094	29.868	29.64	29.89	29.63	38.3	37.4	38.8	37.4	44	31	NE.	SE.	calm	E by S.	37
18.	29.516	29.530	29.376	29.17	—	—	35.2	35.5	35.5	34.7	42	41	E.	SE.	E.	—	32
19.	29.418	29.426	29.356	28.98	—	—	47.7	48.4	47.7	48.4	55	50	S.	S.	calm	—	40
20.	29.282	29.480	29.262	28.82	—	—	51.3	47.3	51.7	47.3	55	46	S.	S.	calm	—	46
21.	29.552	29.566	29.561	28.96	—	—	47.3	46.6	47.7	46.6	53	46	SW.	SW.	calm	—	45
22.	29.550	29.547	29.376	28.95	29.99	—	49.9	46.8	50.6	46.8	55	47	SW.	SW.	calm	—	49
23.	29.586	29.586	29.333	28.95	—	—	48.6	48.4	49.3	48.4	57	44	S.	SW.	W.	—	48
24.	29.244	29.416	29.302	28.64	28.73	—	53.4	45.6	54.6	45.6	54	44	S.	W.	W.	W.	48
25.	29.528	29.568	29.530	28.39	29.02	29.43	47.7	45.9	49.4	45.9	48	28	S.	W.	W.	E.	42
26.	29.732	29.728	29.322	29.28	29.49	29.45	40.7	41.2	41.2	38.3	54	38	S.	E.	calm	E.	42
27.	29.342	29.577	29.386	28.90	29.30	29.42	47.2	39.4	47.8	39.4	46	29	W.	NW.	calm	NE.	45
28.	29.752	30.062	29.759	29.30	29.52	29.79	35.7	35.8	36.0	35.8	39	24	NW.	W.	calm	WNW.	40
29.	30.280	30.440	30.320	29.81	30.08	30.26	33.3	32.2	33.5	32.2	40	21	W.	W.	calm	NW.	33
30.	30.398	30.436	30.163	30.08	30.14	29.80	32.3	32.0	35.2	32.0	44	34	SW.	SE.	calm	ESE.	33
31.	29.924	29.975	29.727	29.45	29.41	29.33	44.8	32.0	45.3	32.0	52	45	S.	S.	S.	SSW.	38
Mean.	29.729	29.821	29.656	29.28	29.58	29.65	41.0	38.4	41.8	38.4	45.35	31.96	Sum.	2.32	1.77	3.67	Mean.
													2.326				38.6

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

MARCH 1840.

XXX. *On the true Order of Succession of the Older Stratified Rocks in the Neighbourhood of Killarney and to the North of Dublin.* By RICHARD GRIFFITH, Esq., F.G.S. L., and President of the Geological Society of Dublin.

[See Section Plate II. and Plan Pl. III.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE Geological map of Ireland, which presents an epitome of my geological labours in the field, continued at intervals for upwards of thirty years, is now before the public, and has in fact become their property. Though I do not venture to assert that all the lines representing rock boundaries which it contains are perfectly correct, still I will say that none have been laid down without consideration. Much detail no doubt still requires to be worked out within the great divisions, particularly in the carboniferous limestone series, and many of the smaller greenstone protrusions have yet to be pointed out; but I am fully of opinion that the great lines nearly represent the true boundaries of the several rock formations, according to the order of superposition indicated by the table of geological colours. Having this impression on my mind, I confess I was somewhat startled by observing in the Number of the Philosophical Magazine for December last, (vol. xv. p. 442.) a paper communicated by my friend Mr. Charles William Hamilton of Dublin, which contains statements, some of which are illustrated by sections, relative to the geological positions of the strata of several parts of Ireland, quite at variance with those assigned by me to the same rocks in the geological map. This paper also asserts that I have indicated the occurrence of rocks in certain places in which no such rocks are to be found. These are charges

Phil. Mag. S. 3. Vol. 16. No. 102. March 1840. M

which I think it incumbent on me shortly to reply to; and I hope to be able to show that my map is correct, and that Mr. Hamilton is incorrect in every case in which he has thrown a doubt on its accuracy.

The main points of difference between Mr. Hamilton and myself are; 1st, he is opinion, that in the county of Kerry, south of Castlemaine Bay and the lower Lake of Killarney, the old red sandstone overlies *unconformably* those schistose rocks which in my map are comprehended under the general name of transition, and which include the Silurian system and the older or Cambrian slate*.

2nd, That the old red sandstone strata of the Gap of Dunloe extend uninterruptedly in a southern direction from the gap to the summit of MacGillacuddy's Reeks, from which point they dip to the south, and are *succeeded* conformably by a new series of rocks which Mr. Hamilton considers to belong to the Devonian system.

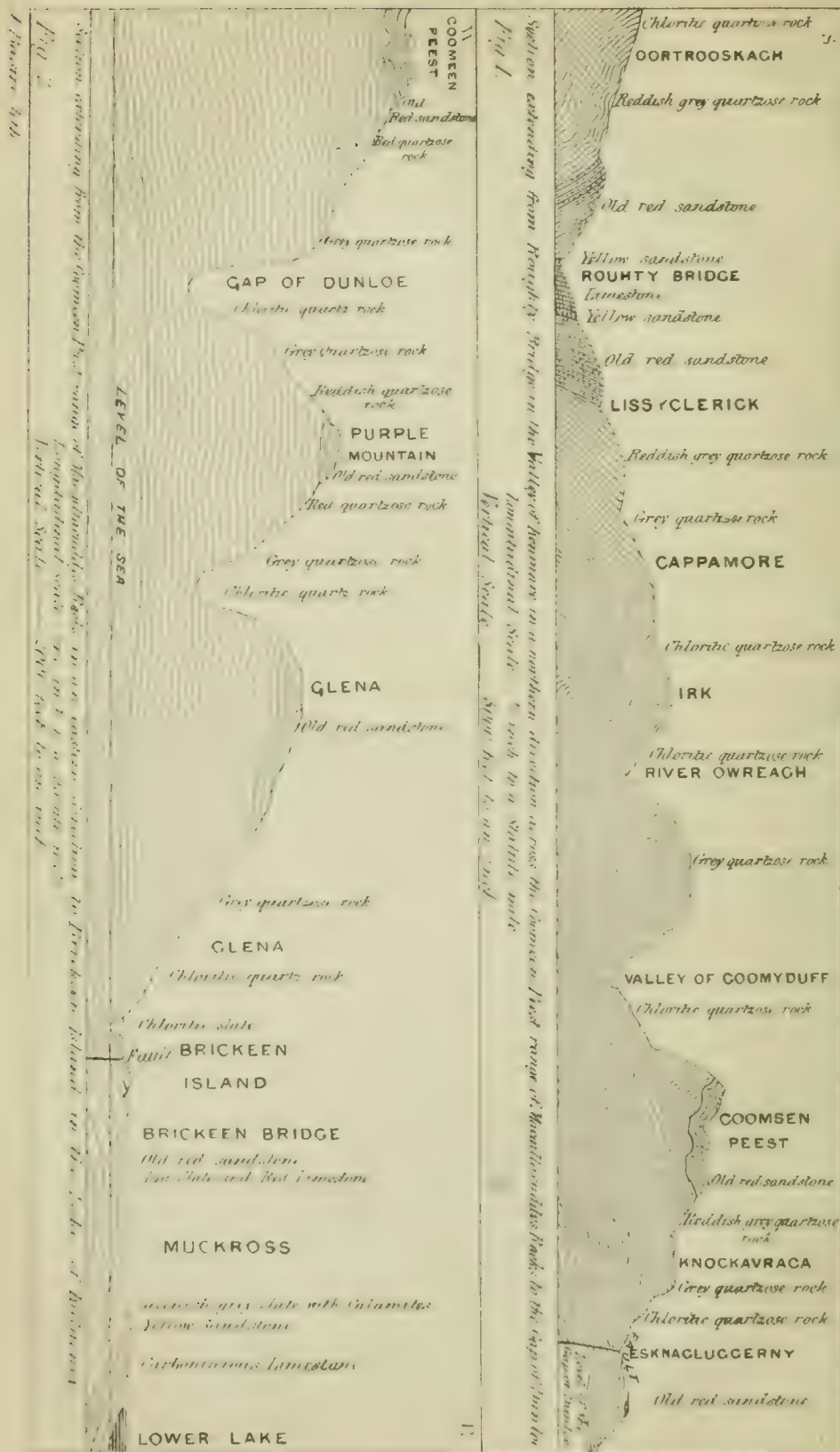
3rd, That the band of yellow sandstone shown on the geological map as underlying the carboniferous limestone in the valley of the River Roughty at Kenmare, does not exist there.

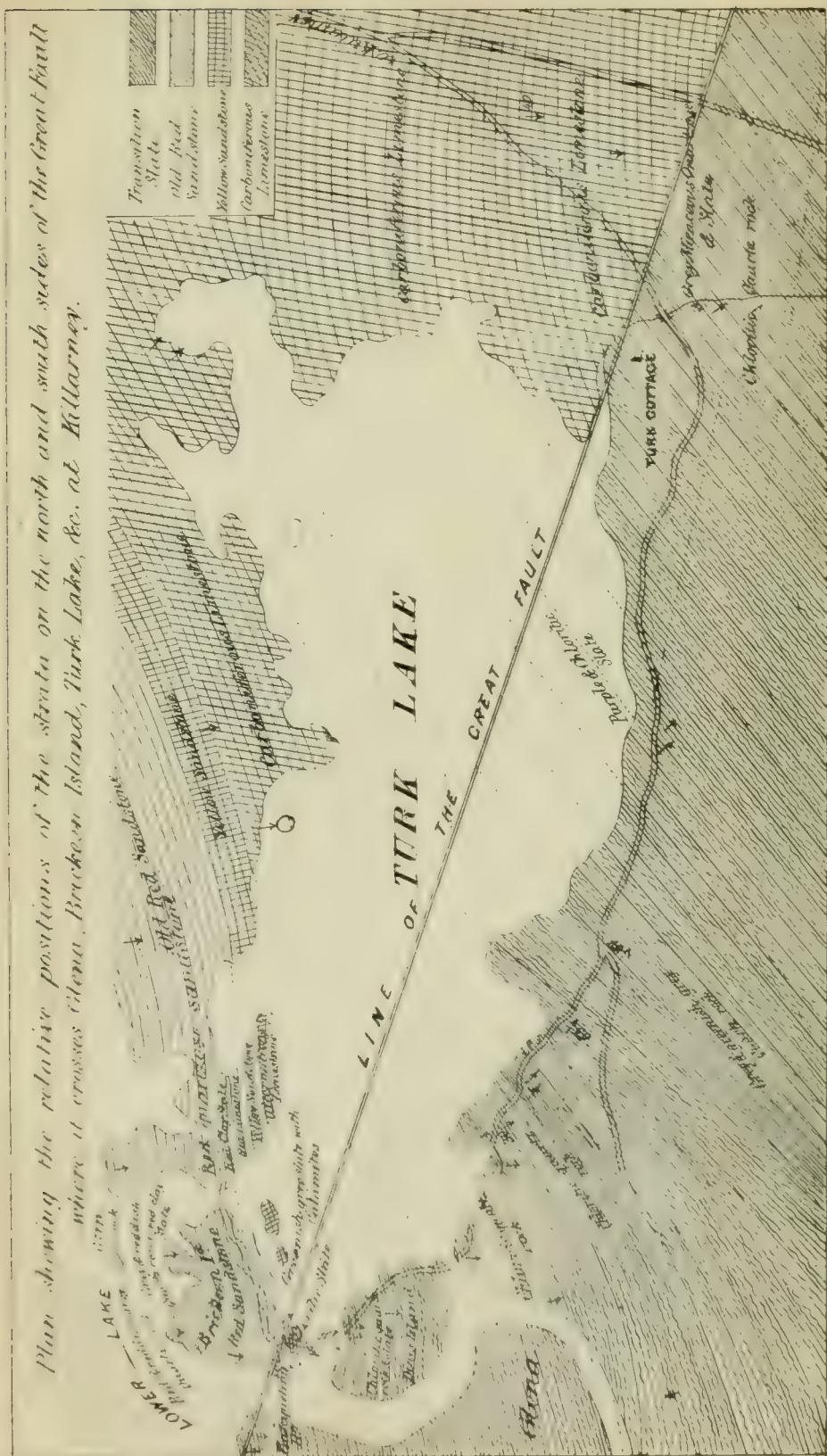
In illustration of these views Mr. Hamilton has given two sections, the first of which extends from the gap of Dunloe in a south-eastern direction across Toomies and Glenna Mountain to the middle or Turk Lake of Killarney, and thence over Turk and Mangerton Mountains to the valley of Kenmare; the second is a representation of Mr. Hamilton's view of the strata as they appear on the west side of the gap of Dunloe.

In both of these sections Mr. Hamilton has represented the old red sandstone as resting *unconformably* on the older schistose rocks, which he calls "Cambrian;" but I state without fear of contradiction that his section and statements are incorrect in this respect, and that in the locality in question the old red sandstone has been deposited *conformably* on the older slate, and in a descending order graduates imperceptibly into that rock. This is also the opinion of Mr. Weaver, who considers the whole to belong to the transition series; and Capt. Portlock, in his presidential address to the Geological Society of Dublin, appears to entertain the same opinion.

In the first volume of the Journal of the Geological Society of Dublin, page 285, Mr. Hamilton describes the old red sandstone as forming "an anticlinal axis on the summit of

* See note appended to the large Geological Map of Ireland.





Currawntoohill mountain*, from whence dipping to the southward it is covered by siliceous sandstones of a greenish or brownish colour;" and this view is illustrated by a section given in Plate I. fig. 3, which, together with those contained in your Magazine, exhibit the whole of his views on the subject.

For the sake of clearness, I have also prepared two sections passing nearly through the same line of country as those given by Mr. Hamilton, from which it appears that the old red sandstone on the summit of MacGillacuddy's Reeks, rests *conformably* on the schistose rocks of the gap of Dunloe, which Mr. Hamilton terms Cambrian; and that the apparent unconformability which is visible nearly in the centre of the gap between the chloritic quartz rock and the old red sandstone, has been occasioned by a great north-west and south-east fault, which crosses the gap of Dunloe nearly at right angles, and extends from thence in a south-eastern direction along the northern declivities of the Purple, Toomies, and Glena Mountains, from whence in continuation it reaches the lower Lake of Killarney, near the Glena Cottage Banqueting House, thence it crosses Brickeen Island, and passing through Turk Lake reaches the north base of Turk mountain.

This fault may be said to form the key to the geology of the Killarney district, as it explains the apparent anomaly deducible from the persistent dip to the south of the strata on both sides of Glena Cottage and Brickeen Island, which might lead, and has led incautious observers to infer that the strata belonging to the transition slate series which occur to the south of the fault rest conformably on the top of the old red sandstone of Brickeen Bridge, of the northern part of Brickeen Island, and likewise on the carboniferous limestone east of Turk Cottage.

In expressing my view of the geology of the district, I shall commence with the west side of the Gap of Dunloe; but here my section, Plate II. fig. 1, is so different from Mr. Hamilton's, that it is with difficulty we can recognise them as being intended to represent the same locality; yet such is the case. Of the accuracy of my own section I entertain no doubt, having made it with great care.

In taking a sectional view of the strata as exhibited on the west side of the Gap of Dunloe, the first beds visible at the surface consist of a reddish gray quartzose rock belonging to the old red sandstone formation. These are succeeded by a series of thick beds, of coarse-grained conglomerate composed of rounded pebbles of white quartz, varying in size

* The highest of the Reeks.

from two inches to a quarter of an inch in diameter, imbedded in a reddish gray arenaceous base. The conglomerate is succeeded by coarse-grained brownish-red slate, which is occasionally quarried and used for inferior roofing slate; these strata are followed by a series of beds, consisting of red quartzose sandstone alternating with coarse slate, the sandstone beds presenting occasionally a conglomeritic character, but the pebbles rarely exceed half an inch in diameter. The strata dip to the west, though irregularly, at an average angle of about 10° from the horizon; and consequently in ascending the glen in a southern direction, the cliffs present the outgoing or strike of the beds, which are not horizontal, but exhibit a tortuous arrangement presenting frequent undulations from north to south. At Esknagluggerny, a short distance beyond the southern extremity of Coosane lake, the old red sandstone strata are cut off by the great north-west and south-east fault already mentioned, immediately to the south of which thick beds of green chloritic quartz rock appear at the surface, dipping to the south at an angle of 30° from the horizon. These strata probably form the *lowest* portion of the transition rocks situated to the south of Castlemaine Bay, as we perceive the whole series to be complete in an ascending order from them to the summit of the Reeks.

Commencing then with this chloritic quartz rock base, and proceeding in a southern direction, we find that the same rocks continue to dip to the south, and present an accumulation of strata for upwards of 500 feet in thickness, varying little in their composition or character, with the exception of an occasional interstratification of thin beds of green and purplish gray clay slate. These slates are quite distinct in character, as well as in colour and composition, from the coarse red slate of the old red sandstone; they are in fact identical with the Valentia slates, and bear a strong resemblance in colour, composition, and lithological character to some of those of North Wales. Still ascending in the series, we find that the colour of the rocks gradually changes from green to gray, and at length the mineral chlorite is altogether wanting. These gray quartzose beds are not so thick as those which contain chlorite; they likewise alternate with thin beds of clay slate, which present a purplish gray colour, without any admixture of green. The gray strata may amount altogether to about 800 feet in thickness. Still continuing to ascend, the same character and alternations are preserved as the last described, but on a fresh fracture the quartzose beds present a slight bloom or tint of red, and the disintegrated surface of the rock exhibits a decidedly reddish hue, which

is not visible lower down: the reddish gray strata alternate as before with purplish gray slate. As we ascend and approach the summits of the Coumeen Peest, or eastern ridge of the Reeks, the strata assume a more decidedly red character, till at length they pass into brick or cherry-red quartz rock, and contain some beds of conglomerate, identical in colour, composition, and structure with the old red sandstone already described, situated to the north of the fault in the Gap of Dunloe, but not quite so coarse-grained*. These red quartzose or old red sandstone beds differ materially from the schistose beds of the lower part of the series. The structure of the rock is decidedly granular, the strata are thinner, and they are divided by joints into rectangular masses, while the schistose beds beneath usually present rhomboidal forms.

The conglomerate on the top of the Reeks is perfectly conformable with the underlying strata, and in fact a regular gradation may be traced from the lower or chloritic portion of the series through the gray and reddish gray into the brick-red quartz rock and conglomerate.

From the summit of the Reeks still proceeding in a southern direction towards the valley of Kenmare, we do not find these old red sandstone strata dipping to the southward, as shown on Mr. Hamilton's section already mentioned, and published in the *Journal of the Geological Society of Dublin*, but on the contrary they crop out to the southward, forming a regular cap resting conformably on the inferior strata, whose ends appear in the precipitous escarpment visible on the northern side of the valley of Coomyduff, in the bottom of which the green chloritic beds already described as occurring near the fault in the Gap of Dunloe again make their appearance, and the whole succession of the strata and passage from the green chloritic beds to the red conglomerate as already described on the northern acclivity of the Reeks, may likewise be traced on the southern.

It is unnecessary to enter into any further particulars respecting the detail of the succession of the strata between the valley of Coomyduff, and the reappearance of the old red sandstone at Lisinisky, to the north of the valley of Kenmare, as it is clearly shown in the section, and the same gradation of colour and character, from the green to the red rocks, is observable. See Plate II. fig. 1.

The old red sandstone of the valley of Kenmare consists of

* The conglomerate visible near the summit of Lisbug mountain on the western ranges of the Reeks, is fully as coarse-grained as that of the Gap of Dunloe.

red quartz rock and red clay slate, the quartz rock predominating; but no beds of true conglomerate have been observed, though such may be discovered on a more careful examination.

Ascending in the series, and approaching the limestone of the valley of Kenmare, the red strata become more schistose and chiefly consist of coarse red clay slate, which, approaching the limestone, is observed to alternate with yellowish green clay slate and red limestone in thin beds. These are succeeded by strata of gray quartzose sandstone containing calamites, the characteristic fossil of the yellow sandstone series, the upper beds of which alternate with greenish gray and dark gray clay slate, with occasional beds of gray limestone. Still ascending, the limestone gradually predominates, till at length the slate disappears, and the whole stratification is composed of carboniferous limestone.

To the south of the river Roughty, in a descending order, a similar series to that above described appears at the surface dipping to the north, so that we have again the dark gray slate and limestone, the yellow sandstone with calamites, the red limestone, the old red sandstone consisting of red slate and red quartz rock, and in continuation the whole suite of the schistose strata of the transition series already described.

Having described this section in detail, I again assert, that the old red sandstone to the south of Castlemaine Bay has been deposited conformably, on the underlying strata, and that the apparent unconformability noticed by Mr. Hamilton as occurring in the Gap of Dunloe, arises from a dislocation of the strata occasioned by a fault, and not from *original deposition*.

Owing to the want of fossils throughout the entire succession of rocks above described, with the exception of the yellow sandstone and carboniferous limestone of the valley of Kenmare, it is difficult to determine the position in geological precedence which should be allotted to the green chloritic quartz rock which forms the basis of the district under consideration, or to determine at what precise point the old red sandstone should be said to commence. It appears to me that the only key which is likely to unravel this mystery, will be found in the peninsula of Dingle, to the north of Castlemaine Bay, where fossils have been discovered in the strata, which have been recognised by Mr. James Sowerby as belonging to the upper Silurian rocks of Murchison.

The fossils consist of

From *Ferriter's Cove*.

<i>Atrypa tenuistriata</i> , Sil. Syst. pl. 12. f. 3.	<i>Euomphalus perturbatus</i> , Sil. Syst. pl. 22. f. 15.
<i>Avicula reticulata</i> , <i>Ib.</i> pl. 6. f. 3.	
——— <i>retroflexa</i> , <i>Ib.</i> pl. 5. f. 9.	<i>Leptaena lata</i> , <i>Ib.</i> pl. 5. f. 3.
<i>Cornulites serpularius</i> , <i>Ib.</i> pl. 26. f. 5-9.	<i>Terebratula bidentata</i> , <i>Ib.</i> pl. 12. f. 13.
<i>Euomphalus funatus</i> , <i>Ib.</i> pl. 12. f. 20.	——— <i>Stricklandii</i> , <i>Ib.</i> pl. 13. f. 19.

If we make a section across the Dingle peninsula from Feilaturrive on the south to Brandon Bay on the north, we find that the strata consist of a base of dark blackish gray clay slate, the upper beds of which alternate with reddish purple slate, some of which contain Silurian fossils; these strata are succeeded by red slaty conglomerates, alternating with red and green slate and brown quartz rock, above which are chloritic quartz rocks with alternating purplish and reddish gray clay slate, similar in composition and character to those of the Gap of Dunloe, and of that district generally. Again, if we make a section along the west coast of the Dingle peninsula from Fawn to Sybil Head, we find numerous fossiliferous beds, some of which have been noticed by Mr. Hamilton*, the true position of which has not yet been clearly made out; but from all the data I possess, I am inclined to place them between the dark gray clay slate and the chloritic quartzose rock. Hence I am of opinion that the chloritic rocks of the Gap of Dunloe should be classed with the Silurian system.

With regard to the dark blackish gray clay slate which forms the lowest member of the series in the Dingle peninsula, it is identical in lithological character with the dark gray slate of the Gatties mountains, which are situated directly in the line of the strike in an eastern direction, as may be clearly seen by reference to my Geological map; and following the same strike similar strata occur in the Shivnamanna mountains of the county of Kilkenny, and also in the slate district to the south of Waterford, where on the sea coast at Knockmahon, and also at Tramore, fossils occur in green chloritic metamorphic slate and quartz rock, belonging to the lower Silurian strata or Caradoc sandstone†. These fossiliferous beds are incumbent on the dark gray slate, in which no fossils have as yet been observed. This dark gray slate bears a striking resemblance in lithological character to the dark gray clay slate of the Festiniog district of Merioneth-

* See Journal of the Geological Society of Dublin, vol. i. p. 280.

† The fossils discovered are *Orthis radiatus*, and several others of that genus; so *Bellerophon bilobatus*, *Atrypa orbicularis*?, &c.

shire; and I am led to conclude, that the whole of the transition slate of the south of Ireland is high in the series generally, and that the principal part belongs to the Silurian system.

Our next point for consideration with regard to the district of Kerry, south of Castlemaine Bay, will be to determine where, in the graduating series between the chloritic quartz rocks and the decided red conglomerate, we should draw the line separating the Silurian system from the old red sandstone. If we again refer to Dingle peninsula, we find that the summit of Cahirconree mountain, together with many other mountains of the district, is composed of rocks belonging to the old red sandstone, which rest *unconformably* on the dark gray clay slate above-mentioned, and on the other schistose rocks which succeed it. From this unconformability, it would appear that the series of the schistose strata is not complete in the peninsula of Dingle, and that it wants the upper members which do occur to the south of Castlemaine Bay. If we examine the old red sandstone series as exhibited in the Dingle district, we find that the under beds consist of rather fine-grained, red quartzose sandstone, for a thickness varying from 100 to 150 feet in different localities; the sandstone is succeeded by thick beds of coarse conglomerate alternating with coarse red slate, and these in continuation by alternations of red and brown quartzose sandstone and coarse red slate.

Now if we compare the red strata which rest conformably on the schistose rocks on the summits of the Reeks, the Purple mountains, &c. we find red granular quartz rock in thin beds underlying the conglomerates; and considering these beds, as in the Dingle peninsula, to represent the lower portion of the old red series, I have drawn the line where the red quartz rock terminates, and where a change is indicated by an alteration in the colour of the rock, which becomes light reddish gray, and the strata present a schistose, instead of a perfectly granular structure.

As a further proof of my opinion of the inaccuracy of Mr. Hamilton's views respecting the order of succession of the strata of the Killarney district, I shall now very shortly describe the succession of rocks visible at the surface on the north and south sides of the great fault where it traverses the strata at Glena and Brickeen Island on the lower lake of Killarney, which fault, as already mentioned, extends in a south-eastern direction from the Gap of Dunloe, by the localities in question, to the northern base of Turk mountain, &c. If we trace the line of the fault on the shore of the lake immediately to the north of Glena Banqueting House, and also

where it crosses Brickeen Island, it will be seen that the strike of the chloritic quartz and slate beds on the south side of the fault is unconformable with the strike of the old red sandstone and carboniferous limestone on the north of it, and that the ends of the strata of both formations abut obliquely against the opposite sides of the fault. This fact will be at once understood by reference to the plan given in Plate III.

The lower portion of the strata on the north side of the fault consists of old red sandstone: commencing near the shore of the lower lake at Cullinagh to the east of O'Sullivan's Cascade, we have a succession of beds of coarse-grained red conglomerate, similar to those already described as occurring at the northern entrance of the Gap of Dunloe; these strata, which dip to the east, are succeeded by alternating red and light gray quartzose beds, which in an ascending order continue to Glena Bay, the dip gradually changing from the east towards the south. At Glena Bay we fall into the line of the strike of the strata of the Gun rock, a small island situated immediately to the west of Brickeen Island, near Brickeen Bridge. At the Gun rock the strata consist of light gray quartzose rock alternating with thin beds of reddish gray limestone, which dip to the south at an angle of 40° . On the north point of Brickeen Island, these strata are succeeded by alternations of red and reddish gray quartzose rock, red slate, and red limestone, the general dip being 25° E. of S. at an angle of 35° . These strata abut obliquely against the fault to the north of the Banqueting House of Glena, and also where it crosses the western portion of Brickeen Island.

Still ascending in the series, the above-mentioned strata are succeeded by alternations of red clay slate, yellowish green clay slate, and red limestone, which may be considered to form the upper portion of the old red sandstone series: above them we find a succession of beds, consisting of coarse greenish gray slate containing calamites, greenish gray calcareous slate, and impure gray limestone, succeeded by beds of gray quartzose sandstone with partings of black clay slate, and alternations of gray limestone. These strata form the lowest portion of the carboniferous limestone series, to which I have given the name of the yellow sandstone*. Above the yellow sandstone we have the black carboniferous slate, which here alternates with gray limestone, the slate as at Kenmare predominating near the commencement; but as the

* In most localities the colour of the sandstone is yellowish gray, and the siliceous rock forms by much the most important feature in the series.

beds accumulate, the calcareous strata increase in thickness, and at length near the old copper mine of Muckross, the slate beds entirely disappear, and the entire stratification consists of limestone, some beds of which are fossiliferous, and contain *Producta depressa*, variety of the mountain limestone *Spirifera bisulcata*, *Spirifera resupinata*, and many of the other fossils which usually occur in the lower limestone.

From Muckross mine the calcareous strata continue without interruption, dipping to the south at an angle of 40° , to the base of Turk mountain, a quarter of a mile to the east of Turk cottage, and are still visible at the surface close to the line of the eastern continuation of the fault which is there concealed from view by diluvial matter.

I have been thus particular in describing the strata visible on the north side of the fault at Brickeen Island and Muckross, as they present one of the most perfect sections which Ireland affords of the entire suite of the old red sandstone, the yellow sandstone, the carboniferous slate, and the lower carboniferous limestone, and which by their variety form a strong contrast with the uniform character of the chloritic quartzose strata visible on the south side of the fault, which strata evidently belong to the same series as those which occur in a similar position at the Gap of Dunloe, and are in fact a continuation of the same strike. No doubt can therefore be entertained that these inferior strata form the lower portion of the schistose series of the district to the south of Castlemaine Bay, that they are identical with the chloritic rocks of the Gap of Dunloe, and do not belong to the Devonian as supposed by Mr. Hamilton *.

Proceeding to the southward from Turk mountain, the succession of rocks is similar to that already described as occurring above the chloritic beds of Dunloe: the strata undulate very much, and present several synclinal and anticlinal axes, so that the upper beds or reddish gray quartzose strata never appear on Turk or Mangerton mountains, and consequently in this line we do not meet with the old red sandstone strata till we approach the northern boundary of the valley of Kenmare, where it occurs in a line parallel to that already noticed in describing the section between the Gap of Dunloe and the valley of Kenmare. It is true, Mr. Hamilton mentions the occurrence of old red sandstone in the centre of Mangerton mountain, but in that locality he may have mistaken the pur-

* A section similar to that from the Gap of Dunloe to the summit of the Reeks may likewise be traced from the chloritic rocks, south of the fault at Brickeen Island, up to the old red sandstone on the summit of Glenna mountain, as represented in Plate III. fig. 2.

ple slate which there alternates with the chloritic quartz for that rock.

It appears to me to be extraordinary, that when arriving at the conclusion that the chloritic rocks of Turk and Manger-ton mountains were newer than the old red sandstone, owing to the observed dip to the south both of the old red sandstone and the chloritic rocks, Mr. Hamilton did not (with Mr. Weaver) consider these chloritic rocks to be superior to the carboniferous limestone of Muckross and Turk. The limestone beds all dip to the south towards the fault, and apparently underlie the chloritic rocks; and as the limestone strata rest upon the yellow and red sandstone, the natural conclusion should have been, that the whole of the strata to the south of the lakes of Killarney belonged to the *millstone grit*, which we find resting on the same limestone to the north of the lakes. Mr. Weaver carefully observed all these dips; and not having noticed the fault or unconformity of the strike of the old red and limestone series on its north side with the chloritic rocks on the south, and conceiving that the limestone together with the old red sandstone belonged to the transition series, he naturally concluded that the chloritic rock to the south belonged to the same; but Mr. Hamilton has overlooked the southern dip of the limestone at the northern base of Turk mountain east of Turk cottage, and placed the chloritic rocks in a position in which they could not occur according to the dips of the strata, namely, between the old red sandstone and the carboniferous limestone series.

I shall not pursue this subject, as I should hope that sufficient data have been brought forward to prove that the strata to the south of the Lake of Killarney, which Mr. Hamilton considers to be Devonian, do really belong to the Silurian system.

I shall next allude to a paragraph in Mr. Hamilton's paper, page 445, in which he states, "that among other localities in which the yellow sandstone is laid down on the Geological map *in positions in which it does not exist*, he may mention the boundary of the carboniferous limestone, on the banks of the River Roughty in the Valley of Kenmare."

I wish Mr. Hamilton had mentioned the other instances that came within his knowledge as well as this, as I have no doubt I should have been equally well able to show that the boundaries marked were founded on actual observation.

In the present case I shall merely observe, that at Kilgawan, on the north side of the valley of Kenmare, above Roughty Bridge, yellow sandstone occurs in considerable thickness, overlying red quartz rock, green and red clay slate, and red

limestone, and underlying black carboniferous slate, interstratified with thin beds of limestone, which is ultimately succeeded by limestone without admixture; the whole of the strata dipping to the south. Again, on the south side of the river Roughty, immediately to the east of Roughty Bridge, a similar succession of strata is observed dipping to the north. In this locality, the yellow sandstone contains that variety of calamite which is characteristic of the rock, and which is also abundant at Brickeen Island, near Killarney, and in the same geological position, underlying the limestone of the several troughs of the counties of Waterford and Cork. In the carboniferous slate of Roughty Bridge, *Retepora membranacea* was observed. I shall mention one other locality in which the strata in connexion with the yellow sandstone have been observed in the valley of the river Roughty, namely, at the pier at Kenmare. At low water in this place, gray quartz rock and black carboniferous slate may be observed dipping to the north under the lower beds of the carboniferous limestone, which are exposed to view in an adjoining quarry. Immediately to the south of this quartz rock and slate, no rocks are visible, the strata being concealed by sand; but in a very short distance, beds of yellowish green slate, alternating with red slate and red limestone, occur which are identical with the strata visible in the localities already mentioned underlying the yellow sandstone: no doubt can therefore be entertained that this rock is continuous on the south side of the valley from Kenmare pier to Roughty Bridge. It is true, in this locality, as well as in most others, that the yellow sandstone has not been seen at the surface throughout the entire length of the carboniferous limestone trough, as, owing to a thick covering of diluvial matter, or of bog, the precise boundary between the base of the limestone series and the old red sandstone rocks is rarely visible; but as the yellow sandstone and dark gray carboniferous slate do occur in every place where the outer boundary of the limestone series is exposed to view, I feel little doubt that these rocks equally occur in those positions where they are concealed from our view. If geologists were only to mark the limits of their rock districts in the precise localities in which the contacts are visible, no geological map could be formed. In maps on a large scale, the observed contacts might be shown by continuous, and the supposed by dotted lines: but on a general map, though desirable, it would be impossible to enter into such detail, or if attempted it would be impracticable, on a map on which the features of the country are shown, to distinguish between the continuous and the dotted lines.

I shall next very shortly allude to another point brought forward by Mr. Hamilton in his paper. In speaking of the district coloured old red sandstone on my large geological map which occupies extensive tracts in the counties of Waterford and Cork, he observes, "As to classification, it appears to me that Mr. Griffith has thrown together two rocks which are very distinct, namely, 1st, The old red sandstone and its conglomerates; 2ndly, Compact arenaceous rocks, agreeing in geological position with the upper part of the Devonian series, as described by Professor Sedgwick and Mr. Murchison."

It appears to me that Mr. Hamilton is mistaken in separating the old red sandstone from the Devonian system, as Professor Sedgwick and Mr. Murchison include the whole series under the general term Devonian: consequently, whether we apply the term Devonian, or old red sandstone, to the system, no advantage could be derived from its arbitrary subdivision.

I freely admit that doubts may be entertained as to whether the yellow sandstone and carboniferous slate, considered by me to belong to the lowest portion of the carboniferous system, should not be placed at the top of the old red sandstone or Devonian system. This however is not a question of position, but of fossils; and as far as my present information extends, I still feel inclined to adhere to my original view, founded on the fact, that in ascending in the old red sandstone series the first alteration observed is a change from *red and yellowish green clay slate*, to yellow quartzose sandstone passing into light gray, which contains in abundance the peculiar variety of calamite already mentioned. Below this sandstone no fossil remains have been discovered; above it they occur in abundance, and the upper beds which alternate with gray clay slate and limestone contain fossils which undoubtedly belong to the carboniferous limestone, though doubts may be entertained respecting a few which occur towards the bottom of the series.

Should it appear on a more careful and extended examination that some of the fossils of these lower beds are similar to those which occur in the upper part of the Devonian system, difficulties will still arise as to where in a graduating series the line should be drawn separating the Devonian from the carboniferous system; and as far as Ireland is concerned, I think I have adopted that which appears to be least liable to objection; but even on this point I am open to conviction.

In the mean time the fact to dwell upon is, that the mineral succession which I have pointed out beneath the carboniferous limestone of Ireland, is similar to that described in North Devon by Professor Sedgwick and Mr. Murchison.

In the conclusion of his paper Mr. Hamilton observes,

“that all those tracts which occur between Dublin and Dundalk, along the course of the Boyne, and in the hills separating the counties of Cavan and Meath, which have been described as the older graywacke or transition series by Mr. Griffith, Mr. Weaver, and others, are in reality all conformable, and immediately inferior to the mountain limestone and superior to the old red sandstone, and consequently belong to the Devonian series.”

I confess I am surprised at the view here taken by Mr. Hamilton, as one of the facts on which his argument is grounded, namely, that the rocks coloured by me as transition are superior to the old red sandstone, have been correctly stated by himself* to be inferior to that rock where it occurs near Balriggeran mill, N.W. of Dundalk, in the county of Louth, in which locality the old red sandstone rests unconformably on the transition slate.

In regard to the second point, namely, that the schistose rocks are succeeded by the limestone in a conformable position, I have to observe that such is not the fact; for in the only localities in which I have been hitherto enabled to observe the contact of the two rocks, the limestone rests unconformably on the transition slate. These localities are in the river north-east of the Naul, in the county of Dublin; at the southern extremity of the village of Duleek, in the county of Meath; at Old Bridge on the banks of the Boyne, two miles west of Drogheda, in the county of Louth; and at Headfort near Kello, in the county of Meath.

Fortunately, in addition to these facts, we have also another, which is quite conclusive, namely, the discovery of fossils belonging to the lower Silurian rocks or Caradoc sandstone, which occur in considerable abundance at Grangegeeth, four miles north of Slane, in the county of Meath: the fossils have recently been examined by Mr. Murchison and Mr. Lonsdale, of the Geological Society of London, to whom I had sent them, and both are of opinion that they belong to the lower Silurian rocks†. Consequently we must come to the conclusion that Mr. Hamilton's opinion is erroneous in respect to the geological position of the slate district, north of Dublin, and of that between Drogheda and Dundalk.

I have now replied to all the important observations contained in Mr. Hamilton's paper, which tend to cast a doubt on the accuracy of my Geological map, and I think I have been

* See Journal of the Geological Society of Dublin, vol. ii. part i.

† The fossils found, and which have been compared with the original Silurian forms collected by Mr. Murchison, are *Orthis semicircularis* and *Orthis virgata*; in addition to which there are several other forms of the genus *Orthis* which have not as yet been clearly identified.

able to show, in each case, that my original views have been supported by a careful *re-examination* of the facts.

Dublin, January 17, 1840.

XXXI. *On Haydenite and Couzeranite.* By H. J. BROOKE, Esq., F.R.S.*

IN the last Number of the Phil. Mag. p. 156, I observed a notice of a new mineral (Beaumontite) which M. Levy has found accompanying Haydenite, a mineral that is said to occur near Baltimore in the United States; and M. Levy remarks that I have classed Haydenite with Heulandite without assigning any reason for so doing.

I did so from perceiving that the small and brilliant yellowish crystals on the specimen I examined resembled Heulandite, in having a nacreous plane in only one direction, corresponding with the P of W. Phillips (Mineralogy, p. 39. Ed. 1823); in the form, as far as I could distinguish it in the minute and closely aggregated condition of the crystals; and in the near agreement, about 112° , of the angle between the nacreous plane and a plane appearing to correspond with the *a* of W. Phillips.

M. Levy is doubtless aware of a variety of Heulandite found at Arendal, of a brown or yellowish brown colour, in small bright crystals, and accompanied by stilbite in globularly-radiated concretions of a dull yellowish colour. The specimen sent to me from America as Haydenite has a similar accompaniment of stilbite of the same description, and this circumstance tended to confirm my impression that Haydenite was Heulandite. It is probable, therefore, that the specimen sent to me from America as Haydenite, is really not that mineral, and I do not find anything like M. Levy's Beaumontite upon it, except that the crystals are "small and brilliant, and of a pearly lustre." Mr. L. does not say on what faces this lustre appears in Beaumontite.

I find I have been formerly led into an error relative to Couzeranite by specimens received from Paris, by Mr. Heuland, from Mr. Pentland. On examining the crystals I found them to be felspar, and I accordingly stated, on the faith of the specimens so transmitted to Mr. Heuland being genuine, that Couzeranite was only felspar. I have since seen other specimens named Couzeranite in apparently square prisms, and if these are the true mineral it has no resemblance whatever to felspar.

H. J. B.

* Communicated by the Author.

XXXII. *Observations on the relative Temperature of the Sea and Air, and on other Phænomena, made during a Voyage from England to India. By the Rev. J. H. PRATT, M.A., Member of the Asiatic Society of Bengal.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I SEND you the accompanying observations on the relative temperature of the sea and the superincumbent air made at various latitudes and longitudes on a voyage from England to India, in case you should deem them of sufficient importance to give them a place in your valuable Magazine.

I regret that they do not extend through the whole voyage. It was not till after passing the Cape that I recorded any observations. I was anxious to see what effect the Mozambique Channel had upon the currents; and after that I continued my observations up to the Bay of Bengal.

I have also given the result of a few observations on the velocity of the waves of a swell in unfathomable water.

Once or twice I attempted to ascertain the temperature of the sea water at a considerable depth, such as 40 and 100 fathoms. The method I adopted was this: I sunk a quart bottle, full of sea-water and well-corked, by means of a line, and allowed it to remain a considerable time (as an hour or more), that the water within the bottle might attain the temperature of the surrounding water by conduction. I then drew it up with great rapidity (perhaps in $1\frac{1}{2}$ minute), instantly uncorked the bottle, and tried the temperature. When I poured the water into a glass it would change its temperature very little in 5 or 10 minutes; so I felt assured that no *great* change could have taken place in its passage from its lowest depth. I should have made more of these observations, but the utter impracticability of sinking a bottle well, except in a dead calm, prevented this. One day I had a bottle 200 fathoms deep for one or two hours; a gentle breeze sprang up, my bottle towed astern, and in pulling it in the line broke!

A notice of my observations will be seen in the accompanying tables of temperature.

I am, Gentlemen, yours, &c.

Bishop's Palace, Calcutta,
March 22, 1839.

JOHN HENRY PRATT.

Results of a Series of Experiments on the Temperature of the Sea, made by J. H. PRATT, M.A., on board the ship Duke of Buccleugh, bound for Calcutta.

The experiments were begun on the east of the Cape.

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		General Remarks.
				Sea.	Air.	
1838.						
Nov. 17.	37° 37' S.	20° 40' E.	11 p.m.	69° 25	66°	
18.	37 40	24 14	10½	64·25	64·25	Clear sky.
19.	37 54	28 30	9 a.m.	65	68	Clear.
			10½ p.m.	69·25	64·50	Cloudy.
20.	37 46	33 42	8 a.m.	63·75	59	Clear.
			3 p.m.	64·75	61	
			11	69·75	62·50	Clear (starlight).
21.	38 11	37 51	7 a.m.	68·25	64	Clear.
			9	68·25	64·25	Clear.
			noon.	66·25	64	
			2 p.m.	64·50	64·50	Clear.
			5	62·50	62·50	Violent squall.
			7	62·50	57	Immediately after heavy rain.
			8½	62	58	bet. squalls.
			10	62	58	Cloudy.
			midnight.	62	56	Starlight.
22.	37 55	42 20	5½ a.m.	67	58	Cloudy.
			7½	67·25	58	
			9	67·25	58	
			10½	68	59	
			noon.	67	59	Clear.
			2 p.m.	65½	59	
			5	59·75	61·50	} Sea became remarkably green: passing through comp. shallow water.
			7	58	59	
			9	55·25	55·25	
			11	62	58	Clear.
23.	37 46	47 4	4 a.m.	65·25	59	Clear.
			7½	64·50	60	
			9	63·50	59·25	Cloudy.
			11	63	62	Clear.
			noon.	63·50	61·75	
			2½ p.m.	63·50	64·50	
			4½	63·50	64·50	
			7	63·50	64	
			8½	63	63·50	Cloudy.
			10¼	63	63·50	
			11½	63	62·50	
24.	38 14	51 50	7½ a.m.	64·75	64	Cloudy.
			9	63·25	64·50	
			10½	63·25	65	
			12½ p.m.	63	65·50	Clear.

TABLE continued.

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		Remarks.
				Sea.	Air.	
1838. Nov. 24.	38° 14' S.	51° 50' E.	2 p.m.	63.50	66	Clear.
			5	64	67.50	
			8 $\frac{1}{2}$	62	64	Cloudy.
			10 $\frac{1}{2}$	61	63	
			12	62	64	
25.	37 55	57 20	12 p.m.	61.50	63.50	Clear.
26.	38 48	62 2	8 a.m.	60.25	60	Cloudy.
			10	60	61	Clear.
			2 p.m.	60.75	62.75	
			5	60.50	60.50	Cloudy.
			7	60.50	59.50	Clear.
			10	60.50	59	Cloudy.
			12	60.25	60	
27.	38 48	64 0	8 a.m.	60	55.75	Rain.
			noon.	60	56.50	
			10 p.m.	60.50	59	Cloudy.
			12 p.m.	60	56	
28.	38 21	67 50	7 $\frac{1}{2}$ a.m.	59	55.50	
			2 p.m.	58.75	56	Clear.
			10 $\frac{1}{2}$	59	56.50	Cloudy.
29.	37 2	70 35	8 a.m.	59.25	57	
			10 $\frac{1}{2}$	59.25	57.75	Clear.
			1 p.m.	60	57	
			10	59	57	Cloudy.
30.	37 30	72 5	8 a.m.	58.50	59	
			2 p.m.	58	61.50	
			6	57.50	59	Rain.
			10 $\frac{1}{2}$	57.75	59.25	Cloudy.
Dec. 1.	37 16	77 1	8 a.m.	57	60.75	
			2 p.m.	58.50	61	Clear.
			8	60	61	Cloudy.
			10 $\frac{1}{2}$	59.25	61	
2.	36 12	81 8	10 p.m.	59	62.25	
3.	34 25	82 50	7 $\frac{1}{2}$ a.m.	60	62	Fog.
			noon.	62.75	66	Cloudy.
			6 p.m.	63	65	
			10	63.50	64 $\frac{1}{2}$	
4.	31 27	85 3	7 $\frac{1}{2}$ a.m.	66.25	67	
			9	66.25	67.50	
			2 $\frac{1}{3}$ p.m.	69	71.50	Clear.
			10	67.50	68	
			12	67.25	67.50	
5.	28 25	86 37	7 a.m.	68.25	68.50	Clear.
			9	68.50	69.50	
			11	69	71.25	
			1 p.m.	69.25	72	
			3	70	73	
			5	71	72	
			7	71.50	71.50	Cloudy.

TABLE continued.

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		Remarks.
				Sea.	Air.	
1838.						
Dec. 5.	28° 25' S.	86° 37' E.	9 a.m.	71·75	71·50	Clear.
			11	71·50	72	Cloudy.
6.	26 8	88 53	7 a.m.	72·25	71·25	
			9	72	71·50	Clear.
			11	73	73·75	Cloudy.
			2 p.m.	73·50	73·50	
			5	73·50	73·50	
			7	73·50	72·75	
			10	73	72	
7.	24 24	89 10	9 a.m.	73·75	73·25	
			1½ p.m.	74	74·50	
			5	74	75	Clear.
			7	73·50	73·50	
			10	74·75	73	
8.	21 2	89 12	7 a.m.	76·75	74	Cloudy.
			9	76·50	76	
			11	77·25	76	Clear.
			1½ p.m.	78	76·50	
			5	77·25	76·50	
			6½	76	76·50	
			10½	76	75·50	
			12	76	75	
9.	17 48	88 45	10 p.m.	79	78½	Cloudy.
10.	13 55	89 2	7 a.m.	80	80	
			9	80·25	79	} very squally
			11½	80·50	80·50	
			5 p.m.	80·50	79·50	
			10	80	79	
11.	10 55	88 30	6 a.m.	79·50	78	Violent squalls at
			8	80·25	80	Clear. } [night.
			noon.	80·25	80	} Gale.
			10 p.m.	80·25	79	
12.	11 35	88 4	7 a.m.	80	79	Rain and heavy
			10 p.m.	80	80	squalls.
13.	12 0	87 8	10 a.m.	79·75	81	} Rain, heavy
			10 p.m.	79·25	78	
14.	11 30	86 28	8 a.m.	80	70	} squalls.
			2 p.m.	80	82	
			12	79·50	79·50	Clear.
15.	11 11	86 42	6 a.m.	80	79·50	
			9	80	79·50	
			12 p.m.	80	80	
16.	10 52	87 8	10 p.m.	80	80	
17.	10 8	85 47	7 a.m.	80·75	81	
			2½ p.m.	81·75	82·50	
			1 0	81	81	
18.	9 43	85 13	7½ a.m.	81	81·50	
			noon.	81	82	
			12 p.m.	81	80·50	

TABLE continued.

Day.	Lat. at noon.	Long. at noon.	Hour.	Temperature.		Remarks.
				Sea.	Air.	
1838.						
Dec. 19.	8° 21' S.	85° 0' E.	7 a.m.	81.25	81.25	Clear.
			noon.	82.50	84	— { A bottle was sunk for an hour or so ; 40 fa- thoms (about noon), when brought up 81°-50 temp. of water.
20.	7 54	85 20	9 a.m.	82.50	82	
			noon.	84	82.50	
			2 p.m.	84	84	
			10½	81.75	84	— { Bottle sunk 100 fathoms. Temp. 78°.
21.	7 29	85 18	9 a.m.	82.50	82	
			noon.	83	82.50	
			2 p.m.	85	83.50	
25.	1 56	87 0	10 p.m.	83.50	81.25	Clear.
26.	1 6	87 0	7 a.m.	84	81	Rain and squalls.
			2 p.m.	85	84	
			10	84	79	
27.	29' S.	87 20	7½ a.m.	84.25	82.25	
			2½ p.m.	85	85.25	Clear.
			10	84	83	
28.	24' N.	88 25	7½ a.m.	84	82½	
			2½ p.m.	84	83	
			10	83½	82	
29.	1 30	89 16	7½ a.m.	83.50	82	
			10½ p.m.	83.25	82	
30.	4 14	91 8	noon.	83	84.50	
			12 p.m.	82.50	82	
1839.						Cloudy ; bottle sunk 100 f. 70°. exp. well made.
Jan. 1.	4 10	91 28	7 a.m.	82.50	82	Clear.
			10	83	83	
7.	11 50	88 50	2 p.m.	80.50	80	
			10	80	80	
8.	13 2	88 57	8 a.m.	80	79	
			1 p.m.	80.50	81	
			10	80	79	
9.	13 54	88 9	8 a.m.	80	79	
			noon.	80	79	
			6 p.m.	80.50	78.50	
			10	80	78	
10.	14 22	88 35	8 a.m.	81	78.50	
			noon.	81.25	80	
			10 p.m.	80.50	78	
11.	14 59	88 23	noon.	80.50	81	
			10 p.m.	80.50	78	
12.	15 57	88 5	8 a.m.	80	79	
			noon.	80	81	
			10 p.m.	78	78	
14.	17 20	88 10	9 a.m.	77.75	77.75	
			2½ p.m.	78.50	81.50	
			10	77.75	77.75	
15.	18 15	88 55	8 a.m.	77.50	78	
16.	20 49	88 47	noon.	77.50	76	
			10 p.m.	76.50	73	
17.	Mouth of	Hoogly.	8 a.m.	75	71.50	

I made the following experiment on the velocity of waves out at sea. Lat. $27^{\circ} 2'$ S. Long. $27^{\circ} 25'$ W.

There was a *swell* on the sea moving from fore aft; wind only sufficient to carry the vessel (all sails set) *steadily* two or three miles an hour. Two large floats were connected by a line forty fathoms in length, the line itself being supported on the surface of the water by smaller floats. This apparatus was towed astern by a long line connected with one of the large floats by one end, the other end being wound round a reel.

The chief officer watched the chronometer; the second officer held the reel fixed; and I observed the large floats.

A few seconds before the first float was raised to its greatest height by a given wave, I gave a signal to the second officer to let the reel run, and immediately the floats became stationary in the water.

At the instant the first float, and also at the instant the second float, was raised to its greatest height by the wave already mentioned, I gave audible signals to the chief officer, who marked the interval of time between the signals.

A very good average of many trials gave a trifle less than six seconds of time for the motion of the wave from float to float, i. e. over forty fathoms. This gives nearly $27\frac{1}{3}$ statute miles an hour.

The chief officer and I changed places, and came to the same result. Two days afterwards this was confirmed, though in rather a rough manner, by observing the motion of the vessel by a swell moving abaft.

Calcutta.

J. H. P.

XXXIII. *Researches in the Undulatory Theory of Light continued: On the Absorption of Light.* By JOHN TOVEY, Esq.

(Continued from p. 455 of last Volume.)

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

YOU will have observed that the formulæ of my last communication are deduced from the fundamental principles of the undulatory theory without the aid of any assumption respecting the arrangement of the molecules, or the nature of the constant quantities, k , &c., which appear in the integration. Those formulæ indicate, as was shown, that the transmission of the light may be accompanied by an absorption, or diminution of its intensity, depending on the nature and thickness of the medium through which it is transmitted,

and on the length of the wave. This, then, is a *general result* from the theory, and it agrees, as we know, with experience.

We shall now proceed with the investigation, in order to compare the theory with experiment a little further.

By (14.) and (23.) we have

$$\kappa = \varepsilon + k\sqrt{-1}, \quad (36.)$$

and, by transformation,

$$e^{k\Delta x\sqrt{-1}} = \cos k\Delta x + \sqrt{-1} \sin k\Delta x; \quad (37.)$$

hence, if we put

$$\begin{aligned} e^{\varepsilon\Delta x} \cos k\Delta x - 1 &= u, \\ e^{\varepsilon\Delta x} \sin k\Delta x &= u', \end{aligned} \quad (38.)$$

we have, by (8),

$$\begin{aligned} s &= \Sigma p u + \sqrt{-1} \Sigma p u', \\ s' &= \Sigma p' u + \sqrt{-1} \Sigma p' u', \\ s_l &= \Sigma q u + \sqrt{-1} \Sigma q u'. \end{aligned} \quad (39.)$$

If we compare these equations with (20.) we shall find

$$\begin{aligned} \sigma &= \Sigma p u, & \sigma' &= \Sigma p' u, \\ \sigma_l &= \Sigma p u', & \sigma'_l &= \Sigma p' u', \\ \sigma_2 &= \Sigma q u, & \sigma_3 &= \Sigma q u'. \end{aligned} \quad (40.)$$

By (13.) and (20.) we find

$$(n^2 + \sigma + \sqrt{-1} \sigma_l) (n^2 + \sigma' + \sqrt{-1} \sigma'_l) = (\sigma_2 + \sqrt{-1} \sigma_3)^2,$$

which, since n is real, gives

$$\begin{aligned} (n^2 + \sigma) (n^2 + \sigma') - \sigma_l \sigma'_l &= \sigma_2^2 - \sigma_3^2, \\ (n^2 + \sigma) \sigma'_l + (n^2 + \sigma') \sigma_l &= 2 \sigma_2 \sigma_3. \end{aligned} \quad (41.)$$

Hence, by eliminating n^2 , and reducing, we find

$$\begin{aligned} (2 \sigma_2 \sigma_3 - (\sigma_l - \sigma) \sigma_l) (2 \sigma_2 \sigma_3 + (\sigma'_l - \sigma') \sigma'_l) \\ = (\sigma_l \sigma'_l + \sigma_2 - \sigma_3) (\sigma'_l + \sigma_1)^2, \end{aligned} \quad (42.)$$

which, as appears by (38.) and (40.), expresses, implicitly, the relation between ε and k .

To obtain a precise idea of the movement represented by the expressions (35.), suppose the arbitrary coefficients denoted by a to be all zero except one; then each of the sums will be reduced to a single term, so that we shall have

$$\begin{aligned} \eta &= a e^{\varepsilon x} \sin (n t + k x + b), \\ \zeta &= \beta a e^{\varepsilon x} \sin (n t + k x + b + \gamma). \end{aligned} \quad (43.)$$

Put $a e^{\epsilon x} = \alpha$, $n t + k x + b = \omega$, then $\eta = \alpha \sin \omega$, $\zeta = \beta \alpha \sin (\omega + \gamma) = \beta \alpha (\cos \gamma \sin \omega + \sin \gamma \cos \omega)$; which last expression for ζ gives

$$\begin{aligned} (\zeta - \beta \alpha \cos \gamma \sin \omega)^2 &= (\beta \alpha \sin \gamma \cos \omega)^2 \\ &= (\beta \alpha \sin \gamma)^2 (1 - \sin^2 \omega). \end{aligned}$$

But, since $\alpha \sin \omega = \eta$, this equation gives

$$(\zeta - \beta \cos \gamma \cdot \eta)^2 = (\beta \alpha \sin \gamma)^2 - (\beta \sin \gamma \cdot \eta)^2:$$

hence we find

$$\frac{\eta^2}{\alpha^2} + \frac{\zeta^2}{\beta^2 \alpha^2 \sin^2 \gamma} - \frac{2 \cos \gamma \cdot \eta \zeta}{\beta \alpha^2 \sin^2 \gamma} = 1, \quad (44.)$$

an equation to an ellipse of which η and ζ are the coordinates.

Consequently, when the system is in the state of motion expressed by the equations (43.) every molecule describes an ellipse round its place of rest; and the equations (35.) show that the general motion of the system is equivalent to a number of coexisting motions of the same kind.

The period of the revolutions of the molecules, in the movement represented by (43.), is equal to $\frac{2 \pi}{n}$; where 2π is the circumference of a circle whose radius is unity. And this movement is transmitted through the medium in a series of continuous waves; the length, or rather thickness, of each wave being $\frac{2 \pi}{k}$. The direction in which the waves travel depends on the sign of k , supposing that of n to continue the same. But, by the equations (22.), it appears that the sign of n is arbitrary: therefore n as well as the arbitrary quantities a and b , may be written either positively or negatively. Now if we change the signs of a , n , b , in (43.), it is virtually the same thing as changing the signs of k and γ , while those of a , n , b , remain the same. Consequently, when we take for the positive direction of x , that in which the waves travel, we may write the equations (43.) thus:

$$\begin{aligned} \eta &= a e^{\epsilon x} \sin (n t - k x + b), \\ \zeta &= \beta a e^{\epsilon x} \sin (n t - k x + b - \gamma), \end{aligned} \quad (45.)$$

and suppose n and k to be positive.

The intensity of the light is considered to be measured by the *vis viva* of the molecules, which, when other things are equal, is proportional to the square of the amplitude of vibration. Thus, when the movement is represented by (45.), the

intensity is proportional to $a^2 e^{2\epsilon x} + \beta^2 a^2 e^{2\epsilon x}$: hence, if we put $c = a^2 + \beta^2 a^2$, it will be proportional to $c e^{2\epsilon x}$. Suppose the origin of x to be at the surface of any medium on which the light falls; then $c e^{2\epsilon x}$ will be the intensity of the light after it has traversed a thickness of the medium equal to x . And if $c_1, c_2, c_3, \dots \epsilon_1, \epsilon_2, \epsilon_3, \dots$ be the values of $(a^2 + \beta^2 a^2)$ and ϵ respectively in the general expressions (35.), the intensity of the light in the transmitted ray will be

$$c_1 e^{2\epsilon_1 x} + c_2 e^{2\epsilon_2 x} + c_3 e^{2\epsilon_3 x} + \&c. \quad (46.)$$

If we were to put y, y', y'', \dots for $e^{2\epsilon_1}, e^{2\epsilon_2}, e^{2\epsilon_3}, \dots$ in this formula, it would become the very same as that which was devised by Sir John Herschel to represent the law of absorption as indicated by experiments.

The formula (46.) shows clearly enough the manner in which the absorption depends on the thickness of the medium, and it indicates, by the different values of ϵ , which belong respectively to different values of k , that the absorption is different for waves of different lengths. But the relation of ϵ to k , which, as we have seen, is implicitly expressed by the equation (42.), is so extremely complicated that the readiest way of testing our theory with reference to it, seems to be by inquiring whether experiments show that it is of so complex a character. Now the nature of this relation, as inferred from experiments, is stated by Sir J. Herschel in his excellent paper on the absorption of light, published in vol. iii. of the current series of your Journal, where at page 402, he says:—"If we represent the total intensity of the light, in any point of a partially absorbed spectrum, by the ordinate of a curve, whose abscissa indicates the place of the ray in the order of refrangibility, it will be evident from the enormous number of maxima and minima it admits, and from the sudden starts and frequent annihilations of its value through a considerable amplitude of its abscissa, that its equation, if reducible at all to analytical expression, must be of a singular and complex nature, and must at all events involve a great number of arbitrary constants, dependent on the relation of the medium to light, as well as transcendents of a high and intricate order." This character is very suitable to our equation (42.), and may, therefore, be taken as an evidence of its truth.

That a spectrum absorbed in the apparently capricious manner described in the above extract, would result from the relation between ϵ and k implied in (42.) may be thus shown. Suppose, in the first place, ϵ to be zero, and k_1, k_2, k_3, \dots

to be the roots of the equation in that case: then the waves of which the lengths are $\frac{2\pi}{k_1}$, $\frac{2\pi}{k_2}$, $\frac{2\pi}{k_3}$, ... will be transmitted without absorption, and consequently will form a number of bright lines in the spectrum, yet probably too few to afford, by themselves, any sensible light. Now suppose ϵ to decrease gradually, then all the roots k_1, k_2, k_3, \dots will vary, but not with equal rapidity. Some of them may be changed in magnitude considerably by a very small change in ϵ , and, consequently, in the parts of the spectrum to which these roots respectively correspond, there will be bright bands. Other roots may be only slightly affected by a considerable change in ϵ ; hence there will be, in the parts of the spectrum which correspond to these roots, rapid variations in the intensity of the light, producing dark bands or dark lines.

Perhaps the equation (42.) which we are considering, may, in certain cases, be much simplified; but I cannot proceed with the subject any further in the present paper.

I am, Gentlemen, yours, &c.,

Littlemoor, Clitheroe, Feb. 6, 1840.

JOHN TOVEY.

P.S. In my last paper, vol. xv. p. 451, last line but three, *for increasing indefinitely read increasing or diminishing indefinitely*, —p. 452, line 14, *for* $\cos mi = \sqrt{-1} \cdot \sin mi$ *read* $\cos mi + \sqrt{-1} \cdot \sin mi$; —p. 453, line 28, *for* ρ, α , *read* ρ_1, α_1 ; and line 29, *for* α , *read* α_1 ; —p. 454, line 21, *for* (23.) *read* (33.); lines 22 and 23, *for* e^{ex} *read* $e^{\epsilon x}$

XXXIV.—*On the Direction and Mode of Propagation of the Electric Force traversing Interposed Media.* By GEORGE J. KNOX, Esq., A.M., M.R.I.A.*

WHATEVER theory be adopted to explain the passage of the electric force traversing an intervening fluid or solid substance not undergoing electrolyzation,—whether we suppose it to originate in an inductive influence affecting the circumambient æther of each particle of the substance in the line of direction of the force, in whose alternate states of induction and equilibrium consists the passage of the electric current, (the rapidity of such changes constituting its intensity,) while the vibratory motion produced in the particles of the æther on each successive return to a state of equilibrium causes the

* From the Transactions of the Royal Irish Academy, vol. xix.

phænomena of the light and heat developed ; or whether we adopt the gross conception of the passage of a fluid ; still it is important to determine if the electric force passes along the surface of the interposed substance, or through the interior of its mass.

Dr. Faraday* has shown that water will convey a feeble current of electricity, without undergoing electrolyzation. To determine whether, under such circumstances, it will convey an electrical current along its surface or through its substance, a glass tube, ten feet long, and half an inch internal diameter, bent in the centre twice at right angles, was filled with distilled water. Two copper wires, twenty feet long, having platina wires soldered to their extremities, were inserted in barometer tubes of six feet in length, the platina wires being sealed in the tubes within half an inch of their extremities. The other ends of the copper wires were connected with a delicate galvanometer, and a constant battery of successively one, two, four, &c., pair of elements.

On immersing the platina wires in the liquid, their relative distances from each other should decrease if the current passes through the water, but should increase if it passes along the surface, the deflexion of the galvanometer indicating the path. With one pair of elements there was no deflexion of the galvanometer ; with two pair of elements there was a slight deflexion visible through a lens, which increased slightly on immersing the platina wires in the liquid. With four pair of elements, a deflexion of two degrees took place when the platina wires were on the surface of the water ; a deflexion of four degrees when they were immersed to the bottom of the tubes. As the number of alternations in the battery increased, so did proportionably the comparative deflexions of the galvanometer ; the experiments proving that water, whether undergoing electrolization or not, conveys an electric current *through its substance*, and not *along its surface*, and that the decomposition of the water is an effect produced by the passage of the electricity when of sufficient intensity, and not the necessary consequence of its passage.

A similar experiment having been tried with phosphorus melted under spirits of wine, (being a non-conductor,) it was found to obey the same law with water ; that is, to convey the current through its substance†.

To determine whether the metals followed the same law, I suspended from the top of the new patent shot tower at Wa-

* Series VIII. (970.)

† It was unnecessary to try similar experiments with the analogous bodies, sulphur, selenium, and iodine.

terloo-bridge a leaden pipe, 170 feet long, and three-fourths of an inch internal diameter, through which was drawn an insulated copper wire, 180 feet long, one extremity of which being soldered to the inside of the end of the pipe, this end was sealed with fused metal, and to its external surface was soldered a copper wire of the same length as the former; round the tube, at its orifice, was twisted a copper wire ten feet long. The insulated wire being connected with a constant battery of one pair of elements in contact with one pole of an exceedingly delicate galvanometer, (constructed by Mr. E. M. Clarke of the Lowther Arcade,) the other pole of the galvanometer was brought successively in contact with the extremities of the uninsulated wires. The deflexion was greater when the current passed along the wire connected with the orifice of the tube, (although here the contact was not so good,) than when it passed along that soldered to the sealed extremity.

Again, the uninsulated wires being connected with separate galvanometers, so as to allow the current of electricity to pass along either of the uninsulated wires alone, or to be distributed between both, it was found (as well as could be determined by transposing the galvanometers,) to have divided itself into two equal currents flowing along both wires.

From the first experiment we may infer that a current of electricity passes with greater facility along the surface of a metal than through the interior of its mass, although we cannot hereby infer that it could not pass through the interior of the metal, when this is the only road open for its transit*.

To the experiments with phosphorus it might be objected that its capability for conducting an electric current is due to the presence of water, of which some have supposed that it could not be entirely deprived, although the experiments of Sir H. Davy, wherein he obtained hydrogen and oxygen from sulphur and phosphorus by heating them in contact with potassium and sodium, and by submitting them to the electrolytic action of a powerful galvanic battery, did not prove that they were united with the basis of these substances in such proportions as to form water, nor indeed does he appear to have entertained such an opinion himself. His opinion of the na-

* The high conducting power of mercury for electricity renders it almost impossible to determine, by this method, whether metals in the *fluid* state obey the same laws of conduction as when in the solid state. If they do not, it is highly probable there is a general law, that *all solids conduct along their surface, and all fluids through their substance*. The investigation of such general law I propose to continue in another paper.

ture of sulphur was, that it was “a compound of small quantities of oxygen and hydrogen, with a large quantity of a basis, that produces the acids of sulphur in combustion, and which, on account of its strong attraction for other bodies, will probably be difficult to obtain in its pure form*.” To put the question beyond any further doubt, I will mention some experiments which I tried in the laboratory of the Royal Dublin Society in the year 1837, having had, through the kindness of Professor Davy, a galvanic battery of sixty pair of plates, five inches square, put at my disposal.

When fused phosphorus, sulphur, selenium and iodine, were submitted separately to the action of this battery charged with a strong acid solution, they conveyed the electrical current freely during the whole time, giving a spark whenever contact was broken; yet at the end of two hours they showed not the slightest trace of decomposition, no gas being evolved at either pole, which would have been the case had there been any water present.

Having by these experiments shown the *direction* of propagation of the electric force, I will now consider the source from which it originates in the voltaic pile, the mode of its transfer, and its sustaining principle.

Sir H. Davy’s† opinion that the contact of the metals was the *primum mobile* of voltaic excitement, having been proved by Dr. Faraday‡ to be erroneous, chemists are now pretty generally agreed that the electrical force developed in the voltaic pile is due altogether to chemical action, concerning which there are different opinions; of these, I will mention two, which are the most applicable to the present argument—Dr. Faraday’s§ and Mr. Becquerel’s||. The former supposes that the development of electricity is due to decomposition alone, and in no case to the chemical union of bodies; while the latter contends that it is due to both, and in proof of his opinion shows that when an alkali unites with an acid, with a neutral salt, and in fact with any solution whose natural state is with regard to it electrically negative, a current of electricity will flow from the alkali to that solution. Sir H. Davy¶ has taken a different view of these experiments from Mr. Becquerel, supposing that the electric current is produced by the action of the acid or alkali upon the platinum plates; but the latter has shown that the electrical current is produced equally when

* Bakerian Lecture, 1809.

† Eighth Series, (880).

‡ Tom. ii. from page 77 to 81.

† Phil. Trans., Bakerian Lecture, 1826.

§ Eighth Series, (927) (928).

¶ Phil. Trans., Bakerian Lecture, 1826.

no such action could take place, the platinum poles being placed in separate cups filled with water*.

The accuracy then of Mr. Becquerel's experiments having been fully established, the question arises, how are we to reconcile them with other well-known contradictory facts? such as for instance those of Sir H. Davy†,—solid potash and sulphuric acid combining in an isolated platinum crucible, and causing no electrical development. Again, a plate of copper and of sulphur, when heated, have their electrical states increased until chemical action begins, when they cease.

The simplest and clearest course, and that most reconcileable with the laws of statical electricity, seems to me to be:—to consider that no electrical development is caused by the *union* of an alkali with an acid, (the electricity being thereby disguised,) but that, at the *instant before* the union takes place, the particles of the alkali and of the acid, being in opposite electrical states, affect their surrounding particles by induction, causing thereby a feeble current of electricity to circulate from the acid through the galvanometer to the alkali, which supposition is borne out by the fact, that a dry acid and alkali, when in contact, show opposite electrical states.

The same arguments apply equally well with regard to thermo-electricity. The contact of two metals produces in them opposite electrical states. Their chemical union in an isolated vessel gives no electrical development; thus a “solid amalgam of bismuth and lead become liquid when mixed together, without producing any electrical effect‡.” Again, “a thin plate of zinc placed upon a surface of mercury, and separated by an insulating body, is found to be positive, the mercury negative; but when kept together a sufficiently long time to *amalgamate*, the compound gives no signs of electricity‡.”

These experiments explain why the *contact* of the two extremities of metallic wires, constituting a closed circuit, should, as the potash and nitric acid just mentioned, produce an induced electric current. That the electric states of different metals in contact, when excited by heat, do not follow the law of their natural electrical states, and change on increase of temperature, is no argument against the explanation I have given, for upon what this change in the electrical excitation

* He might have added another experiment, free from all objections—namely, the increased intensity consequent upon an increased number of alternations of acid and alkali.

† Phil. Trans., Bakerian Lecture, 1807.

‡ Ibid.

produced by heat depends, whether upon a peculiar arrangement of the crystalline parts of the metal, or of their compound elementary particles, we are as yet perfectly ignorant.

That the same general law of the contact of metals and of fluids applies equally (although in an inferior degree, owing to their want of conducting power) to the contact of the gases, may be shown by the experiment of Dr. Faraday (Sixth Series) of the union of hydrogen and oxygen by a plate of platinum; the electrical force, which circulates by the interposed platinum plate, facilitating the union of the two gases*.

To return to the source of the voltaic force in the battery. Zinc, when placed in contact with a dry acid, has been found to become positively electrified. When the zinc plate has been immersed in the acid solution, being positive, it attracts oxygen, by union with which its electrical state is disguised, while the hydrogen, set free in a highly positive electrical state, reacts upon the oxide of zinc, rendering it negative by induction. The platinum wire connecting the positive solution with the negative zinc plate, reduces all for the moment to a state of equilibrium, so that the electricity becomes disguised, not transferred bodily from the platinum to the zinc; which state of equilibrium is no sooner restored than it is destroyed, the zinc regaining its positive state, and the oxide being removed by the acid.

If we consider then what takes place, we shall perceive that the zinc plate undergoes alternate states of induction and equilibrium, as do likewise the particles of the solution between the zinc and platinum plates, and, in fine, the platinum plate itself, and that as the number of alternations of zinc and platinum increases, the electrical energy of the zinc plate increases, as does also *the rapidity of its oxidation and deoxidation, and as a consequence the rapidity of change of induction and equilibrium upon which the intensity of the current depends.*

The decomposition of the electrolyte may be considered to be the effect produced by two forces acting upon its particles; the attraction of the poles† of the battery (whether they be

* Aqueous solutions of different gases, when brought into contact, have been found to produce electrical currents.

† In place of *poles*, I should more properly have said *electrodes*, their bounding surfaces. It follows, as a consequence of the theory, that the particles of oxygen in contact with the electrodes should be attracted by, and set free from, those electrodes upon each alternation of the states of induction and equilibrium; and that, when the induced state has not sufficient energy to overcome the affinities already engaged, the current of electricity passes without producing electrolyzation. For a different explanation, vid. Dr. Faraday's Series of Researches, 493, 494, 495, 534, 535, 536, 537, 807.

metal, water, or air) *originating*, while the electrical states induced upon the particles give the *direction* to the electrolytic action.

From what has been said above, we may, I think, presume that an electric current originates in a natural electro-inductive power of bodies when brought into *contact*, and is continued by alternate states of induction and equilibrium, the *rapidity* of change of state constituting its *intensity*. And inasmuch as the accumulation of the electric æther on the surface of the particles by the inductive force, and its recession on each return to a state of equilibrium produces what may be called an oscillation in the æther, the theory may be otherwise stated thus:—the mass of oscillating æther which surrounds the particles constitutes the quantity, while the rapidity of the oscillations constitutes the intensity of an electric current.

The late experiments of Dr. Faraday upon induction (Eleventh Series) showing that an insulated body (the particles of bodies may be presumed to be such) cannot receive an absolute charge of electricity, but only an inductive charge, afford a strong argument in favour of my views.

The theory proposed in this paper, and deduced from the experiments of Sir H. Davy, given in his Bakerian Lectures, is an extension of the views therein developed, reconciles the contact with the chemical theory, and reduces to the laws of *statical* electricity *all* the phænomena of electricity in motion. I will now endeavour to show how the law of the definite nature of electro-chemical decomposition, so beautifully developed by Dr. Faraday, follows as a consequence from this theory. Were the particles of all bodies endued with the same quantity of electricity, and of the same density, it is evident from the laws of statical electricity, that no one body could have an attraction or repulsion for another; consequently, it is an evident fact, that the quantity and density of the electric æther varies in different bodies; and as, from the theory above stated, electricity never leaves the particles, but merely (to use the words of statical electricity) accumulates upon the surface, and returns, it follows that the electrical states of the particles of bodies are constant and unalterable, and therefore it is obvious that the law discovered by Dr. Faraday follows as a consequence from this hypothesis, which is at once clear and simple, which includes all the phænomena, and is but a reference of the laws of *statical* electricity to the *particles* of bodies in place of their *masses*.

*Researches on Fluorine. By G. J. Knox, A.M., M.R.I.A.**1. *On the Insulation of Fluorine.*

“In a paper on the Insulation of Fluorine which the Rev. Thomas Knox and I had the honour of presenting to the Royal Irish Academy in the year 1837†, and which was afterwards published in their Transactions, (vol. xviii. p. 127,) we proved that we had obtained fluorine in an insulated state, by showing its action upon bismuth, palladium, and gold; but being unable, from our mode of experimenting, to determine what the nature of fluorine at ordinary temperatures might be, i. e. whether it be a solid, a liquid, or a gas, we suggested that such information might be obtained from the electrolyzation of a fluoride, using as the positive electrode some substance with which this energetic principle should not enter into chemical combination.

“Finding that, since the publication of our paper, no person had entered upon this field of investigation, I considered that the ultimate solution of this problem devolved as a point of duty upon myself; under which impression I undertook the following experiments.

“A fluorspar stopper was made to fit the mouth of one of the fluorspar vessels described in our former paper; that part of the stopper within the vessel being made of the form of a semi-cone, the vertex of which reached nearly to the bottom of the vessel. Through the stopper were drilled vertically three small holes, one through its entire length, the other two through one-third of its length. In the first was inserted a platinum wire, to be used as the negative electrode; in one of the two small holes was inserted a thin platinum wire, bound round a piece of charcoal, intended to form the positive electrode; in the other hole I put gold-leaf, litmus, or any other substance upon which I wished to try the action of the gas. Matters being so arranged, the fluorspar vessel was about half filled with anhydrous hydrofluoric acid, the chemical purity of which had been previously ascertained. The platinum wire forming the negative electrode was raised a little above the bottom of the stopper, in order to allow the bubbles of hydrogen to rise through the perforation in the stopper, in place of mixing with the fluorine in the vessel; the wires were then placed in contact with the poles of a constant battery of sixty pair of plates, and

* From the Proceedings of the Royal Irish Academy.

† See Lond. & Edinb. Phil. Mag., vol. ix. p. 107.

the action was allowed to continue for the space of two hours; at the end of which time the litmus was found to be reddened, and the gold not acted upon, but a large quantity of subfluoride of iron formed.

In the next experiment I made use of a piece of charcoal, from which the iron had been removed by boiling it in nitric acid; in this experiment there was no subfluoride of iron formed, but the vessel was found to contain fluosilicic acid gas.

In a third experiment a piece of charcoal was employed, which had been previously freed from all metallic impurities and from silica, by being first boiled in pure nitric acid, and afterwards in hydrofluoric acid. Employing this purified charcoal as the positive electrode, I obtained no *immediate* action upon the litmus paper; but after the action had continued for two hours, it was found to be completely bleached, while the gold had undergone no sensible action. That the bleaching was not due to the action of the vapour of hydrofluoric acid was ascertained, by leaving litmus paper for several hours in the neck of a platinum retort, from which hydrofluoric acid was distilling.

The battery was now kept in action for fifteen hours, at the end of which time the vessel being examined, the litmus had disappeared, and the gold-leaf showed signs of having been strongly acted upon, having assumed a dark brownish colour, and having gathered itself into little balls, as if it had undergone the action of heat. The platinum wire was acted upon in those parts where it was in contact with the charcoal, but nowhere else.

When the platinum wire forming the positive electrode passed through the stopper to the bottom of the vessel, the hydrogen, in place of rising through the perforation in the stopper, as in the former instance, rose now into the receiver, where, upon applying a light, it exploded, showing that it does not enter into combination with fluorine without the aid of heat. The presence of the vapour of hydrofluoric acid in the vessel prevented me from determining by other experiments how far fluorine was a supporter of combustion.

To determine the colour of the gas, a stopper of fluorspar similar to the former was made to fit one of the transparent fluorspar receivers formerly described. The gas evolved in the receiver appeared colourless.

As the action of the gas upon glass could not be determined, owing to the presence of the vapour of hydrofluoric acid, I fused in a bent tube of German glass (such as is used in organic analysis) fluoride of lead. The wire holding the

charcoal was made to pass through a cork inserted in one end of the tube, the other platinum wire merely dipped into the fused fluoride. On connecting the wires with the battery, strong electrolytic action commenced, bubbles of gas were evolved rapidly at the surface of the charcoal, which, on arriving at the surface of the fused fluoride of lead, acted instantly upon the glass. The litmus paper was not bleached, nor the gold-leaf or platinum wire acted upon. Whether fluorine would act upon perfectly dry cold glass remains to be proved.

Conclusion.—Fluorine then, when obtained in an insulated state, is a colourless gas, possessing properties analogous in all respects to those of chlorine; having, like it, strong attractive powers for hydrogen and metals, but inferior to it in negative electrical energy.

2. *Note on a Compound of Fluorine with Selenium.*

When the vapour of selenium is passed over fluoride of lead fused in the platinum apparatus which I employed in obtaining the fluorides of carbon and cyanogen, a seleniuret of lead is formed, and crystals similar in form to those of fluoride of carbon are condensed in the cold receiver. These crystals are soluble in strong hydrofluoric acid. They sublime unaltered at a high temperature. They are instantly decomposed by water or acids, in which property they resemble the fluorides of sulphur and phosphorus.

XXXV. *On a simple mode of obtaining from a common Argand Oil Lamp a greatly increased quantity of Light: in a letter from Sir J. HERSCHEL, Bart.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following simple, easy, and unexpensive mode of greatly increasing the quantity of light yielded by a common Argand burner, has been used by me for some years, and is adapted to the lamp by which I write, to my greatly increased comfort. It consists in merely elevating the glass chimney so much above the usual level at which it stands in the burners in ordinary use that its *lower* edge shall clear the *upper* edge of the circular wick by a space equal to about the fourth part of the exterior diameter of the wick itself. This may be done to any lamp of the kind, at a cost of about sixpence, by merely adapting to the frame which supports the chimney four pretty stiff steel wires, bent in such a manner as to form

four long upright hooks, in which the lower end of the chimney rests; or still better if the lamp be so originally constructed as to sustain the chimney at the required elevation without such addition, by thin laminæ of brass or iron, having their planes directed to the axis of the wick.

The proper elevation is best determined by trial; and as the limits within which it is confined are very narrow, it would be best secured by a screw motion applied to the socket on which the laminæ above mentioned are fixed, by which they and the chimney may be elevated or depressed at pleasure, without at the same time raising or lowering the wick. Approximately it may be done in an instant, and the experiment is not a little striking and instructive. Take a common Argand lamp, and alternately raise and depress the chimney vertically from the level where it usually rests, to about as far above the wick, with a moderately quick but steady motion. It will be immediately perceived that a vast difference in the amount of light subsists in the different positions of the chimney, but that a very marked and sudden *maximum* occurs at or near the elevation designated in the commencement of this letter: so marked indeed as almost to have the effect of a flash if the motion be quick, or a sudden blaze as if the wick-screw had been raised a turn. The flame contracts somewhat in diameter, lengthens, ceases to give off smoke, and attains a dazzling intensity.

With this great increase of light there is certainly not a correspondingly increased consumption of oil. At least the servant who trims my lamp reports that a lamp so fitted consumes very little if any more oil than one exactly similar on the common plan.

I have the honour to be, Sir,

Your obedient servant,

Slough, Feb. 15, 1840.

J. F. W. HERSCHEL.

XXXVI. *Observations on the Blood Corpuscles, or Red Particles, of the Mammiferous Animals.* By GEORGE GULLIVER, F.R.S., F.Z.S., Assistant Surgeon to the Royal Regiment of Horse Guards. No. III.*

AN account is now to be given of the blood corpuscles of several other mammalia which I have examined since the publication of my last papers; and similar communications will be continued occasionally, until the observations have been made as complete as possible, when, as already intimated, they will be presented in a systematic form, so as to

* Communicated by the Author.

exhibit a comprehensive view of the results, particularly as regards the size and figure of the blood particles in the different subdivisions of the mammiferous animals.

It is very desirable that the blood of the larger species of the cetaceous animals should be examined; for although the corpuscles of the Mouse (13.) are bigger than those of the Horse (34.), and there is generally no relation between the size of the animal and that of its blood particles, yet they are larger in the Elephant (51.), as far as we at present know, than in any other mammal. The corpuscles of the Goat were the smallest known to physiologists before my observation of the singularly minute blood disks of the Napu Musk Deer*.

In some instances the corpuscles are found to be a little larger in the dead than in the living animal, although they may subsequently become smaller, in consequence of the removal of their colouring matter by the serum. It will be perceived that many observations have been made on the blood after death; these have led me to ascertain that the particles are subject to modifications in size, and in some degree in shape, as compared with those of the living animal; and similar variations are often observable during life in disease. Besides the instances in which these facts are barely indicated in the preceding communications, it may be mentioned that I have seen the changes in the human blood particles. Thus in a man affected with dropsy, in connexion with granular degeneration of the kidney, some blood was drawn from a vein of the arm, and the corpuscles found to differ remarkably from those of the healthy subject. Though examined before the blood was perfectly cold, as well as after the lapse of a day, their size was singularly irregular, generally smaller than natural, having an average diameter of only 1-4400th of an inch. But as the morbid conditions of the blood corpuscles are probably more extensive and important than has been hitherto supposed, this is a novel and interesting subject for further and special inquiry; and it is merely alluded to at present as one of the many circumstances under which the size, form, and general appearance of the disks are liable to variations, which will doubtless attract the attention of pathologists now that the necessity of minute researches concerning the morbid as well as healthy fluids has been so fully recognised.

With regard to the blood corpuscles of the foetus as compared with those of the mother, I apprehend that I have de-

* See Dublin Med. Press, No. 27, 1839, and Annals of Nat. Hist., &c. Dec. 1839.

tected a source of error in some of the observations. The statement, therefore, formerly made (13.) is withdrawn for the present; and I hope soon to be able to give the result of another inquiry on the subject. In the mean time I may mention that in the foetal Guinea pig at the full period of utero gestation the corpuscles corresponded in size with those of the mother; and in a human foetus at the fifth or sixth month they were smaller than in the adult.

116. Mona Monkey, (*Cercopithecus Mona*), a male about a third grown. Most frequent sizes of corpuscles 1-3554th and 1-3428th. Extreme diameters, 1-5333rd and 1-2900th. Blood from the left coronary vein as well as from the different cavities of the heart. In the blood of the inferior cava vein the corpuscles were more variable in size, the extremes being 1-4800th and 1-2400th, with most numerous intermediate gradations.

117. Sooty Monkey, (*Cercopithecus fuliginosus*), a female about half-grown. All the following diameters very frequent, 1-3600th, 1-3428th, 1-3368th, and 1-3200th. Extreme sizes 1-5333rd and 1-3000th. Blood from the left ventricle of the heart.

118. Patas or Red Monkey, (*Cercopithecus ruber*), a female, nearly full-grown. The disks most commonly 1-3330th of an inch in diameter; extreme sizes, 1-4000th and 1-3000th. Blood from the pulmonary artery and vein, a few hours after death.

119. Crown Monkey, (*Cercopithecus pileatus*), a male about two-thirds grown. The following the most common sizes: 1-3635th, 1-3600th, and 1-3423rd. Extreme diameters, 1-4800th and 1-2900th. Blood from a prick of the forehead.

120. Vervet Monkey, (*Cercopithecus pygerythrus*), an adult male, 1-3309th, 1-3429th, and 1-3552nd common sizes; extreme diameters 1-4000th and 1-2900th of an inch. Blood from a wound at the end of the tail.

121. Dog-faced Baboon, (*Cynocephalus anubis*?) a female about half-grown. The dried corpuscles 1-3600th, 1-3530th and 1-3428th generally; extreme diameters 1-4000th, and 1-3000th. In the serum there were several disks exactly of the same magnitude, besides many of smaller size, viz. about 1-5333rd of an inch in diameter.

The blood was procured from a wound of the tail, and examined in less than two hours afterwards. The shrinking of some of the corpuscles in their own serum is well exemplified. In another dog-faced Baboon (1.) the corpuscles were a little larger; but as in that instance the blood was obtained from

the animal a day or two after death, the variation is within the limits that may occur in the same species.

122. Black-backed Papio, or Indian Ape, (*Papio melanotus*,) a male. Common diameter of corpuscles 1-3432nd of an inch; extreme sizes 1-4570th and 1-2666th. Blood from the axillary vein after death.

123. Wanderoo Monkey, (*Papio silenus*,) a full-grown male. Frequent sizes of corpuscles 1-3600th, 1-3552nd, and 1-3270th of an inch. Extreme diameters 1-4570th and 1-2666th. Blood from the left ventricle of the heart.

124. Chameck Spider Monkey, (*Ateles subpentadactylus*,) a female about two-thirds grown. The following diameters most frequent: 1-3790th, 1-3600th, and 1-3429th of an inch. Extreme sizes 1-4920th and 1-2900th. Blood from a prick of the fore hand.

125. Black Spider Monkey, (*Ateles ater*,) an adult male. All the following sizes very common: 1-3429th, 1-3528th, 1-3555th, 1-3600th, 1-3693rd and 1-3792nd. Extreme diameters 1-4555th and 1-3000th. Blood from a prick of the fore hand.

126. Weeper Monkey, (*Cebus apella*,) a female nearly full-grown. 1-3600th, 1-3554th, 1-3429th, and 1-3368th; most common sizes. Extreme diameters 1-4800th and 1-2666th of an inch. Blood from a prick of the fore hand.

The measurements slightly smaller than afforded by the corpuscles of *C. capucinus* (5.), but as the blood was procured from a dead specimen of the latter, the discrepancy is not greater than may be often observed under similar circumstances in one species.

127. Squirrel Monkey, (*Callithrix sciureus*,) a male about two-thirds grown. The following sizes all very frequent: 1-3790th, 1-3693rd, 1-3600th, and 1-3552nd. Extreme diameters, 1-4800th and 1-3200th. Blood from a prick of the nose.

The blood of the Toque Monkey (81.) was obtained from the heart of a dead specimen. The corpuscles, procured from a wound in the tail, of a healthy full-grown male afforded the following measurements, viz. dry, 1-3764th, 1-3600th, and 1-3552nd. In the serum many disks of the same magnitude were observed, besides a large quantity of smaller size, viz. from 1-6000th, to 1-5333rd of an inch, though the blood was carefully secured in a small glass tube and examined within an hour after it was taken from the animal.

128. The Mole, (*Talpa Europæa*,) recently killed, smallest disks about 1-5000th, the largest about 1-4000th of an inch. Blood from the heart.

For this observation I am indebted to Dr. Davy.

129. Grisly Bear, (*Ursus ferox*), a female about half-grown. Most common diameters 1-3340th and 1-3552nd of an inch. Extreme sizes 1-4570th and 1-3000th. Blood from a prick of the nose.

130. Badger, (*Meles vulgaris*), an old male. All the following sizes very common: 1-4128th, 1-4000th, 1-3973rd, 1-3810th, and 1-3693rd. Extreme diameters 1-5333rd and 1-3200th of an inch. There were besides some of the very small circular particles as in the genus *Sciurus*. Blood from the integuments of the thigh.

131. Common Jackal, (*Canis aureus*), an old male. The corpuscles, dried quickly, afforded the following measurements: 1-4000th, 1-3764th, and 1-3840th most frequently, the extreme sizes being 1-4800th and 1-3200th of an inch. In the serum, examined within two hours after they were obtained, 1-4800th was the most common diameter, and the extreme sizes 1-6000th and 1-3200th. Disks of the last, or largest size, as well as of 1-3555th, were not unfrequent, generally collecting together quickly into rouleaux, from which it was seen that the thickness of the edges of these corpuscles was 1-14,000th of an inch. The smaller particles, though much more abundant, did not arrange themselves together by their flat surfaces, and indeed could hardly be seen edge-wise.

The blood was obtained freely from a puncture in a vein of the hind leg; and the above facts are merely mentioned as exemplifying the variations which may often be observed in the corpuscles only a short time after the blood has been removed, with every care, from various mammals.

132. African Civet Cat, (*Viverra civetta*), adult male. 1-4615th, 1-4360th, 1-4000th, and 1-3552nd of an inch all frequently observed. Extreme sizes 1-6000th and 1-3200th. The corpuscles in this instance, therefore, very variable in magnitude. Blood from a prick of the tail.

In another adult male, after death, the corpuscles were also extremely variable in size, as the following were all common, viz. 1-5333rd, 1-4760th, 1-4500th, 1-4412th, 1-4365th and 1-4000th; the extreme measurements being 1-6000th and 1-3200th of an inch. Blood from the different cavities of the heart, from the coronary veins, and from the portal vein.

133. Javanese Ichneumon, (*Herpestes Javanicus*), a full-grown male. All the following sizes common: 1-4800th, 1-4924th, 1-5142nd, and 1-5120th. Extreme diameters 1-6000th and 1-4000th. Blood from a wound at the end of the tail.

134. Chetah or Hunting Leopard, (*Felis jubata*), an adult

female. All the following sizes common : 1-4365th, 1-4268th, 1-4173rd, and 1-4000th. Extreme sizes 1-5333rd and 1-3555th of an inch. Blood from a prick of the nose.

135. Alexandrian Rat, (*Mus Alexandrinus*, albino, var.,) an adult male. 1-4173rd, 1-4000th, 1-3810th, and 1-3764th, very common sizes. Small corpuscles 1-4800th; the large 1-3200th. Edges of disks 1-14,000th of an inch thick. Blood from a vein of the hind leg.

136. Coendu or Ring-tailed Porcupine, (*Synetheres prehensilis*,) a full-grown male. Common diameters 1-3428th, 1-3309th, and 1-3600th. Extreme sizes 1-4570th and 1-2460th of an inch. Blood from a cut at the end of the tail.

Error in the last paper (No. 2.) p. 108, l. 28, *for* Haller, *read* Harvey.

XXXVII. *Contributions to Electricity and Magnetism.*
No. III. *on Electro-magnetic Induction.* By JOSEPH HENRY, LL.D., Prof. of Natural Philosophy in the College of New Jersey, Princeton*.

INTRODUCTION.—SECTION I. *Conditions which influence the induction of a Current on itself.*—SECTION II. *Conditions which influence the production of Secondary Currents.*—SECTION III. *On the Induction of Secondary Currents at a distance.*—SECTION IV. *On the Effects produced by interposing different Substances between the Conductors.*—SECTION V. *On the Production and Properties of induced Currents of the Third, Fourth and Fifth Order.*—SECTION VI. *The Production of induced Currents of the different Orders from ordinary Electricity.*—NOTE *on the investigations of Professor Ettingshausen.*

1. SINCE my investigations in reference to the influence of a spiral conductor, in increasing the intensity of a galvanic current, were submitted to the Society, the valuable paper of Dr. Faraday, on the same subject, has been published, and also various modifications of the principle have been made by Sturgeon, Masson, Page, and others, to increase the effects. The spiral conductor has likewise been applied by Cav. Antinori to produce a spark by the action of a thermo-electrical pile: and Mr. Watkins has succeeded in exhibiting all the phænomena of hydro-electricity by the same means. Although the principle has been much extended by the re-

* From the Transactions of the American Philosophical Society, vol. vi, having been read Nov. 2, 1838.

searches of Dr. Faraday, yet I am happy to state that the results obtained by this distinguished philosopher are not at variance with those given in my paper.

2. I now offer to the Society a new series of investigations in the same line, which I hope may also be considered of sufficient importance to merit a place in the Transactions.

3. The primary object of these investigations was to discover, if possible, inductive actions in common electricity analogous to those found in galvanism. For this purpose a series of experiments was commenced in the spring of 1836, but I was at that time diverted, in part, from the immediate object of my research, by a new investigation of the phænomenon known in common electricity by the name of the lateral discharge. Circumstances prevented my doing anything further, in the way of experiment, until April last, when most of the results which I now offer to the Society were obtained. The investigations are not as complete, in several points, as I could wish, but as my duties will not permit me to resume the subject for some months to come, I therefore present them as they are; knowing, from the interest excited by this branch of science in every part of the world, that the errors which may exist will soon be detected, and the truths be further developed.

4. The experiments are given nearly in the order in which they were made; and in general they are accompanied by the reflections which led to the several steps of the investigation. The whole series is divided, for convenience of arrangement, into six sections, although the subject may be considered as consisting, principally, of two parts; the first relating to a new examination of the induction of galvanic currents; and the second to the discovery of analogous results in the discharge of ordinary electricity*.

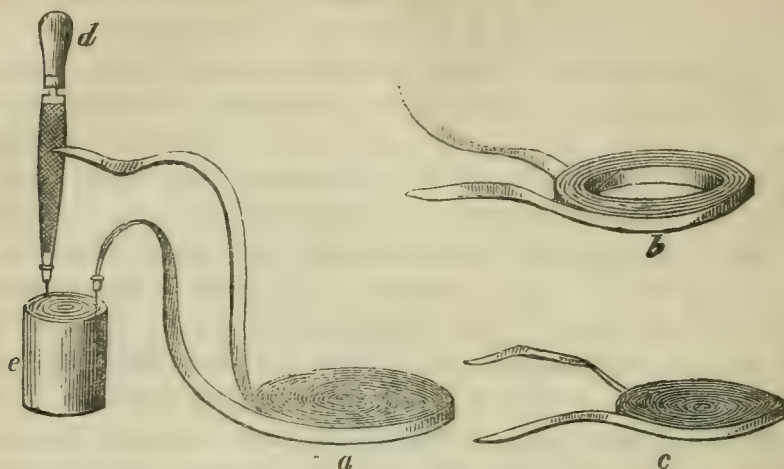
5. The principal articles of apparatus used in the experiments, consist of a number of flat coils of copper riband, which will be designated by the names of coil No. 1, coil No. 2, &c.; also of several coils of long wire; and these, to distinguish them from the ribands, will be called helix No. 1, helix No. 2, &c.

6. Coil No. 1 is formed of thirteen pounds of copper plate, one inch and a half wide and ninety-three feet long. It is well covered with two coatings of silk, and was generally used in the form represented in fig. 1, which is that of a flat spiral sixteen inches in diameter. It was however sometimes formed

* The several paragraphs are numbered in succession, from the first to the last, after the mode adopted by Mr. Faraday, for convenience of reference.

into a ring of larger diameter, as is shown in fig. 4, Section III.

Fig. 1.

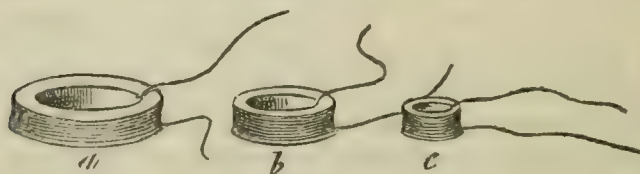


a represents coil No. 1, *b* coil No. 2, and *c* coil No. 3; *e* the battery, *d* the rasp.

7. Coil No. 2. is also formed of copper plate, of the same width and thickness as coil No. 1. It is, however, only sixty feet long. Its form is shown at *b*, fig. 1. The opening at the centre is sufficient to admit helix No. 1. Coils Nos, 3, 4, 5, 6, &c. are all about sixty feet long, and of copper plate of the same thickness, but of half the width of coil No. 1.

8. Helix No. 1. consists of sixteen hundred and sixty yards of copper wire, $\frac{1}{9}$ th of an inch in diameter; No. 2, of nine hundred and ninety yards; and No. 3, of three hundred and

Fig. 2.



a represents helix No. 1, *b* helix No. 2, *c* helix No. 3.

fifty yards, of the same wire. These helices are shown in fig. 2, and are so adjusted in size as to fit into each other; thus forming one long helix of three thousand yards: or, by using them separately, and in different combinations, seven helices of different lengths. The wire is covered with cotton thread, saturated with bees-wax, and between each stratum of spires a coating of silk is interposed.

9. Helix No. 4 is shown at *a*, fig. 4, Section III.; it is

formed of five hundred and forty-six yards of wire, $\frac{1}{19}$ th of an inch in diameter, the several spires of which are insulated by a coating of cement. Helix No. 5 consists of fifteen hundred yards of silvered copper wire $\frac{1}{25}$ th of an inch in diameter, covered with cotton, and is of the form of No. 4.

10. Besides these I was favoured with the loan of a large spool of copper wire, covered with cotton, $\frac{1}{6}$ th of an inch in diameter, and five miles long. It is wound on a small axis of iron, and forms a solid cylinder of wire, eighteen inches long, and thirteen in diameter.

11. For determining the direction of induced currents, a magnetizing spiral was generally used, which consists of about thirty spires of copper wire, in the form of a cylinder, and so small as just to admit a sewing needle into the axis.

12. Also a small horseshoe is frequently referred to, which is formed of a piece of soft iron, about three inches long, and $\frac{2}{5}$ ths of an inch thick; each leg is surrounded with about five feet of copper bell wire. This length is so small, that only a current of electricity of considerable quantity can develop the magnetism of the iron. The instrument is used for indicating the existence of such a current.

13. The battery used in most of the experiments is shown in fig. 1. It is formed of three concentric cylinders of copper, and two interposed cylinders of zinc. It is about eight inches high, five inches in diameter, and exposes about one square foot and three quarters of zinc surface, estimating both sides of the metal. In some of the experiments a larger battery was used, weakly charged; but all the results mentioned in the paper, except those with a Cruickshanks trough, can be obtained with one or two batteries of the above size, particularly if excited by a strong solution. The manner of interrupting the circuit of the conductor by means of a rasp, *b*, is shown in the same figure.

SECTION I.—*Conditions which influence the induction of a Current on itself.*

14. The phænomenon of the spiral conductor is at present known by the name of the induction of a current on itself, to distinguish it from the induction of the secondary current, discovered by Dr. Faraday. The two, however, belong to the same class, and experiments render it probable that the spark given by the long conductor is, from the natural electricity of the metal, disturbed for an instant by the induction of the primary current. Before proceeding to the other parts of these investigations, it is important to state the results of a number of preliminary experiments, made to determine

more definitely the conditions which influence the action of the spiral conductor.

15. When the electricity is of low intensity, as in the case of the thermo-electrical pile, or a large single battery weakly excited with dilute acid, the flat riband coil No. 1, ninety-three feet long, is found to give the most brilliant deflagrations, and the loudest snaps from a surface of mercury. The shocks, with this arrangement, are, however, very feeble, and can only be felt in the fingers or through the tongue.

16. The induced current in a short coil, which thus produces deflagration, but not shocks, may, for distinction, be called one of quantity.

17. When the length of the coil is increased, the battery continuing the same, the deflagrating power decreases, while the intensity of the shock continually increases. With five riband coils, making an aggregate length of three hundred feet, and the small battery, fig. 1, the deflagration is less than with coil No. 1, but the shocks are more intense.

18. There is, however, a limit to this increase of intensity of the shock, and this takes place when the increased resistance or diminished conduction of the lengthened coil begins to counteract the influence of the increasing length of the current. The following experiment illustrates this fact. A coil of copper wire $\frac{1}{16}$ th of an inch in diameter, was increased in length by successive additions of about thirty-two feet at a time. After the first two lengths, or sixty-four feet, the brilliancy of the spark began to decline, but the shocks constantly increased in intensity, until a length of five hundred and seventy-five feet was obtained, when the shocks also began to decline. This was then the proper length to produce the maximum effect with a single battery, and a wire of the above diameter.

19. When the intensity of the electricity of the battery is increased, the action of the short riband coil decreases. With a Cruickshanks trough of sixty plates, four inches square, scarcely any peculiar effect can be observed, when the coil forms a part of the circuit. If however the length of the coil be increased in proportion to the intensity of the current, then the inductive influence becomes apparent. When the current, from ten plates of the above-mentioned trough, was passed through the wire of the large spool (10.), the induced shock was too severe to be taken through the body. Again, when a small trough of twenty-five one-inch plates, which alone would give but a very feeble shock, was used with helix No. 1, an intense shock was received from the induction when the contact was broken. Also a slight shock in this

arrangement is given when the contact is formed, but it is very feeble in comparison with the other. The spark, however, with the long wire and compound battery is not as brilliant as with the single battery and the short riband coil.

20. When the shock is produced from a long wire, as in the last experiments, the size of the plates of the battery may be very much reduced, without a corresponding reduction of the intensity of the shock. This is shown in an experiment with the large spool of wire (10.). A very small compound battery was formed of six pieces of copper bell wire, about one inch and a half long, and an equal number of pieces of zinc of the same size. When the current from this was passed through the five miles of the wire of the spool, the induced shock was given at once to twenty-six persons joining hands. This astonishing effect placed the action of a coil in a striking point of view.

21. With the same spool and the single battery used in the former experiments, no shock, or at most a very feeble one, could be obtained. A current, however, was found to pass through the whole length, by its action on the galvanometer; but it was not sufficiently powerful to induce a current which could counteract the resistance of so long a wire.

22. The induced current in these experiments may be considered as one of *considerable intensity*, and *small quantity*.

23. The form of the coil has considerable influence on the intensity of the action. In the experiments of Dr. Faraday, a long cylindrical coil of thick copper wire, inclosing a rod of soft iron, was used. This form produces the greatest effect when magnetic reaction is employed; but in the case of simple galvanic induction, I have found the form of the coils and helices represented in the figures most effectual. The several spires are more nearly approximated, and therefore they exert a greater mutual influence. In some cases, as will be seen hereafter, the ring form, shown in fig. 4, is most effectual.

24. In all cases the several spires of the coil should be well insulated; for although in magnetizing soft iron, and in analogous experiments, the touching of two spires is not attended with any great reduction of action, yet in the case of the induced current, as will be shown in the progress of these investigations, a single contact of two spires is sometimes sufficient to neutralize the whole effect.

25. It must be recollected that all the experiments with these coils and helices, unless otherwise mentioned, are made without the reaction of iron temporarily magnetized; since the introduction of this would, in some cases, interfere with the action, and render the results more complex.

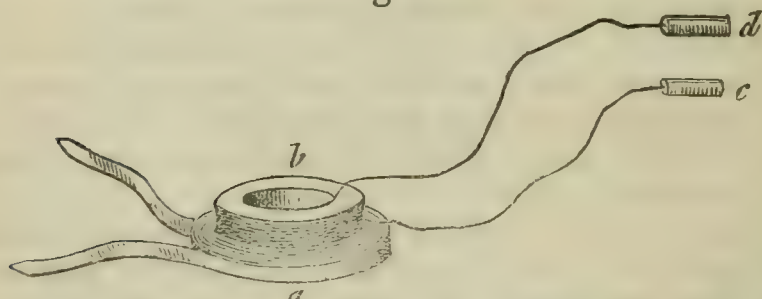
SECTION II.—*Conditions which influence the production of Secondary Currents.*

26. The secondary currents, as it is well known, were discovered in the induction of magnetism and electricity, by Dr. Faraday, in 1831. But he was at that time urged to the exploration of new, and apparently richer veins of science, and left this branch to be traced by others. Since then, however, attention has been almost exclusively directed to one part of the subject, namely, the induction from magnetism, and the perfection of the magneto-electrical machine. And I know of no attempts, except my own, to review and extend the purely electrical part of Dr. Faraday's admirable discovery.

27. The energetic action of the flat coil, in producing the induction of a current on itself, led me to conclude that it would also be the most proper means for the exhibition and study of the phænomena of the secondary galvanic currents.

28. For this purpose coil No. 1 was arranged to receive the current from the small battery, and coil No. 2 placed on this, with a plate of glass interposed to ensure perfect insulation; as often as the circuit of No. 1 was interrupted, a

Fig. 3.



a represents coil No. 1, *b* helix No. 1, and *c, d*, handles for receiving the shock.

powerful secondary current was induced in No. 2. The arrangement is the same as that exhibited in fig. 3, with the exception that in this the compound helix is represented as receiving the induction, instead of coil No. 2.

29. When the ends of the second coil were rubbed together, a spark was produced at the opening. When the same ends were joined by the magnetizing spiral (11.), the enclosed needle became strongly magnetic. Also when the secondary current was passed through the wires of the iron horseshoe (12.), magnetism was developed; and when the ends of the second coil were attached to a small decomposing apparatus, of the kind which accompanies the magneto-electrical machine, a stream of gas was given off at each pole. The shock, however, from this coil is very feeble, and can scarcely be felt above the fingers.

30. This current has therefore the properties of one of moderate intensity, but considerable quantity.

31. Coil No. 1 remaining as before, a longer coil, formed by uniting Nos. 3, 4 and 5, was substituted for No. 2. With this arrangement, the spark produced when the ends were rubbed together, was not as brilliant as before; the magnetizing power was much less; decomposition was nearly the same, but the shocks were more powerful, or, in other words, the intensity of the induced current was increased by an increase of the length of the coil, while the quantity was apparently decreased.

32. A compound helix, formed by uniting Nos. 1 and 2, and therefore containing two thousand six hundred and fifty yards of wire, was next placed on coil No. 1. The weight of this helix happened to be precisely the same as that of coil No. 2. and hence the different effects of the same quantity of metal in the two forms of a long and short conductor, could be compared. With this arrangement the magnetizing effects, with the apparatus before mentioned, disappeared. The sparks were much smaller, and also the decomposition less, than with the short coil; but the shock was almost too intense to be received with impunity, except through the fingers of one hand. A circuit of fifty-six of the students of the senior class, received it at once from a single rupture of the battery current, as if from the discharge of a Leyden jar weakly charged. The secondary current in this case was one of small quantity, but of great intensity.

33. The following experiment is important in establishing the fact of a limit to the increase of the intensity of the shock, as well as the power of decomposition, with a wire of a given diameter. Helix No. 5, which consists of wire only $\frac{1}{125}$ th of an inch in diameter, was placed on coil No. 2, and its length increased to about seven hundred yards. With this extent of wire, neither decomposition nor magnetism could be obtained, but shocks were given of a peculiarly pungent nature; they did not however produce much muscular action. The wire of the helix was further increased to about fifteen hundred yards; the shock was now found to be scarcely perceptible in the fingers.

34. As a counterpart to the last experiment, coil No. 1 was formed into a ring of sufficient internal diameter to admit the great spool of wire (11.), and with the whole length of this (which, as has before been stated, is five miles) the shock was found so intense as to be felt at the shoulder, when passed only through the fore-finger and thumb. Sparks and decomposition were also produced, and needles rendered mag-

netic. The wire of this spool is $\frac{1}{16}$ th of an inch thick, and we therefore see from this experiment, that by increasing the diameter of the wire, its length may also be much increased, with an increased effect.

35. The fact (33.) that the induced current is diminished by a further increase of the wire, after a certain length has been attained, is important in the construction of the magneto-electrical machine, since the same effect is produced in the induction of magnetism. Dr. Goddard of Philadelphia, to whom I am indebted for coil No. 5, found that when its whole length was wound on the iron of a temporary magnet, no shocks could be obtained. The wire of the machine may therefore be of such a length, relative to its diameter, as to produce shocks, but no decomposition; and if the length be still further increased, the power of giving shocks may also become neutralized.

36. The inductive action of coil No. 1, in the foregoing experiments, is precisely the same as that of a temporary magnet in the case of the magneto-electrical machine. A short thick wire around the armature gives brilliant deflagrations, but a long one produces shocks. This fact, I believe, was first discovered by my friend Mr. Saxton, and afterwards investigated by Sturgeon and Lenz.

37. We might, at first sight, conclude, from the perfect similarity of these effects, that the currents which, according to the theory of Ampere, exist in the magnet, are, like those in the short coil, of great quantity and feeble intensity; but succeeding experiments will show that this is not necessarily the case.

38. All the experiments given in this section have thus far been made with a battery of a single element. This condition was now changed, and a Cruickshanks trough of sixty pairs substituted. When the current from this was passed through the riband coil No. 1, no indication, or a very feeble one, was given of a secondary current in any of the coils or helices, arranged as in the preceding experiments. The length of the coil, in this case, was not commensurate with the intensity of the current from the battery. But when the long helix, No. 1, was placed instead of coil No. 1, a powerful inductive action was produced on each of the articles, as before.

39. First, helices No. 2 and 3 were united into one, and placed within helix No. 1, which still conducted the battery current. With this disposition a secondary current was produced, which gave intense shocks but feeble decomposition, and no magnetism in the soft iron horseshoe. It was there-

fore one of intensity, and was induced by a battery current also of intensity.

40. Instead of the helix used in the last experiment for receiving the induction, one of the coils (No. 3) was now placed on helix No. 1, the battery remaining as before. With this arrangement the induced current gave no shocks, but it magnetized the small horseshoe; and when the ends of the coil were rubbed together, produced bright sparks. It had therefore the properties of a current of quantity; and it was produced by the induction of a current, from the battery, of intensity.

41. This experiment was considered of so much importance, that it was varied and repeated many times, but always with the same result; it therefore establishes the fact *that an intensity current can induce one of quantity*, and by the preceding experiments, the converse has also been shown, that *a quantity current can induce one of intensity*.

42. This fact appears to have an important bearing on the law of the inductive action, and would seem to favour the supposition that the lower coil, in the two experiments with the long and short secondary conductors, exerted the same amount of inductive force, and that in one case this was expended (to use the language of theory) in giving a great velocity to a small quantity of the fluid, and in the other in producing a slower motion in a larger current; but in the two cases, were it not for the increased resistance to conduction in the longer wire, the quantity multiplied by the velocity would be the same. This, however, is as yet a hypothesis, but it enables us to conceive how intensity and quantity may both be produced from the same induction.

43. From some of the foregoing experiments we may conclude, that the quantity of electricity in motion in the helix is really less than in the coil, of the same weight of metal; but this may possibly be owing simply to the greater resistance offered by the longer wire. It would also appear, if the above reasoning be correct, that to produce the most energetic physiological effects, only a small quantity of electricity, moving with great velocity, is necessary.

44. In this and the preceding section, I have attempted to give only the general conditions which influence the galvanic induction. To establish the law would require a great number of more refined experiments, and the consideration of several circumstances which would affect the results, such as the conduction of the wires, the constant state of the battery, the method of breaking the circuit with perfect regularity,

and also more perfect means than we now possess of measuring the amount of the inductive action; all these circumstances render the problem very complex.

[To be continued.]

XXXVIII. *Researches of Mons. R. PIRIA on the Combinations of Salicyle*.*

EVERY one who is at all acquainted with the gigantic strides made in organic chemistry since the discovery of the real nature of the oil of bitter almonds, and the development of the remarkable combinations of benzoyle, must have hailed with peculiar pleasure the discovery of an analogous series of compounds having for their base a compound radical termed spiroil, from its being present in the oil of the *Spiræa Ulmaria*, or meadow-sweet. This body was discovered by Löwig, who ascertained the volatile oil of the *Spiræa* to be really an hydracid, consisting of a compound radical analogous to benzoyl, combined with hydrogen. The researches of Löwig have been already presented to the English reader in the pages of the valuable Scientific Memoirs of Mr. R. Taylor. We have now the pleasure of laying before our scientific readers an account of a valuable series of researches of M. Piria on a new compound organic base, bearing considerable resemblance to benzoyl and spiroil, and promising, from this very resemblance, to throw much light on the nature of the respective combinations of these curious bases.

The active principle of the bark of different species of salix has been long known to chemists, and salicin is now an ordinary article of commerce, being employed in medicine as a substitute for quinine, as a remedy in intermittent fever.

Salicine was first obtained in a white crystallizable state, by M. Leroux, and has been submitted to ultimate analysis by MM. Jules Gay-Lussac and Pelouse. Piria has also analysed it, and its per centage composition was in three experiments found to be as follows:—

	Exp. 1.	Exp. 2.	Exp. 3.
Carbon.....	55.68	55.04	55.54
Hydrogen	6.36	6.39	6.43
Oxygen	37.96	38.57	38.03
	<hr/>	<hr/>	<hr/>
	100	100	100

* For this account of M. Piria's researches, the Editors are obliged to Dr. Golding Bird.

Berzelius suggests the probability of the atom of salicine consisting of (2 C, 2 H, O). To ascertain this fact by experiment, Piria dissolved salicine in warm water, added a few drops of ammonia, and then dropped in a solution of the tribasic acetate of lead: a white flocculent precipitate was obtained. This was collected, dried, and submitted to analysis, the result of which proved the *crystallized* salicine to consist of

		Per cent.
21 atoms carbon.....	= 126	= 55·76
14 ——— hydrogen ..	14	6·06
11 ——— oxygen	88	38·18
	<hr/>	<hr/>
Weight of atom ...	228	100

The *anhydrous* salicine contains two atoms less of water, and consists of

		Per cent.
21 atoms carbon.....	= 126	= 60·49
12 ——— hydrogen ..	12	5·63
9 ——— oxygen	72	33·88
	<hr/>	<hr/>
Weight of atom	210	100

Döbereiner has long ago shown that salicine, when distilled with oxidating bodies, as a mixture of sulphuric acid and peroxide of manganese, yielded a larger proportion of formic acid than any other known substance. By distilling it, however, with other oxidating agents, taking care to avoid an excess of acid, Piria obtained a distinct product, which consisted of an organic base combined with hydrogen, which, in conformity with the adopted nomenclature, he has named *hydruret of salicycle*: like benzoyl and spiroyl, salicycle has not yet been obtained in a free state. It bears so close an analogy to the former, that they may be considered as having one common radical, as may be seen by comparing their elementary composition.

	Carbon.	Hydrogen.	Oxygen.
Benzoyl	= 14 atoms	5 atoms	2 atoms
Salicycle	14	5	4

and thus benzoyle and salicycle may be respectively considered as two oxyds of an hydrocarbon, consisting of (14 C + 5 H).

Dumas has, as is well known, suggested that benzoyle and benzoic acid may be considered as two oxides of this hypothetical hydro-carbon or benzogene, in which case salicycle and salicic acid may be considered as two other members of the same group, thus—

Radical (14 C + 5 H). Oxygen.

1	2 =	benzoyl.
1	3	anhydrous benzoic acid.
1	4	salicyle.
1	5	anhydrous salicic acid.

Hydruret of Salicyle.

This substance when pure is colourless, but when impure is deep red, possessing an agreeable and aromatic odour, to a certain extent resembling that of the oil of bitter almonds: by distillation it is rendered completely colourless, by exposure to air it again acquires a reddish hue. Digested with water a small quantity is dissolved, sufficient however to cause the latter fluid to acquire its odour; its taste is very pungent and acrid, like that of most essential oils. It is without action on litmus paper. Placed in contact with the salts of the sesqui-oxide of iron it assumes a splendid violet colour, which by exposure to air becomes yellow. The salts of the protoxide of iron do not exert any manifest action upon it. In æther and alcohol it is soluble in all proportions: water precipitates it from its alcoholic solutions. Its specific gravity at 56° Fahr. is 1.1731: it boils at a temperature of about 400°. Mixed with alkaline carbonates it slowly decomposes them, causing the evolution of their acid; upon the application of heat this action becomes much more energetic. Caustic alkalies combine with it, producing a considerable disengagement of heat. Chlorine and bromine combine with the base of the hydruret, producing chloride and bromide of salicyle, disengaging hydrochloric and hydrobromic acids; iodine dissolves in the hydruret without suffering any obvious change.

Digested in nitric acid, hydruret of salicyle is converted into a substance termed *nitro-salicide*, which becomes, by continuing the digestion, changed into carbazatic acid.

The hydruret of salicyle may be readily prepared by the following process. Dissolve four parts of bichromate of potass in a sufficient quantity of water, and add three parts of strong sulphuric acid. Then dissolve some salicine in six parts of hot water, place the solution in a retort, and raise it to the boiling temperature. Adapt a carefully cooled receiver, and through the tubulure of the retort add, by small quantities at a time, the acid solution of bichromate of potass to the hot fluid; violent action ensues; the mixture turns green from the formation of green sulphate of chromium, and a milky fluid distils over. The products of this distillation by repose deposit salicyle in the form of oily drops. These

drops should be collected, and purified by being distilled with chloride of calcium. It is important that no more acid be used than is here prescribed, otherwise a quantity of formic acid is produced, and a corresponding diminution in the quantity of hydruret of salicyle ensues.

The hydruret consists of

	Carbon.	Hydrogen.	Oxygen.
Salicyle	= 14	5	4
Hydrogen ...		1	
	—	—	—
Atomic weight ..	14	6	4

Hence it appears that hydruret of salicyle is isomeric with hydrated benzoic acid; and it is not a little remarkable, that the density of its vapour is identical with that of the latter acid, as determined by Dumas, being 4.276. It therefore consists by volume of

7 volumes vapour of carbon
3 ——— hydrogen
1 ——— oxygen

The hydruret of salicyle may be considered as a true hydracid, with a compound base like hydrocyanic acid; like that compound it combines with metallic oxides, its hydrogen forming water with the oxygen of the oxyd, and a *salicide* of the metal results. The metallic salicides are isomeric with the corresponding anhydrous benzoates of the oxides, thus placing M for an atom of a metal, and M + O for one of a metallic oxide; a given *benzoate* and *salicide* will consist of

	Carbon.	Hydrogen.	Oxygen	
Benzoate =	(14	+ 5	+ 3)	+ (M + O)
Salicide =	(14	+ 5	+ 4)	+ M

So that the hydruret of salicyle bears the same relation to hydrated benzoic acid as oxalic acid, according to the view of Dulong ($2\text{C}, 4\text{O} + \text{H}$) does to the generally received view of its composition at the present day, or ($2\text{C}, 3'\text{O} + \text{H O}$).

Metallic Salicides.

The salicide of potassium may be very readily prepared, by mixing the hydruret of salicyle with a strong solution of pure potass. On shaking the mixture, a considerable quantity of salicide of potassium separates in small yellow crystals from the supernatant alkaline fluid. These crystals must be collected, freed from adhering moisture by pressure between folds of bibulous paper, and dissolved in alcohol; by crystallization numerous well-defined square tables of the salicide are obtained. This salt is readily soluble in water and al-

cohol. When quite dry it undergoes no change by exposure to the air, but when wet it absorbs oxygen, becomes covered with numerous green spots, which ultimately turn black; the whole mass becoming eventually a black sooty powder, which will be alluded to in the course of these remarks on account of a peculiar acid which it contains.

A solution of salicide of potassium is precipitated yellow by salts of lead, silver, mercury, manganese, and barium. Salicide of ammonium is procured by mixing liquid ammonia with the hydruret of salicyle, in the same manner as the corresponding salt of potassium. It crystallizes in yellow needles, and by exposure to the air is soon decomposed into its constituents, ammonia being copiously evolved.

Salicide of barium is obtained by precipitating a solution of salicide of potassium by chloride of barium. It may be obtained in minute needles, but is very slightly soluble in water. It consists of

	Atoms.		Theory.	Exp.
Carbon	14 = 84		40.93	41.15
Hydrogen ...	7 7		3.34	3.41
Oxygen	6 48		22.96	22.57
Barium	1 68		32.77	32.87
<hr/>			<hr/>	
Atomic weight...	207		100	100

it hence contains two atoms of water, and when dried in Liebig's desiccating apparatus, it gives up this quantity, 1.237 parts losing 0.110 parts of its weight.

Salicide of copper may be prepared by dissolving freshly precipitated oxide of copper in hydruret of salicyle, and evaporating to dryness over a water bath; it forms a light yellow powder, which, when heated, partly sublimes in iridescent scales. Submitted to analysis, it was found to consist of

	Atoms.		Theory.	Exp.
Carbon	14 = 84		55.50	55.75
Hydrogen ...	5 5		3.24	3.47
Oxygen	4 32		20.74	20.70
Copper	1 32		20.52	20.08
<hr/>			<hr/>	
Atomic weight...	153		100	100

Salicic Acid.

This compound may be obtained by heating hydruret of salicyle with an excess of potass. The mixture becomes deep reddish-brown, and hydrogen is evolved. Dissolve the resulting mass in water, and add hydrochloric acid in excess; a copious deposit of salicic acid in fine needles occurs. This

acid closely resembles the benzoic; it is scarcely soluble in cold water, readily soluble in hot water, as well as in alcohol and æther. By heat it sublimes with great facility. Mixed with alkaline carbonates, salicylic acid decomposes them, driving off carbonic acid, and forming a series of salts. Digested with sulphuric acid, the new acid undergoes no obvious change until the mixture is heated, and then the whole turns black. Nitric acid does not affect salicylic acid in the cold; on applying heat, however, violent and tumultuous action ensues, dense red fumes are evolved, and a yellow solution is obtained; this, by repose, deposits minute yellow bitter crystals, which appear to be identical with the nitro-salicylic obtained by the action of nitric acid on hydruret of salicycle.

Salicylic acid contains an atom of combined water, which it loses when it combines with bases; hence the crystallized acid is a salicylate of water, or (14 C, 5 H, 5 O, + H O). Submitted to ultimate analysis, this consists of

	Atoms.		Theory.	Exp.
Carbon	14 = 84		61·32	61·10
Hydrogen ...	6 6		4·29	4·41
Oxygen	6 48		34·39	34·43
<hr/>			<hr/>	<hr/>
Atomic weight...	138		100	100

Salicylate of silver was prepared by precipitating a solution of salicylate of ammonia by nitrate of silver; this salt is a white insoluble powder, and consists of

	Atoms.		Theory.	Exp.
Carbon	14 = 84		34·70	34·91
Hydrogen ...	5 5		2·02	2·09
Oxygen	5 40		16·22	16·43
Oxyd of silver	1 116		47·06	46·57
<hr/>			<hr/>	<hr/>
Atomic weight...	245		100	100

Chloride of Salicycle.

This compound may be obtained by a process precisely similar to that used for the preparation of the chloride of benzoyl, by transmitting a current of dry chlorine gas through pure hydruret of salicycle; much heat is evolved, the chlorine unites with the hydrogen of the hydruret to form hydrochloric acid, which is copiously given off in a gaseous state, whilst the salicycle itself unites with more chlorine to form the chloride. A nearly solid crystalline mass is obtained, which should be purified by solution in alcohol and subsequent crystallization. The chloride of salicycle thus obtained is insoluble in water and acids; but in æther, alcohol, and alkaline

fluids, as well as ammonia, it readily dissolves, in the latter case undergoing some remarkable changes. From its solutions in the fixed alkalies, acids throw it down unchanged. Submitted to ultimate analysis, chloride of salicyle is found to consist of

	Atoms.		
Carbon ...	14	=	84
Hydrogen.	5		5
Oxygen ...	4		32
Chlorine...	1		36

54.18

3.16

20.25

22.41

Atomic weight... 157

100

so that the atom of hydrogen in the hydruret of salicyle becomes replaced by an equivalent of chlorine, as in the analogous compounds of benzoyl and spiroil.

Bromide of Salicyle.

This compound is produced whenever bromine is added to hydruret of salicyle, heat is evolved, and the whole consolidates into a crystalline mass, which, like the chloride, may be purified by solution in alcohol and subsequent crystallization. In this substance the hydrogen of the hydruret is replaced by an equivalent of bromine, the whole consisting of

	Atoms.		
Carbon ...	14	=	84
Hydrogen.	5		5
Oxygen...	4		32
Bromine ..	1		78

42.62

2.48

15.94

38.96

Atomic weight... 199

100

Combinations of Salicyle with Amidogene.

When a current of gaseous ammonia is passed over chloride of benzoyl, it has been shown by Woehler and Liebig to suffer decomposition; a mixture of chloride of ammonium and benzamide being produced, the latter compound being a combination of benzoyl with amidogene ($N + 2H$), analogous to oxamide.

When a current of dry ammonia is passed over chloride of salicyle it becomes absorbed, and a yellow pasty mass results, which must be frequently broken up and again exposed to the gas to effect its entire decomposition. No hydrochlorate of ammonia is formed, the ammonia removing oxygen from the compound without abstracting any chlorine, and forming water, which condenses in the tube in which the experiment is performed. The resulting compound is *Chlorosamide*; it

is a yellow crystalline body, soluble in alcohol and æther, nearly insoluble in water; the addition of weak alkalies causes the disengagement of ammonia; by heating it in an acid fluid it is decomposed, a salt of ammonia being formed, and chloride of salicyle being set free. *Chlorosamide* consists of

	Atoms.		
Carbon ...	14	=	84
Hydrogen.	5		5
Oxygen...	2		16
Nitrogen..	$\frac{4}{3}$		18.4
Chlorine..	1		36
			<hr/>
Atomic weight...	159.4		100

When gaseous ammonia is made to act on bromide of benzoyl, a compound called *bromosamide* results; its composition is identical with chlorosamide, with the substitution of the chlorine by its equivalent of bromine.

Action of the air on Salicide of Potassium.

When salicide of potassium in a perfectly dry state is exposed to the air, or to an atmosphere of oxygen gas, it undergoes, as has been already observed, no obvious change; but if this salt be previously moistened, it, by a similar exposure, alters in colour, its surface becomes covered with green specks, and it darkens in hue until the whole mass becomes completely black. If the moist salicide of potassium be placed in a receiver of oxygen gas inverted over mercury, the latter will be observed to rise rapidly in the glass, from the absorption of its gaseous contents. When the black, changed, salt is digested in water, a certain portion dissolves, and a soot-like powder is left; this should be collected, washed, and dried. This black compound dissolves readily in alcohol and æther, but is insoluble in water; it dissolves in alkaline solutions, forming saline compounds, from which the black powder is thrown down unchanged by acids. This substance appears to possess properties of a decidedly electro-negative character, and from this circumstance, as well as from its colour, it has been termed *melanic acid*, a name unhappily chosen, as it has already been applied to an ingredient in certain morbid animal secretions.

Melanic acid decomposes alkaline carbonates, forming saline combinations, attended with the evolution of carbonic acid gas. When heated this acid burns slowly, but without flame, and leaves no fixed residue.

Melanate of silver was prepared by precipitating a solution of melanate of ammonia by nitrate of silver; the precipitate

was dried and submitted to ultimate analysis, and was found to consist of

	Atoms.		Theory.	Exp.
Carbon	10 = 60.0		27.63	27.67
Hydrogen	4 4.9		1.71	1.95
Oxygen	5 40		18.18	18.82
Oxyd of silver	1 116		52.48	51.56
<hr/>			<hr/>	
Atomic weight . . .	220		100	100

hence the acid itself consists of

	Atoms.		
Carbon ...	10	=	60
			58.16
Hydrogen.	4		4
			3.80
Oxygen...	5		40
			38.04
<hr/>			
Atomic weight..	104		100

The aqueous solution obtained by washing the decomposed salicide of potassium, during the separation of melanic acid, was submitted to examination, and after evaporating it to a small bulk, mixing it with sulphuric acid, and submitting it to distillation, acetic acid was obtained, whilst sulphate of potass was left in the retort.

From these experiments, it appears, that salicide of potassium, when moistened and exposed to the air, or to an atmosphere of oxygen gas, is resolved into melanic acid and acetate of potass; and for every atom of salicide of potassium decomposed, an equivalent of acetate of potassa is produced. For this decomposition to be understood, we must admit that three atoms of oxygen and the elements of two atoms of water, are appropriated by each equivalent of the salicide of potassium, thus

	Carb.	Hyd.	Oxy.	Potassium.
Melanic acid	= 10	+ 4	+ 5	+ 0
Acetic acid	= 4	+ 3	+ 3	+ 0
<hr/>				
	14	+ 7	+ 8	+ 0
Minus { 2 atoms water	=	2	+ 2	+ 0
<hr/>				
	14	+ 5	+ 6	+ 0
3 atoms oxygen	=		3	+ 0
<hr/>				
	14	+ 5	+ 3	+ 0
1 atom potassa	=		1	+ 1
<hr/>				
1 atom of salicide potassium	=	14	+ 5	+ 4 + 1

Decomposition of Salicin by Sulphuric Acid.

When salicin is immersed in strong sulphuric acid it assumes a blood-red tint; but when digested at a boiling temperature in the acid previously moderately diluted with water, the salicin dissolves, forming a colourless solution. If a solution thus prepared, be poured into cold water, a white precipitate falls down of a resinoid character. This substance is turned red by sulphuric acid; like the unchanged salicin, it readily dissolves in alkaline fluids. Submitted to ultimate analysis, this new substance, which from its resinoid characters is termed *saliretin*, was found to consist of

Carbon	72·96
Hydrogen ..	5·83
Oxygen	21·21

100

No gas is disengaged during the formation of saliretin; indeed the action of the sulphuric acid appears to be purely catalytic, as in the cases of ætherification, and the formation of starch-sugar. On examining the acid fluid from which saliretin has been precipitated, it was found to contain grape-sugar; so that under the influence of the catalytic action of sulphuric acid, salicin is resolved into saliretin and grape-sugar, from the re-arrangement of its elements; affording another example of the resolution of organic products into new arrangements, under the influence of *catalysis*, or action of presence.

Action of Chlorine on Salicin.

When a current of gaseous chlorine is transmitted through a quantity of salicin diffused in water, solution ensues, and a yellow fluid is formed; and if a sufficient quantity of salicin be present, a yellow crystalline mass is deposited.

These crystals are but slightly soluble in water or alkaline fluids; they possess a very disagreeable odour and pungent taste; submitted to analysis, they were found to consist of

	Atoms.		
Carbon	21	=	126
Hydrogen ..	12		12
Chlorine . . .	2		72
Oxygen	11		88

Atomic weight . . . 298 100

so that, by this process, salicin loses two atoms of hydrogen, and gains two atoms of chlorine; a fact quite conformant with the doctrine of substitution. If the fluid through which the

chlorine is passed be kept at a boiling heat, no crystals are formed, but a reddish oily fluid is produced; this, when analysed, was found to consist of

	Atoms.		
Carbon	21	=	126
Hydrogen . .	$8\frac{1}{2}$		8.5
Chlorine . . .	$3\frac{1}{2}$		126
Oxygen	9		88
			<hr/>
			348.5
			<hr/>
			100

In this case, the *anhydrous* salicine loses $3\frac{1}{2}$ atoms of hydrogen, and gains an equivalent proportion of chlorine. This new compound is soluble in alcohol and alkaline solutions.

G. B.

XXXIX. *Letter to Richard Taylor, Esq., as Editor of the Philosophical Magazine and Journal.* By R. POTTER, Esq., B.A., F.C.P.S.

DEAR SIR,

THE part you have taken in the controversy between Professor Forbes and myself has a good deal surprised me. That you should deprecate "personal imputations" in controversial papers, is what every reader of your periodical must heartily approve. However searching a review of Professor Forbes's "memorandum" was required for the defence of my own investigations, I congratulate myself on having avoided personalities or any imputation of unworthy motives. It is a philosophical and legitimate line of defence to inquire whether your opponent is a competent judge of the matter in controversy, from the consideration he has given to the subject, and whether he is to be considered in the light of an impartial and unbiassed inquirer, or otherwise whether he has imbibed theoretical views so deeply as to place him only in the situation of a partisan of the particular theory adopted by him. This course I have always endeavoured to pursue, and would cheerfully concede to an opponent. I think, however, that I have just reason to complain of the admission of such terms as "gratuitously misinterpreted," and "take a pleasure in misinterpreting my expressions," admitted in the very communication to which your note was appended.

In respect to the unsupported quotation from your unnamed correspondent's letter, which you have adopted, I have only to express my surprise that you did not suspect more than a filial solicitude for the Society's honour on his part. My notice of the proceedings of the Society to which I have the honour to belong, and amongst the leading members of

which I have the honour to number so many scientific friends, could arise from no other motive than a desire that its authority and pre-eminent position in the scientific world might be permanent and undiminished, by allowing no analytical essays relating to physical problems to be ushered forth under its auspices, until, in all practicable cases, their accordance with the involved physical facts was ascertained. The notice of one such essay in my former paper was sufficient to show that such had not always been the case. Your correspondent prudently preferred leaning on his influence with you, to challenging me to the proof of my assertion.

I have also to ask at your hands an explanation of the editorial censure contained in your note. I fully bow to your claim of right to moderate any expressions in communications which may be sent to you for publication. The advantage of a review by an impartial editor is great to all parties in a controversy, who in their excitement and sensibility naturally see a poignancy in the expressions of their opponents which they do not suspect in their own. On the other hand, I maintain that it is an unheard of proceeding in an editor who has published a paper without remark or private notice to the author, to pronounce a censure such as is contained in your note; and I claim from you this admission, which I think you will allow to be due under the circumstances.

I remain, dear Sir, yours truly,

Queen's College, February 4, 1840.

RICHARD POTTER.

In inserting Mr. P's letter, we have only to state that the Editor's note was written in consequence of the remonstrance of the Member of the Cambridge Phil. Soc., and before the receipt of Prof. Forbes's letter, with which it had no connection; and was indeed intended to have occupied a distinct place in the Number.

We can assure Mr. Potter that his surmise respecting influence with us is unfounded; and willingly express our regret, that from having overlooked some portions of his communication, they should have become the subjects of public instead of private discussion.—R. T.

XL. *Additional Note on the Authenticity of the disputed Passage in the treatise of Boetius de Geometria on Numerical Contractions.* By J. O. HALLIWELL, Esq., F.R.S., F.S.A., F.R.A.S., &c.

I TRANSLATE the following extract from a letter which I have recently received from M. Chasles, because the view which he takes of this question is new, and his supporting arguments forcible:—"In a passage found in some MSS. at the end of the second book of Boetius, the expression *mensa geometricalia* occurs; and this calculus is mentioned as ha-

ving been employed chiefly by the geometers. This explains why Boetius introduced that passage into his treatise on geometry; and in his treatise on arithmetic, which treats on the properties of numbers, no mention of it is made. This latter work is indeed only a new version of the treatise by Nicomachus on the same subject."

This ought to be compared with what has been stated in the number of this Magazine for December, and it will be seen that it is quite destructive of M. Libri's principal argument. I may add, in corroboration of the opinion of M. Chasles, that Abelard's tract in the Leyden library is entitled *de Doctrina Abaci vel radii Geometrici*; the manuscript itself is thus described in the printed catalogue:—"Adolardus, qui statim in principio dicitur philosophorum assecla ultimus, de doctrina abaci, vel radii geometrici, ut ipse scribit quoque vocari. In fine legitur, *Regularum abaci nobilis arithmetici tractatus explicit feliciter*."

And now a word with M. Libri. When he says, "Si l'opinion de M. Halliwell avait été aussi explicite que le pense le savant géomètre de Chartres, il semble qu'on n'aurait pas dû employer plusieurs pages pour tâcher de le prouver," he had forgotten that the *plusieurs pages* were the produce of his own pertinacity. When I had explicitly stated that the Bodleian manuscripts indicated a knowledge of the value of local position, and that one of them actually made use of the *sipos*, surely no one could reasonably accuse me of withholding my assent from the explanation given by M. Chasles. Much less, in that case, could there have been a necessity for occupying the attention of two meetings of the Institute on a mere question of opinion.

XLI. *On a Pseudomorphous variety of Iodide of Potassium*
By ROBERT KANE, M.D., M.R.I.A.

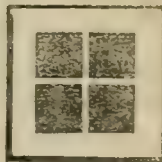
DURING the crystallization of a large quantity of iodide of potassium, in the manufacturing laboratory of Apothecaries' Hall, I observed a large group of long prisms to be formed, of great lustre and regularity. These prisms were in many cases terminated by four-sided pyramids, formed by the joining of four rhomboidal planes, by which the solid angles of the prism were replaced; and I succeeded in obtaining a series of specimens, in some of which the prism was simple and terminated by a single plane perpendicular to its axis; in others the solid angles were replaced by very minute triangular planes, rendering the terminal plane octagonal, and finally, as the triangular planes increased in size, square, the diagonals of the square being parallel to the sides of the

original surface, and hence the gradual conversion of the replacing triangles into rhombs, which effacing the terminal plane formed the pyramidal summit already noticed.

The prisms are straight, with square bases. The replacing triangular or rhomboidal planes form with the lateral edges, to which it is inclined, an angle of 150° , and with the terminal plane an angle of 120° : its angles with the vertical and horizontal axes of the crystal being therefore 60° and 30° , giving the ratio of the axes therefore as $1 : 1.73$. The angle formed by two adjacent rhomboidal planes of the pyramid was found to be 105° , and that of the summit formed by two opposite rhomboidal planes was 60° . The angle across the summit, measured on the edges of the rhomboidal planes, was 80° , and that of a rhomboidal plane, on the adjacent side, was 140° . The angles of the rhomboidal plane were 60° and 120° .

In these measurements I could not obtain greater accuracy than within a degree, from the circumstance that the replacing surfaces were not, in reality, planes, but portions of spherical or at least curved surfaces of great radius, so that the adjacent edge had different inclinations to different portions of the rhombic surface. In addition to this peculiarity, other marks of a complex or macled structure were very evident. The smaller crystals, although equally well marked as to form and replacements with the larger, differed from them in being wholly clean and transparent. The larger ones, on the contrary, consisted of three distinct portions, the external being a hollow shell of perfectly transparent material, the next being a core of opaque white substance, apparently porous and granular, as if formed of a congeries of minute crystals independent of the case in which they were contained, whilst in the centre there was to be seen a delicate but well-defined transparent rectangular cross, the arms of which generally penetrated quite through the opaque substance and united with the external transparent shell.

A section of such a crystal had in fact the appearance represented in the little sketch, the white opaque portion being shown shaded.



These crystals possessed single refraction. They had no action on polarized light transmitted along their axis; and hence, although with a pyramid belonging to the square prismatic system, they belonged really to the regular system, by a congeries of minute crystals (probably cubes) of which they must be formed. In solubility they were the same as the common iodide of potassium, with which their composition likewise identified them. From their

having formed on the top of a large cross of common crystals, they must have been generated under circumstances either on a lower temperature or a less concentrated solution than that by which the common variety is produced. If the new-formed crystals be dissolved in water, it is under the ordinary form that they recrystallize.

When iodide of potassium is crystallized in leaden or tinned iron vessels, Mr. Scanlan has informed me that the crystalline form is altered, from the presence of a minute quantity of iodide of tin or lead; but what the alteration is exactly, I do not believe has been determined. Having heard the fact, however, from my friend Mr. Scanlan, I sought for metallic impurity in the crystals now described, but in vain. They are chemically pure.

XLII. *Proceedings of Learned Societies.*

ROYAL IRISH ACADEMY.

Nov. 11, 1839. **R**EV. Dr. Dickinson gave a verbal account of a remarkable waterspout, which he had observed at Killiney during the last summer.

Towards the end of the month of July, about 10 A.M., while standing on the shore of the bay of Killiney, his attention was directed by a friend to a waterspout, distant about a quarter of a mile from the land. It was not similar in form to the representations of waterspouts usually given, and may therefore deserve to be noticed. It was shaped like a double siphon, the whole being suspended at a considerable elevation in the air; the longer end of the siphon reached towards the sea, and appeared to approach it nearer and nearer, till, at length, its waters were distinctly seen rushing into the deep. The loop gradually lowered, as if sinking and lengthening by its own weight, while the upper part of the siphon seemed not to lose in elevation. At length the loop burst, and there were three streams of water pouring into the sea, two of those streams still continuing united by the arch at the top. The breadth of these streams gradually diminished till they became invisible, but their length seemed undiminished as long as they were at all seen. The quantity of water poured down must have been very considerable, as the bubbling of the sea beneath could be distinctly observed.

Dr. Dickinson was informed that a waterspout fell a few days after inland, towards the Three-Rock mountain. It is said to have done some injury; but his informant did not see it, and he could not, therefore, ascertain its shape.

November 30, 1839. Mr. Clarke read a paper "On Atmospheric Electricity."

The author commenced his paper with a description of the apparatus which he had employed in the experimental investigation of this subject. He showed the inapplicability of the electrometers

hitherto employed, and exhibited a highly insulated galvanometer, containing about three thousand turns of very fine wire covered with silk, varnished and baked,—which instrument, although exquisitely sensitive to the feeblest voltaic electricity, was not at all acted upon by atmospheric electricity of the low tension which exists during serene weather in this country. Mr. Clarke added, that although the application of such an instrument would be a great desideratum in experiments on atmospheric electricity, and in this point of view had been recommended by the highest scientific authorities in Europe, yet he had reason to think that it had never, in any country, been deflected by atmospheric electricity in serene weather.

The author then exhibited the electrometer which he had devised for, and used in his experiments on this subject. It consisted of a bell of glass, seven inches in diameter, through the side of which passed a sliding graduated rod, furnished with a vernier, which indicated the distance, in hundredths of an inch, through which a single pendent slip of leaf gold was attracted towards the rod which was in connexion with the earth. The slip of leaf gold was attached to a vertical and well-insulated rod, which passed through a collar of leathers, and could therefore be raised or depressed, as required by the varying intensity, so that the lower end of the leaf should always, when electrified, be a tangent to the ball terminating the graduated rod.

The author then alluded to the received opinion, that the Aurora Borealis is an electric discharge of considerable intensity occurring near the polar regions, at great heights in the atmosphere, where the air is necessarily rare, and where, consequently, the electric light (as shown in our artificial imitation of the phenomenon) must be very much diffused and ramified. Hoping to throw light upon this subject, he had made a series of observations on the electric intensity of the twenty-four hours, commencing at mid-day on the 12th of Nov, 1838, and continued at intervals of fifteen minutes,—except during the appearance of the Aurora, when they were made every five minutes, and even oftener. The results of these observations were laid down in a chart, which exhibited the intensity of the electric fluid during these twenty-four hours, a period including that of the magnificent crimson Aurora, which was observed on the night of the 12th, and morning of the 13th of November, 1838, over every portion of the globe. It appeared, by this chart, that the electric intensity during the existence of this magnificent display of Auroral light was but little above the mean electric intensity of that hour during the month; from which the author inferred that this phenomenon, if at all electric, occurred at such a distance as to be unable to affect the apparatus.

The author then proceeded to give an account of the extended series of experiments which he had undertaken at the recommendation of the Academy, and which he had continued during twelve months, at intervals of fifteen minutes, during at least ten days, and

from three to seven nights in each month. He stated, that when he had undertaken this series of experiments, he had the following objects in view—namely, to determine the mean amount of electric intensity existing in this country, at the different hours of day and night, and the periods of maxima and minima; and, secondly, to endeavour to trace the cause of this varying intensity to the influence of some of the recognised agents in nature,—such as the variations of atmospheric pressure; the variations of temperature; or the varying quantity of vapour in our atmosphere.

He was happy to announce, that he had not only determined the mean monthly and annual force of electricity at the several hours of the day and night, but also had succeeded in establishing its dependence upon two, out of the three agents, with which he had originally proposed to investigate its connexion. The two with which he has established its connexion and proved its dependence are, *temperature*, and the total *quantity of moisture* present in the air, as shown by the dew-point. Indeed these two phenomena, as the author remarked, are referrible to each other, the temperature producing evaporation, and the force of electricity at any period being shown to be almost exactly proportional to the tension of the vapour so produced.

The hour of the first electric minimum was shown to be about 3 A.M., the electricity increasing with the temperature until 10 A.M., when a slight decrease occurred: the electric tension again commences rising at about 11 A.M., and continues to increase until about 2^h 45^m, P.M.—all these movements being in exact proportion to the elevation of the dew-point and temperature. At 3 P.M. the dew-point and temperature begin gradually to lower, as does also the electricity (but not so quickly); but from 5 to 7 P.M., the electric intensity rises, being acted upon and increased by the precipitation of the evening dew, which has set free the latent electricity of the condensed vapour, in conformity with the experiment of Volta. Again, from 7 P.M., the electric intensity weakens rapidly, and descends in common with the dew-point and temperature, until they all reach their minimum about 3 A.M.

Thus the patient investigation of this subject has laid bare the cause of the varying diurnal intensity of the electric fluid,—showing it to be the result of evaporation, which, besides its agency in carrying the electric fluid from our earth to the upper regions of the air, daily returns it to us by the conducting power of this vapour, in the direct proportion of its quantity.

Dr. Apjohn read a note by George J. Knox, Esq., “On the Oxidating Power of Glass for Metals, and on the want of Transparency in ancient Glass.”

“In a late work, which treats of the manufacture of glass, an experiment of Guyton Morveau is mentioned, in which six per cent. of copper filings having been mixed with pounded glass, and the compound completely melted, it was found to have assumed a red

colour uniformly diffused throughout the mass, so deep as to render the glass nearly opaque. The experiment originated from a workman in the glasshouse having dipped a heated copper ladle into a pot of fused glass. The copper ladle was melted; the casting and annealing of the plates were proceeded with as usual; and on their completion the workmen were surprised to find, that not only were grains of metallic copper imbedded in the substance of the glass, but bands uniformly coloured of a fine bright red, were distributed throughout the mass.

“The experiment of Guyton Morveau, being but a repetition of the accidental one made by the workman, seems to have but little engaged his attention, the colour being conceived to be due to an *imperfect* state of oxidation, as oxide of copper imparts to glass a greenish colour.

“It appeared to me, at first sight, that the red colour was due to the actual solution of the copper in the metallic state, the globules of copper imbedded in the mass having been deposited from a state of solution, upon cooling. To determine this, I mixed in different proportions with powdered glass, iron, lead, copper, silver, bismuth, antimony, tin, gold, platinum, in a minute state of division; and found that glass, when mixed with iron filings, will oxidate and dissolve almost as much iron, when mixed with it in the metallic state, as if it were mixed with it in the state of oxide. Of copper, only a small proportion is oxidated and dissolved, imparting a green colour to the glass, while the rest remains disseminated throughout the glass in globules of copper and red streaks, which are probably the protoxide; whereas lead (for whose oxide glass has such a strong affinity) oxidates but a small portion, when mixed with it in the metallic state, the rest being found imbedded in globules throughout its mass. Tin, antimony, and bismuth, are more easily oxidized and dissolved than lead. Gold, when fused with glass, imparts to it a light greenish tinge, increasing in depth with the relative proportion of silica in the glass,—producing a deeper colour with the bisilicate than the silicate of potash, and still deeper when German glass (which contains a large proportion of silica) is employed; globules of gold are found (as in the analogous cases of lead and copper) disseminated throughout the mass. If the heat be increased, and the crucible containing the gold be left for some hours in the furnace, the glass assumes a pinkish hue, which is the colour imparted to it by the protoxide of gold. When platinum sponge is fused with glass, it sinks to the bottom of the crucible unaltered, owing to its infusibility. When charcoal is heated with glass, a large proportion is oxidated, the remainder presenting the appearance of a mechanical mixture.

“From these experiments it appears, that glass, at high temperatures, not only has the property of oxidating the metals, and forming a chemical compound with the oxide, but moreover, when the chemical affinity is satisfied, of dissolving the oxides, and probably the metals themselves when in a state of fusion; the latter, on the

cooling of the glass, being deposited in globules throughout its interstices, (at least the appearance presented by the glass seems to favour such an opinion.)

“The colours produced by the fusion of metals with glass, being different in many cases from those obtained when their oxides were employed, and presenting the dull untransparent appearance which is so remarkable in ancient glass, led me to suppose that the ancients did not employ any colouring matter unknown at the present day, but that, being unacquainted with the mineral acids, they employed the metals either in the metallic state, in filings, or else in an imperfect state of oxidation. To determine the probability of this conjecture, I selected three specimens of mosaic glass, analysed by Klaproth; and substituting for the oxides, in the same relative proportion, the metals in a minute state of division, I obtained coloured glasses of nearly the same colour as the mosaics, while the colours produced when the oxides were employed were not only perfectly different, but the glasses were clear and transparent.

“One of a lively copper red, opaque and very bright, contained, in 200 grains, silica 142, oxide lead 28, copper 15, iron 2, alumina 5, lime 3.

“Another, of a light verdigris green, contained, in 200 grains, silica 130, oxide copper 20, lead 15, iron 7, lime 13, alumina 11.

“A specimen of blue glass contained, in 200 grains, silica 163, oxide iron 19, oxide copper 1, alumina 3, lime $\frac{1}{2}$.”

December 9, 1839.—Mr. Clarke read a supplement to his paper “on Atmospheric Electricity.”

The author gave in this supplement a more detailed description than he had before done of the mode of insulating the apparatus for experiments on atmospheric electricity, which he had used in the course of his recent researches.

He then described an experiment by which he had shown the absence of decomposing agency in the electricity of serene weather, and stated his opinion of the cause.

Mr. Clarke next directed attention to the fact, that the curve representing the diurnal variation of the barometric column was the reverse of the electric, thermometric, and hygrometric curves. He considered that such a result was to be expected; for the barometric column should naturally be lower from midday to 3 P. M. than at midnight, in consequence of the greater quantity of aqueous vapour which exists in the atmosphere at the former than at the latter time,—air charged with aqueous vapour being known to be of less specific gravity than dry air. Thus the barometric and hygrometric curves would be the inverse of each other, the maxima of the one corresponding to the minima of the other; and as the author had previously shown that the hygrometric, thermometric, and electrometric curves were in accordance, the barometric curve would be the inverse of the thermometric and electrometric curves also. The author remarked, that if this character of the horary

oscillations of the barometer in Ireland be confirmed by the experiments of other observers, it will either lead to new views of this phænomenon generally, or show that the quantity of aqueous vapour existing in Ireland is so great as to cause the horary barometric oscillations to present themselves in a different form from that in which they are recognised in drier climates.

The author adverted, in the last place, to the hypothesis of Priestley and Beccaria,—that the upper regions of our atmosphere were the chief depositories of the electric fluid,—an opinion which he conceived must fall, if the origin of atmospheric electricity be due (as his experiments prove) to the existence of vapour; as these elevated parts of our atmosphere are far above the region of permanent vapour, or even of vapour at all.

Professor MacCullagh read a paper “on the Dynamical Theory of crystalline Reflexion and Refraction.”

In a former paper, presented to the Academy in January, 1837, and printed in volume xviii. of the Transactions, the author had reduced all the complicated phænomena of reflexion and refraction at the surfaces of crystals to the utmost regularity and order, by means of a simple rule, comprised in his theorem of the *polar plane*. This rule, which was verified by its agreement with exact experiments, he had deduced from a set of hypotheses relative to the vibrations of light in their passage through a given medium, and out of one medium into another; but he had not attempted to account for his hypotheses, nor to connect them together by any known principles of mechanics; and the only evidence in favour of their truth, was the truth of the results to which they led. He had observed, however, that these hypotheses were not independent of each other; he had ascertained that the laws of reflexion at the surface of a crystal were connected with the laws of propagation in its interior; and he had thence been led to conclude that all these laws and hypotheses “had a common source in other and more intimate laws not yet discovered.” He became impressed, in short, with the idea, “that the next step in physical optics would lead to those higher and more elementary principles by which the laws of reflexion and the laws of propagation are linked together as parts of the same system.”

This step the author has now made; and the present paper realizes the anticipations scattered through the former. Setting out with the general dynamical theorem expressed by the equation

$$\iiint dx dy dz \left(\frac{d^2 \xi}{dt^2} \delta \xi + \frac{d^2 \eta}{dt^2} \delta \eta + \frac{d^2 \zeta}{dt^2} \delta \zeta \right) = \iiint dx dy dz \delta v, \quad (1).$$

where ξ, η, ζ , are the displacements at the time t of a particle whose co-ordinates are x, y, z , and where the density of the æther is supposed to be unity, as being constant for all media, the author determines the form of the function v , for the particular case of luminiferous vibrations, by means of the property which may be regarded as distinguishing them from all others—namely, that they take place

entirely in the surface of the wave. From this property he shows, in the first place, that v is a function of the three differences

$$\frac{d\eta}{dz} - \frac{d\zeta}{dy}, \quad \frac{d\zeta}{dx} - \frac{d\xi}{dz}, \quad \frac{d\xi}{dy} - \frac{d\eta}{dx};$$

and, in the next place, that the only part of it which comes into play is of the second order, containing the squares and products of those quantities, with of course six constant coefficients. Then, supposing the axes of coordinates to be changed, he proves that the usual formulæ for the transformation of coordinates apply also to the transformation of those differences; so that, by assuming the new axes properly, the terms in the function v which depend on the products of the differences may be made to vanish, and v will then contain only the three squares, each multiplied by a constant coefficient. The axes of coordinates in this position are defined to be the principal axes, (commonly called the axes of elasticity); and when we put, with reference to these axes,

$$-2v = a^2 \left(\frac{d\eta}{dz} - \frac{d\zeta}{dy} \right)^2 + b^2 \left(\frac{d\zeta}{dx} - \frac{d\xi}{dz} \right)^2 + c^2 \left(\frac{d\xi}{dy} - \frac{d\eta}{dx} \right)^2, \quad (2)$$

it turns out that a, b, c , are the three principal velocities of propagation within the crystal.

To find the laws of propagation in a continuous medium of indefinite extent, we have only to take the variation of v from the expression (2), and, after substituting it in the right-hand member of equation (1), to integrate by parts, so as to get rid of the differential coefficients of the variations $\delta\xi, \delta\eta, \delta\zeta$. Then equating the quantities by which these variations are respectively multiplied in the triple integrals on each side of the equation, we obtain the value of the force acting on each particle in directions parallel to the principal axes. The double integrals which remain on the right-hand side of the equation are to be neglected, as they belong to the limits which are infinitely distant. The resolved values of the force thus obtained lead to the precise laws of double refraction which were discovered by Fresnel, with this difference only, that the vibrations come out to be *parallel* to the plane of polarization, whereas he supposed them to be *perpendicular* to it.

When there are two contiguous media, and the light passes out of one into the other, suppose out of an ordinary into an extraordinary one, and we wish to determine the laws of the reflected and refracted vibrations, it is only necessary to attend to the double integrals in the equation of limits; but the integrations must now be performed with respect to other coordinates. Taking the separating surface of the two media for the new plane of xy , the axis of x being in the plane of incidence, let the principal axis x of the crystal make with these new axes the angles α, β, γ , while the principal axes y and z , in like manner, make with them the angles α', β', γ' , and $\alpha'', \beta'', \gamma''$, respectively. Then, marking with accents the quantities relative to the new coordinates, we have

$$\left. \begin{aligned}
 \frac{d\eta}{dz} - \frac{d\zeta}{dy} &= \left(\frac{d\eta'}{dz'} - \frac{d\zeta'}{dy'} \right) \cos \alpha + \left(\frac{d\zeta'}{dx'} - \frac{d\xi'}{dz'} \right) \cos \beta \\
 &\quad + \left(\frac{d\xi'}{dy'} - \frac{d\eta'}{dx'} \right) \cos \gamma, \\
 \frac{d\zeta}{dx} - \frac{d\xi}{dz} &= \left(\frac{d\eta'}{dz'} - \frac{d\zeta'}{dy'} \right) \cos \alpha' + \left(\frac{d\zeta'}{dx'} - \frac{d\xi'}{dz'} \right) \cos \beta' \\
 &\quad + \left(\frac{d\xi'}{dy'} - \frac{d\eta'}{dx'} \right) \cos \gamma', \\
 \frac{d\xi}{dy} - \frac{d\eta}{dx} &= \left(\frac{d\eta'}{dz'} - \frac{d\zeta'}{dy'} \right) \cos \alpha'' + \left(\frac{d\zeta'}{dx'} + \frac{d\xi'}{dz'} \right) \cos \beta'' \\
 &\quad + \left(\frac{d\xi'}{dy'} - \frac{d\eta'}{dx'} \right) \cos \gamma''.
 \end{aligned} \right\} (3)$$

Now if we take the variations of these expressions, and substitute them in the value of δv derived from equation (2), then multiply by $dx' dy' dz'$, and integrate between the limits $z' = 0$ and $z' = \infty$, neglecting to take account of the latter limit, as well as of the integrations with respect to x' and y' , of which both the limits are infinite, we shall get, in the equation which holds at the separating surface, a term of the form

$$f \int dx' dy' (q \delta \xi' - p \delta \eta'), \quad (4)$$

where

$$\left. \begin{aligned}
 p &= a^2 \left(\frac{d\eta}{dz} - \frac{d\zeta}{dy} \right) \cos \alpha + b^2 \left(\frac{d\zeta}{dx} - \frac{d\xi}{dz} \right) \cos \alpha' \\
 &\quad + c^2 \left(\frac{d\xi}{dy} - \frac{d\eta}{dx} \right) \cos \alpha'', \\
 q &= a^2 \left(\frac{d\eta}{dz} - \frac{d\zeta}{dy} \right) \cos \beta + b^2 \left(\frac{d\zeta}{dx} - \frac{d\xi}{dz} \right) \cos \beta' \\
 &\quad + c^2 \left(\frac{d\xi}{dy} - \frac{d\eta}{dx} \right) \cos \beta''.
 \end{aligned} \right\} (5)$$

This term, along with a similar but simpler one arising from the ordinary medium, must be equal to zero; and as the variations $\delta \xi'$ and $\delta \eta'$ are independent, this condition is equivalent to two. Moreover, the quantities ξ' and η' are to be put equal to the corresponding quantities in the other medium, and thus we have two more conditions, which are all that are necessary for the solution of the problem.

The four conditions may be stated by saying, that each of the quantities p , q , ξ' , η' , retains its value in passing out of one medium into another. Hence it is easy to show that the *vis viva* is preserved, and that ζ' likewise retains its value. These two consequences were used as hypotheses by the author in his former paper, and accordingly all the conclusions which he has drawn in that paper will follow from the present theory also.

It will be perceived that this theory employs the general processes of analytical mechanics, as delivered by Lagrange. The first attempt to treat the subject of reflection and refraction in this manner was made by Mr. Green, in a very remarkable paper, printed in the Cambridge Transactions, vol. vii. part 1. After stating the dynamical principle expressed by equation (1), (though with a different hypothesis respecting the density of the æther,) Mr. Green observes, that, supposing the function v to be known, "we can immediately apply the general method given in the *Mécanique Analytique*, and which appears to be more especially applicable to problems that relate to the motions of systems composed of an immense number of particles mutually acting upon each other." Such is certainly the great advantage of starting with that general principle; but the chief difficulty attending it, namely, the determination of the function v , on which the success of the investigation essentially depends, has not been surmounted by Mr. Green, who has consequently been led to very erroneous results, even in the simple case of *uncrystallized* media, to which his researches are exclusively confined. In this case Mr. MacCullagh's theory confirms the well-known formulæ of Fresnel, one of which Mr. Green conceives to be inaccurate, and proposes to replace by a result of his own, which, however, will not bear to be tested numerically. The present theory applies with equal facility to all media, whether crystallized or not, and is distinguished throughout by the singular elegance and simplicity of its analytical details; a circumstance which the author regards as a strong indication of its truth.

A paper was read by Mr. J. Huband Smith, descriptive of certain porcelain seals, amounting to upwards of a dozen, found in Ireland within the last six or seven years, and in places very distant from each other.

He exhibited to the Academy one of these seals, with impressions of several others in sealing-wax. He stated that they were all uniform, consisting of an exact cube, having by way of handle, some animal (probably an ape) seated upon it; and that they were so precisely similar in size and general appearance as to be undistinguishable, except by the characters on the under surface. Little is known respecting these seals beyond the mere fact of their having being found in this country.

An extract from the Chinese grammar of Abel-Rémusat showed that the inscriptions on these seals are those of a very ancient class of Chinese characters, "in use since the time of Confucius," who is supposed to have flourished "in the middle of the sixth century, before J. C." The remote period to which these characters are assigned, leaves open a wide field for conjecture as to the time in which these porcelain seals found their way into this country.

The situations in which some of them have been found are remarkable. One was discovered in ploughing a field near Burriskane, county of Tipperary, in 1832; another was found last year at Killcad, in the county of Down; another in the bed of the river

Boyne, near Clonard, in the county of Meath, in raising gravel ; and a fourth was discovered many years ago at a short distance from Dublin.

From the extreme degree of heat to which they appear to have been subjected, and the consequent vitrification which has in some measure taken place, they are quite as capable of resisting the attacks of time as the glass and porcelain deities and ornaments found in the mummy cases of Egypt, and may have lain for an indefinite period beneath the surface of the earth. It is therefore, at least, possible that they may have arrived hither from the East, along with the weapons, ornaments, and other articles of commerce, which were brought to these islands by the ships of the great merchant-princes of antiquity, the Phœnicians, to whom our ports and harbours were well known.

Mr. Smith then called the attention of the Academy to the remarkable discovery, by Rosellini, Lord Prudhoe, and other recent travellers, of unquestionable Chinese vases in the tombs of Egypt. He read a passage from Davis's *China*, in which some of them were described ; and also an extract from Wilkinson's *Ancient Egyptians*, from which it appeared that the number of Chinese vases found at Coptos, Thebes, and elsewhere, amounted to seven or eight, and that the inscriptions on them had been translated by Chinese scholars to mean, "The flower opens, and, lo ! another year," being a line from an ancient Chinese poem.

From this the trade of China with distant countries, at a period of the remotest antiquity, being clearly proved, Mr. Smith submitted to the Academy that a case of strong probability had been made out, that the porcelain seals found their way into Ireland at some very distant period. In fact, if they be not of modern introduction into this country—a supposition which the situations in which several of them have been found seems utterly to preclude—their arrival here must of necessity have been most ancient.

January 13, 1840.—Sir Wm. R. Hamilton, LL.D., President, in the chair. Professor Mac Cullagh made a communication respecting the optical Laws of Rock-crystal (Quartz).

In a paper read to the Academy in February 1836, and published in the *Transactions*, (vol. xvii. p. 461), he had shown how the peculiar properties of that crystal might be explained, by adding, to the usual equations of vibratory motion, certain terms depending on differential coefficients of the third order, and containing only one new constant c . This hypothesis, which was very simple in itself, not only involved as consequences all the laws that were previously known, but led to the discovery of a new one—the law, namely, by which the ellipticity of the vibrations depends on the direction of the ray within the crystal. He was not able, however, to account for his hypothesis, nor has it since been accounted for by any one.

But the theory developed in the paper which he read at the last meeting of the Academy, now enables him to assign, with a high degree of probability, the origin of the additional terms above-men-

tioned, and, if not to account for them mechanically, at least to advance a step higher in the inquiry. In that theory it was supposed, (and the supposition holds good in all known crystals, except quartz,) that the molecules of the æther vibrate in right lines, the displacements remaining always parallel to each other as the wave is propagated; and it was shown that the function v , by which the motion is determined, then depends only on the *relative displacements* of the molecules. But when this is not the case,—when, as in quartz, each molecule is supposed to vibrate in a curve—then it is natural to conceive that the function v may depend, not only on the relative displacements, but also on the *relative areas* which each molecule describes about every other more or less advanced in its vibration. This idea, analytically expressed, introduces a new term v into the value of the function $2v$; and, if the plane of the wave be taken for the plane of xy , it is easy to show that

$$v = c \left(\frac{d\eta}{dz} \frac{d^2\xi}{dz^2} - \frac{d\xi}{dz} \frac{d^2\eta}{dz^2} \right).$$

Now if we integrate by parts the expression

$$\iiint dx dy dz \delta v,$$

so as to get rid of the variations of differential coefficients, the reduced form of the triple integral will be

$$2c \iiint dx dy dz \left(\frac{d^3\eta}{dz^3} \delta\xi - \frac{d^3\xi}{dz^3} \delta\eta \right);$$

from which it appears that the quantities

$$c \frac{d^3\eta}{dz^3}, \quad - c \frac{d^3\xi}{dz^3},$$

are to be added to the usual expressions for the force in the directions of x and y respectively. These are the very terms in the addition of which the hypothesis before alluded to consists.

The Secretary read a paper by James Orchard Halliwell, Esq., F.R.S., &c., entitled “an Inquiry into the Period of the first Use of the Zero by those Writers who adopted the Notation of the Boetian numerical Contractions*.”

The author referred, at the commencement of this communication, to the opinion which he had formerly expressed on the nature of the change from the use of the abacus, to that of local position, and the cipher. This opinion is contained in the following extract:—“It would be impossible, with the few materials yet brought to light, to conjecture with any great probability, how far these Boetian contractions may have influenced the introduction, or co-operated with the Arabic system, to the formation of our present numerical notation. It appears to me highly probable that the two systems became united; because the middle age forms of the figure

* Papers by Mr. Halliwell on subjects immediately connected with the above, will be found in the last and present vols. of Lond. and Edin. Phil. Mag.

five coincide with the Boetian mark for the same numeral, and those of two others are very similar. The idea of local position, again, may have had an independent European origin; the inconveniences of the abacus on paper would have suggested it by destroying the distinguishing boundaries, and inventing an arbitrary hieroglyphic for the representation of an empty square."

The author then proceeded to adduce evidence from some documents recently discovered in support of these views. He showed from the Mentz MS. in the Arundel collection, in what manner the mode of operation with the abacus had been improved, so as to lead naturally to the present system. He then brought forward some passages from MSS. illustrative of the first employment of the zero; and concluded by adducing an instance from a MS. of the translation of Euclid by Athelard, of the fourteenth century, belonging to the Arundel collection, in which the number 15 is written in these contractions, and without a division.

XLIII. *Intelligence and Miscellaneous Articles.*

PRECIPITATION OF IRON BY ZINC.

M. CAPITAINE states that when a plate of zinc is immersed in a neutral solution of protochloride of iron, the zinc in a short time, especially if heated to ebullition, obeys the magnet and becomes brittle, and on continuing the immersion there remains only a friable fragment of pure iron. Nevertheless, as it may be suspected that some zinc may remain unacted upon, he has invented a very simple arrangement to avoid this inconvenience. It consists in immersing into the solution of iron a plate of copper perfectly cleaned and soldered at one end to a piece of zinc. It is very nearly the same apparatus as employed to obtain the lead tree, and it acts unquestionably in the same manner. The iron is deposited on the copper in a thin friable layer, having a metallic splendour, but without any appearance of crystallization: this mode of operating has no other inconvenience than its slowness; but in whatever manner it is conducted, there is always a disengagement of hydrogen, which continues as long as the metallic precipitation.—*Journal de Chimie Med.* January 1840.

ACTION OF CHLORINE ON THE CARBURETTED HYDROGEN OF ACETATES.

M. Dumas has read a notice to the Academy, of which the following is an abstract. Acetic acid treated with chlorine yields chloroacetic acid, and this under the influence of the alkalies is converted into carbonic acid and chloroforme. If there exist, as I have announced, a similarity of type between acetic and chloroacetic acid, the first ought to give with the alkalies a carburetted hydrogen C^4H^8 , corresponding to chloroforme $C^4H^2Cl^6$. The production of this carburet under the influence of the alkalies is not known; but of the carburet C^4H^8 produced by the acetates corresponding to chlo-

roform $C^4 H^2 Cl^6$, it ought to give rise by means of chlorine to the following series :

$C^4 H^6 Cl^2$ hydrochlorate of methylen

$C^4 H^4 Cl^4$ chloride of hydrochlorate of methylen

$C^4 H^2 Cl^6$ chloroforme

$C^4 Cl^8$ chloride of carbon.

“ I have made many attempts,” says M. Dumas, “ to ascertain the production of these various bodies ;” and he adds, “ according to the details of the experiments, the gas of the acetates acts under the influence of chlorine as the law of substitutions and the theory of types had previously indicated, for the body $C^4 H^8$ is converted into $C^4 Cl^8$; it being well understood that this conclusion relates only to the gas of the acetates, no experiments having been made with pond gas ; and he has but little experimented with the gas from alcohol, which may be merely a mixture. M. Dumas maintains purely and simply his preceding conclusions : acetic acid and chloroacetic acid belong to the same type, and the same exists with respect to chloroforme and the carburetted hydrogen of the acetates ; for acetic acid produces the carburetted gas, under the circumstances in which chloroacetic acid yields chloroforme ; and the chloroforme, as well as the carburetted gas of the acetates, is converted by the action of chlorine into a chloride of carbon $C^4 Cl^8$, which belongs to the same type as they.—*L’Institut*, No. 318.

HYDROCARBURET OF BROMINE.

M. Pelouze and M. Millon by subjecting to the action of bromine the carburetted hydrogen obtained from the decomposition of alcohol by barytes, hydrocarburet of bromine corresponding to the liquor of the Dutch chemists, M. Pelouze stated it as his opinion that this compound, which is perfectly identical with that obtained from olefiant gas, could not be explained by the law of substitutions. He adds, that in his opinion, this law, when it is well known, is only a particular case of the theory of chemical equivalents ; and that he has undertaken with M. Millon, some experiments to support his opinion in this respect.—*L’Institut*, No. 318.

NATIVE SULPHATE OF MAGNESIA.

Indiana, one of the United States, contains a great number of grottos ; one of these, near the Ohio, is celebrated for the masses of Epsom salt which are found in it. The mountain in which it is placed is 400 feet high, and is formed of limestone. The summit is covered with cedars and oaks. The entrance to the grotto is half-way up the mountain ; it is from 12 to 15 feet wide, and 3 to 4 in height. The descent is easily made into a spacious chamber, about a quarter of a mile long ; its height varies from 4 to 20 feet, and its width from 10 to 20. The roof is sometimes flat and sometimes arched. At the extremity of the grotto it bifurcates ; the bifurcation on the right side is short ; that on the left hand leads by some

steps of stones to a stage of ten feet high, and is in a south-east direction. Here the roof begins to form a regular arch, the height of which from the floor varies from five to eight feet, the grotto being from six to twelve feet wide, to the part called the *crawling place*, a name which is given to it in consequence of travellers being obliged to crawl, in order to reach another large neighbouring chamber; from this to the spot, in which a pillar is found, for a mile and a quarter, there occurs an alternating succession of large and small chambers. Sometimes the way is flat, at other times enormous blocks of rock must be climbed, which have been detached from the roof, and then the pillar occurs in the form of a magnificent white column, which reflecting the sombre light of the torches, has a majestic and dazzling aspect. Visitors rarely proceed further than from 100 to 150 fathoms. The pillar or column is 15 feet in diameter and thirty high; it is regularly fasciculated from the summit to the base. Not far from it are several other pillars of the same form, but of smaller dimensions: it is composed of carbonate of lime.

The date of the discovery of this grotto is not known; it is known only that it was visited in 1807 by some persons who found in it a bed of salts from 6 to 9 feet thick on the bottom of the grotto, where they observed enormous blocks scattered over it, whilst the walls were covered with saline products. The sulphate of magnesia abounds throughout this grotto in different forms, and sometimes in masses of one pound to ten. The soil has a brilliant appearance on account of the numerous portions of this salt disseminated in it. This sulphate lines the walls at various distances; if it be removed it is reproduced in four or five weeks in needle-form crystals. The poorest earth which has been washed gave four pounds per bushel, and the richest from 20 to 25 pounds. The salt which next occurs in the greatest quantity is nitrate of lime, and afterwards nitrate of alumina, which yields as much nitrate of potash as the nitrate of lime. Sulphate of lime also occurs, with traces of sulphate of iron and of carbonate and nitrate of magnesia. The sulphate of magnesia is not pure, as will be readily conceived.—*Journal de Chimie Medical*, January 1840.

MANUFACTURE OF CHLORATE OF POTASH.

M. Pelouze has communicated a new and advantageous mode of preparing chlorate of potash. Hitherto carbonate of potash has always been decomposed by chlorine: M. Pelouze describes the inconveniences of this process, which he proposes to remedy by substituting soda for potash; by this chlorate of soda and common salt are obtained, and the chlorate of soda is converted by double decomposition in chlorate of potash by one of the cheap salts of potash which occur in commerce.

M. Pelouze also proposes to pass chlorine into milk of lime, by which chloride of lime is obtained, and this is then decomposed by chloride of potassium.—*L'Institut*, No. 318.

DIABETIC BLOOD AND URINE.

M. Muller observes that the opinion that diabetic blood contains sugar has been many times contradicted, because it has not always been procurable from it.

The following are the results of M. Muller's experiments on diabetic blood obtained by venæsection, and also of the urine :—

Twelve ounces of the blood gave :						Oz.	Dr.	Grs.
Chloride of sodium	0	0	24·5
———— of potassium	0	0	13
Sulphate of potash	0	0	9
Carbonate of potash	0	0	17
———— of lime	0	0	6·75
———— of magnesia	0	0	9
Phosphate of magnesia	0	0	10
Carbonate of soda	0	0	11
Phosphate of soda	0	0	0·5
———— of iron	0	0	22·25
Sugar	0	1	5
Albumen	1	3	27
Hæmatosin	1	5	24
Liquid fat	0	0	19
Crystallizable fat	0	0	33
Fibrin	0	0	26
Extractive matter	0	0	22·5
Carbonate of lime	0	0	7·5
Water	8	1	33

 12.

Fifty ounces of the urine of the same patient contained :

	Oz.	Dr.	Grs.
Diabetic sugar	2	3	37
Urea	0	0	1½
Extract of a very disagreeable odour	0	5	40
Mucous matters	0	0	5
Gum	0	2	26
Albumen	0	0	7
Sulphate of potash	0	0	5
Chloride of sodium	0	0	13
———— of potassium	0	0	3·5
Phosphate of lime	0	0	6
Hydrochlorate of ammonia	0	0	8
Phosphate of soda	0	0	26·25
———— of magnesia	0	0	0·75
Silica	0	0	1
Oxide of iron		trace.	
Hippuric acid		trace.	
Water	46	3	0

Ibid.

SIR JOHN F. W. HERSCHEL'S NEW RESEARCHES ON THE SOLAR SPECTRUM AND IN PHOTOGRAPHY.

The following are some of the points of novelty which occur in a paper by Sir John Herschel, now in course of reading before the Royal Society :—

1. Detection of luminous rays, and a new prismatic colour beyond the extreme violet.

2. Discovery of a chemical spectrum *beyond the extreme red rays*.

3. Assumption, according to circumstances, of either an oxidizing or a de-oxidizing action by the chemical rays at either end of the spectrum.

4. Formation of photographic impressions of the spectrum, exhibiting the prismatic colours in imitation of the colours of those rays by which they are produced ; and a variety of other tints.

5. Photographic effects produced by the simultaneous action of two rays differing in refrangibility, which neither of them, acting alone, are capable of producing at all.

6. Action of the spectrum on vegetable colours.

7. Discovery of a process of secret photographic painting, in which the image may be preserved *ad infinitum* in an invisible state, capable of being at any moment rendered visible.

8. Account of a process for fixing photographic pictures on glass plates.

9. Analysis of the absorbent action of various media on the chemical rays.

10. Account of a self-registering photometer for meteorological purposes.

METEOROLOGICAL OBSERVATIONS FOR JAN., 1840.

Chiswick.—Jan. 1. Overcast : fine. 2. Very fine. 3. Fine : slight rain. 4. Rain. 5. Cloudy and fine : frosty at night. 6. Frosty. 7. Clear and frosty : severe frost at night. 8. Severe frost. 9. Overcast : fine. 10. Overcast : frosty at night. 11. Sharp frost. 12. Frosty : fine. 13. Clear. 14. Hazy. 15. Drizzly. 16. Fine. 17. Foggy. 18. Frosty and foggy : rain. 19. Boisterous, with heavy rain. 20. Rain : fine : boisterous at night. 21. Very boisterous with rain. 22. Cloudy : clear at night. 23. Rain : windy at night. 24. Boisterous. 25. Overcast : rain : fine. 26. Stormy and wet. 27. Clear and cold. 28. Rain : boisterous. 29. Very fine. 30. Hazy. 31. Very fine.

The frost was, for a short time, very intense between the 7th and 8th, being 20° below freezing.

Boston.—Jan. 1. Cloudy. 2. Fine. 3, 4. Cloudy. 5. Fine. 6. Fine : little snow P.M. 7. Fine. 8, 9, 10. Cloudy. 11, 12, 13. Fine. 14, 15. Cloudy. 16. Fine. 17. Rain. 18. Cloudy. 19, 20. Cloudy : stormy with rain P.M. 21. Stormy : thunder and forked lightning with rain A.M. 22. Cloudy. 23. Rain. 24. Stormy : rain P.M. 25. Fine : snow A.M. 26. Rain : rain early A.M. 27. Fine. 28, 29. Rain. 30. Fine. 31. Cloudy : rain early A.M.

Applegarth Manse, Dumfries-shire.—Jan. 1. Fine morning : rain P.M. 2. Very wet A.M. : showery all day. 3. Quiet day with slight showers. 4. Fine day and fair : aurora borealis. 5. Clear day : hard frost. 6. Fine frosty day. 7. Dull and cloudy. 8. The same : thaw. 9. Frost again. 10. Still frosty but cloudy. 11. Wet and stormy. 12. The same all day. 13. Fair, but threatening rain. 14, 15, 16. Wet and boisterous, 17. Clear and tending to frost. 18. Rain again and wind. 19. Heavy rain A.M. : showery all day. 20. Frequent showers. 21. Wind very high. 22, 23, 24. Boisterous weather. 25. The same : slight showers. 26. Moderate but showery. 27. Succession of snow showers. 28. Frost A.M. : snow : thaw P.M. 29. Frost A.M. : fine winter day. 30. Frost early A.M. : change P.M. 31. Slight showers A.M. : fine day.

Days of Month. 1840. Jan.	Barometer.			Thermometer.						Wind.				Rain.		Dew point. Roy. Soc. 9 a.m.					
	London : Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London : Roy. Soc.		Chiswick.		Dumfries- shire.	London: Roy. Soc. 9 a.m.	Chiswick	Dum- fries- shire.	Bost. shire.		London: Roy. Soc. 9 a.m.	Chiswick.	Bos ton.	Dumfries- shire.	
		Max.	Min.		Fahr. 9 a.m.	Self-register. Max. Min.	Max.	Min.	Max.	Min.											
1.	29.724	29.944	29.699	29.30	29.46	29.35	49.8	50.5	44.3	55	46	51	44	39	s.	calm	e bys.	44
2.	29.738	29.941	29.757	29.21	29.25	29.41	47.6	48.0	47.6	52	39	47	47	42½	sw.	sw.	sw.	43
3.	30.000	29.999	29.924	29.47	29.73	29.95	43.2	43.6	41.8	46	40	43	45½	42	w.	calm	w-N.	40
4.	29.956	29.975	29.908	29.60	29.90	29.89	39.7	40.6	40.6	44	32	38	39	32	ne.	calm	ne.	40
5.	29.896	29.968	29.902	29.52	29.84	29.87	36.3	36.6	36.7	40	24	30	35½	27½	n.	calm	n.	37
6.	30.000	30.253	30.000	29.60	29.99	30.18	33.6	34.0	31.5	38	19	31	34	27½	nw.	calm	ne.	32
7.	30.282	30.313	30.259	29.92	30.10	29.98	22.0	32.0	29.0	32	12	27	32	19½	ene.	calm	ne.	28
8.	30.156	30.219	30.182	29.73	29.93	30.03	22.8	29.4	22.3	32	26	31	38	29	sw.	calm	nw.	17
9.	30.224	30.389	30.264	29.80	30.09	30.21	33.2	32.8	22.3	37	29	35	35	27	sw.	calm	ne.	28
10.	30.476	30.588	30.511	30.20	30.26	30.25	34.2	34.8	32.6	35	15	33.5	34½	25	e.	calm	ws.	26
11.	30.516	30.557	30.413	30.13	30.14	30.07	27.4	37.2	26.0	37	17	26	41½	34	se.	calm	sse.	27
12.	30.286	30.352	30.288	29.94	29.85	29.87	31.5	35.3	27.3	42	22	27	44	38	se.	s.	s.	27
13.	30.144	30.192	30.003	29.83	29.71	29.47	34.9	39.8	31.4	40	29	31	45	42	se.	s.	se.	28
14.	29.924	30.078	29.944	29.55	29.54	29.68	40.9	41.5	34.8	46	41	37	46	41½	s.	se.	se.	33
15.	30.142	30.131	29.879	29.68	29.70	29.36	43.2	44.7	40.2	46	38	43	46½	43	ssw.	se.	s.	39
16.	29.718	29.797	29.760	29.23	29.30	29.39	40.7	46.8	40.8	43	36	40	46½	40	sw.	w.	w.	40
17.	29.490	29.858	29.518	29.18	29.42	29.71	41.5	46.6	40.8	43	24	39	42	35	s.	calm	nne.	36
18.	29.972	29.990	29.726	29.54	29.43	29.28	36.3	43.8	34.2	50	40	33.5	49½	28	s.	calm	sw.	44
19.	29.436	29.697	29.358	28.94	28.88	28.80	49.4	50.7	35.6	54	40	46	46½	41	s.	calm	ws.	44
20.	29.618	29.712	29.425	29.15	29.20	29.03	42.6	54.2	41.4	54	44	42.5	42½	36	s.	w.	w.	41
21.	29.312	29.726	29.325	28.73	28.73	29.05	51.3	53.7	41.4	52	43	51	44	35½	s.	w.	ssw.	45
22.	29.684	29.838	29.652	29.25	29.10	29.05	48.3	53.2	45.0	51	38	47	44	37½	s.	w.	w.	43
23.	29.808	29.841	29.622	29.10	29.05	29.05	47.7	51.4	41.2	54	49	41.5	49	35½	s. var.	w.	s.	41
24.	29.346	29.366	29.010	28.73	28.78	28.41	52.0	53.5	47.4	53	37	55	49	40	s. var.	s.	ssw.	47
25.	29.316	29.382	29.352	28.76	28.77	28.95	43.0	53.3	39.6	54	38	39	39	36	s.	w.	sw.	42
26.	28.938	29.311	28.742	28.43	28.44	28.62	50.5	52.7	40.6	52	54	48	44½	35½	s. var.	w.	ws.	45
27.	29.444	29.758	29.499	28.91	29.00	29.28	37.8	38.5	37.0	46	35	36	39½	29½	sw.	w.	w.	38
28.	29.422	29.427	29.182	29.05	29.05	28.85	42.8	43.4	37.0	54	42	36	37½	32	s.	calm	ws.	40
29.	29.500	29.830	29.562	29.56	29.35	29.54	44.8	45.5	42.6	47	26	37	39½	33	w.	n.	ws.	41
30.	29.938	29.962	29.685	29.53	29.30	29.45	34.4	35.0	34.6	45	30	34	39	27	ssw.	calm	se.	36
31.	29.574	29.584	29.539	29.15	29.20	29.30	43.4	44.3	34.8	50	29	41	42	33½	s.	s.	sse.	40
Mean.	29.806	29.931	29.732	29.36	29.440	29.474	40.4	43.5	36.9	45.93	32.64	38.6	42.34	34.3	Sum.	2.48	Sum.	1.25	4.61	Mean.	35.6

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

APRIL 1840.

XLIV. *Letter from M. KREIL, Director of the Observatory at Milan, to M. KUPFFER, Director General of the Physical Observatories in Russia, containing a succinct Account of the principal Results of M. KREIL's Magnetic Observations at Milan.* Communicated by Major SABINE, R.A.*

THE observations arrange themselves under three heads: i. e. absolute observations, observations of the periodical changes, and of the perturbations of the magnetic forces. The absolute observations have shown that a correction must be applied to the declinations previously published, which had been determined within the confines of the Palace of Brera, where the Astronomical Observatory is placed, and where masses of iron appear to have exercised a disturbing influence. A series of observations made last spring in an open meadow 640 metres from the observatory, showed that the previous determinations were $23' 16''$ in excess. A second series made in the botanical garden adjoining the observatory, on a spot which is 47 metres from the palace, but which was not available before, gave an error of $21' 51''$. I have taken the mean of these determinations, i. e. $22' 33''.5$, as the quantity by which all the declinations hitherto published are to be diminished.

The apparatus employed in these observations was made in Göttingen by M. Meyerstein, and is furnished with two needles, similar in form and weight, but of unequal magnetism. Under circumstances precisely similar, one of them (No. 4.) makes a vibration in $25''.2$; the other (No. XVII.) in $29''.6$.

* From the *Bulletin Scientifique publié par l'Acad. Imp. des Sciences de St. Pétersbourg*, vol. v. No. 21.

If these two needles are employed for determining the declination precisely in the same manner as described in p. 146 of the 1° *Supplemento alle Ephemeridi di Milano*, a constant difference of nearly 8 minutes is found between their results, the weaker needle (No. XVII.) giving the greatest declination. With needles of still weaker magnetism, some of which have been made here, this difference amounts even to half a degree, not only when the observations are made within the influence of the iron of Brera, but also in the open meadow. All the observations of declination hitherto published have been made with the stronger needle (No. 4.). I have not been able to discover any reason for this phænomenon, unless it is caused by temporary magnetism. However it showed itself in the first year too plainly to be doubted; and I should have mentioned it before, if I had not been desirous of obtaining the greatest certainty which repeated observations could give, and of waiting to see whether the same circumstance might not have been remarked by other observers. In respect to the horizontal intensity I have not as yet been able to perceive any difference between the determinations made with these two needles.

The observations for the periodical changes were made six times a day; they included in the first year observations of declination and of the time of vibration of the horizontal needle; in the second year the inclination was observed in addition to the above phænomena; and in the third year the time of oscillation of the dipping-needle was also included. The result has shown that no clear view of these complicated phænomena can be obtained unless they are *all* observed. The results derived from our observations are the following:

1. In Milan, and at the present epoch, the horizontal portion of the magnetic force reaches its least daily intensity between 8 and 10. 30 a.m., it then immediately begins to increase rapidly and attains its greatest intensity between 4. 30 and 7. 30 p.m., after which it decreases. An irregularity shows itself in the increase of the force, which is still rapid between 1 and 2 o'clock, becomes almost imperceptible between 2 and 4, is again more rapid between 4 and 6, and is then changed into a decrease; we shall see presently the explanation of this.

2. These epochs of the maximum and minimum of the horizontal intensity, which as they are obtained from all the observations may be termed mean epochs, are not constant. In the summer months the minimum is earlier and the maximum is later than in the winter.

3. The difference between the maximum and minimum is

greatest near the time of the summer solstice and least in December.

4. The intensity of the horizontal force increases from January to June, and decreases from July to December.

5. The declination begins to increase at 8 in the morning, and increases rapidly until 1 or 2 in the afternoon, when it attains its highest amount; it then decreases more slowly until it reaches its lowest value: in the winter months the declination is usually less at 11 in the evening than at 8 in the morning, but only occasionally so at other seasons of the year.

6. The mean value of the difference between the greatest and the least declination is $12' 2''$. The difference is greatest in the month which follows the vernal equinox, and least in December.

7. This diversity is the consequence of the annual change in the declination, which at different hours in the day follows an opposite course; in the forenoon, it decreases in spring and increases in autumn; in the afternoon, it increases in spring and decreases in autumn; hence it follows that there must be some hour in the day which is free from the annual change in the declination and which is therefore most suitable for insulated experiments. At Milan and at the present epoch, this hour is between 10 and 11 a.m.

8. The total force attains its least intensity at 8 in the morning or earlier, and its greatest intensity between 1 and 4 p.m. We are still in want of a sufficiently extended series of observations to furnish with equal certainty other facts relating to this element.

9. The inclination increases in the morning till towards 10 o'clock, when it decreases, but not uninterruptedly, for it increases again in the afternoon, and at 4 attains a second maximum, after which it decreases without interruption till near midnight. This alteration in the inclination retards the time of the maximum and minimum of the horizontal force by two or three hours; and its second maximum explains the anomaly mentioned in No. 1.

10. The amount of the alteration of the inclination seems to be also dependent on the season of the year. A greater alteration (above a minute) was observed in summer, and a less (about half a minute) in winter.

11. The times of the absolute maximum and minimum are very variable, but even in this variability a law is plainly manifest. In January and February the maximum was observed at 4. 30' p.m., in March and April at 10. 30' a.m., from May to August at 8 a.m., in September and October at 10. 30' a.m., in November at 1 p.m., and in December at 4. 30' p.m.

The minimum of inclination was at 8 a.m. in the winter months (i. e. November to March) and at 11 p.m. in the remaining months, with the exception of June and July, when it took place as early as 7. 30' p.m.

These results are deduced from the general or monthly means of the different hours of observation. Another combination of the observations in daily means, i. e. the averages of all the observations taken on the same day, ought to show those alterations which have a period longer than a day and less than a year: a monthly period is thus shown; but as yet it is only in the horizontal elements that it can be recognised with certainty. The observations with the inclinorium have not been brought into the calculation, because they were frequently interrupted, and because at first the axis of oscillation of the needle was too far removed from its centre of gravity.

12. If the daily means of the times of vibration of the horizontal needle reduced to the temperature of 0, are combined together in such series that the middle of each shall coincide with a phase of the moon, and if the means of these series are freed from the influence of the loss of magnetism of the needle by being reduced to the same epoch, we then see that the total means of all the times of vibration observed near the new moon, and during the first quarter, are less than those near the full moon and in the last quarter. If we compare the different months with each other, we see that the phænomenon, as it is here enounced, is only found in the eight months from November to June, and that in the four remaining months, i. e. July to October, the contrary takes place; for in the latter interval the longest times of vibration coincide with the new moon and the first quarter, and the shortest times of vibration with the two other phases.

13. This phænomenon might be thought to be an effect of the rotation of the sun round its own axis, which, supposing the sun to be magnetic, would cause sometimes one and sometimes the other of the poles of its magnetic axis to be turned towards the earth; and this hypothesis would also explain the alternations of the phænomena according to the different seasons of the year, as the earth is opposite to one or the other of the solar hemispheres according as she is in the summer or in the winter half of her orbit: but this will not hold good. The epoch of the least value of the intensity is open to the objection that the time of rotation of the sun is two days shorter than the time of the synodic moon, and this difference of time combined with the different positions of the earth relatively to the sun, would cause the phænomenon to

be nearly the same in the summer and in the winter months. We must therefore give up the idea of the effect being produced by the sun, and must seek its cause rather in the position of the moon's path, by which in winter the moon when new is but little raised above the horizon, whereas in summer when she is in this phase she approaches the zenith. If she has a sensible influence on the horizontal needle, it must be greatest when she is near the horizon, and thus the alternation of the phænomena at different parts of the year would be explained. If this be the true reason, it must show itself also when the observations are combined by another method, that is to say, when they are arranged according to the moon's declination. The daily means were therefore formed into series, one of which always comprehended all the observations of the same or of two successive months during which the moon's declination was south, and the other, all those during which the moon was north of the equator. These series, when freed from the gradual increase of the time of vibration caused by the diminishing magnetism of the needle, and collected into two general means, showed that the time of vibration (which is nearly $22' 5''$ mean time) is less by $0''.00168$ when the moon has south declination than when she is north of the equator, which confirms the above-mentioned hypothesis.

14. As the influence of the moon differs so sensibly according to the difference of her position in the heavens, it seemed worth while to examine whether her greater or less distance from the earth might be indicated by our magnetic needles. The daily means were collected for this purpose into series, in such manner that the middle of one series should coincide with the moon's apogee, and the middle of the next with her perigee, then proceeding as before. The general means showed that the times of vibration at the time of the perigee were $0''.00198$ less than at the time of the apogee, agreeing also with what has been said above.

15. If the intensity of the magnetic force is so sensibly subject to the influence of the moon, it is highly probable that the direction of our needles may also be altered by it, and it must be possible, by a suitable mode of combination of the observations, to recognise such an alteration. It is plain that the daily means are inapplicable for this purpose. The observations of each hour must be considered apart, and divided into series according to whether at the time of observation the moon was east or west of the magnetic meridian. It is true that in single months, the effect of this influence is obliterated by the annual alteration in the declination, which

as we have seen in (7.) is different at different hours of the day. But in a longer series of observations, on account of the periodicity of the last named alteration, the effect of a constant though much weaker cause acting according to quite a different law must be traceable, as the result has shown. The following are the differences at the different hours of the day between the declinations found with moon east and moon west.

Hours.	⊖ East.—⊖ West.
20 0	+ 10 ^{''} ·8
22 30	+ 27 ·5
1 0	+ 9 ·1
4 30	+ 25 ·9
7 30	+ 8 ·3
11 0	+ 8 ·0
Mean	+ 14 ·9

It is true that these numbers are not yet corrected for the secular decrease of the declination, but it does not appear to have been great enough to affect the result essentially. In the year 1836 the observations were not made exactly at the same hours, and cannot therefore be employed for determining the secular alteration, but from 1837 to 1838 it was 59^{''}·8, which would give a correction of 2^{''}·5. This if doubled would not alter any of the signs in the above table. We conclude hence, that in this country the declination is always greater when the moon is east of the magnetic meridian, and less when it is west.

16. All the results which we have derived from our observations in reference to the effects produced by the moon agree in presenting her as a body exercising magnetic influence, and in which the prevailing magnetism on the hemisphere turned towards the earth attracts that pole of our needles which is turned towards the south, and increases the magnetic intensity of our hemisphere.

The observations of perturbations have manifested the following facts :

17. Great disturbances frequently occur on the same day, or nearly on the same day, in different years. Thus the greatest disturbances which took place in the year 1836 were on the 22nd and 23rd of April, and on the 18th of October, and both these were repeated on the same days in 1837. In 1838 several of the disturbances took place a few days later

than they had done in 1837, as is shown by the following comparison :

	Jan.	Feb.	Mar.	April.	June.	July.	Aug.	Sept.	Oct.	Dec.
1837.	25	13 18	22	6 22 27	2	2 28	25	14 16	18	14 19
1838.	28	16 21	17	11 29 30	4	12 34	23	14 15 16	17	20 28

These coincidences would show a new fact, namely, the periodical nature of these phænomena, if equally considerable disturbances had not occurred, (ex. gr. those of the 12th, 14th, and 15th November 1837, and of the 17th January 1838,) without any corresponding disturbances being traceable in other years. The subject requires to be further elucidated by continued observation.

Another point to which the attention of the observer should also be directed is, the symmetrical arrangement of these disturbances in the same year, many of them being nearly six months apart, for example :

In the year 1836 and

1837 the disturbances on the 22nd April and 18th Oct.

1838 — — 17th Jan. and 12th July.

do. — — 21st Feb. and 23rd August.

do. — — 29th April and 31st October.

do. — — 4th June and 5th December.

18. All hours of the day do not appear to be equally favourable to the development of this phænomenon, at least its first indications occur much more frequently in the evening than in the morning hours. If we take from the 105 disturbances marked in our day-book, those which were obviously continuations of perturbations which had begun on preceding days, as well as those which were first remarked during the intervals between the regular hours of observation, then the first indications of

19 were at 20^h 0'

1 — at 22 30

9 — at 1 0

21 — at 4 30

16 — at 7 30

11 — at 11 0

Here it is to be remarked, that the observation hour, 11, precedes the longest interval namely, the night, and that the greater number of the more considerable disturbances extend over several hours or even days; therefore perturbations which may have begun in the early hours of the night ought

to be taken from those which are ascribed to the first hour in the morning when the apparatus was observed. It follows that much fewer disturbances begin at that hour than in the evening. It is remarkable that a disturbance hardly ever begins in the latter hours of the forenoon.

19. The perturbations appear to commence for the most part suddenly, as by shocks. At least it was so in those cases in which the phænomenon began under our eyes, that is to say, at the time of an observation. It was so on the 18th of February 1837, on which day an aurora showed itself with a magnificence unusual in this country. Two series of transits of a division of the scale across the wire had been observed as usual. In the first series at 4^h 38^m these passages agreed to a tenth of a second of time;—a proof that the needle was still performing its minute vibrations with perfect regularity; nor in the preceding observations of the same day was any trace of disturbance to be discovered. In the second series, which was made 12 minutes later, there were differences of 2 seconds of time, and the needle was visibly drawn backwards and forwards by the disturbing forces. I may be permitted to mention one more among many similar cases. It is that of the very great disturbance of the 14th of November 1837, at which time we were making magnetic observations for three days uninterruptedly from 5 to 5 minutes, and when necessary at still shorter intervals, for the purpose of examining whether the periodical phænomenon of the falling stars was or was not connected with magnetism. Between 10 and 11 in the evening the needle appeared pretty tranquil, although earlier in the day it had been much disturbed. The observer, Sig. Della Vedova, was engaged in observing the passage of a division of the scale across the wires, and had chosen for that purpose a division nearly in the middle of the arc of vibration, which was then about 7', when all at once, at 11^h 7', he saw that this division did not come to the wire, although it had taken its direction towards it; but before reaching it, the needle had turned the opposite way, and immediately after the scale disappeared from the field of view, which was left quite dark. The observer, thinking that the lamp which illuminates the scale had gone out, was about to rise in order to light it again, when he saw the scale suddenly reappear, move rapidly across the field, and disappear on the opposite side. The rapidity of the movement indicated a much larger arc of vibration. The arc had in fact increased 40 minutes without any apparent cause. As the observations could not be exact whilst the vibrations were so great, Sig. D. Vedova was about to employ a magnetic bar which is always

at hand to be used in quieting the needle; but he was anticipated, for the arc of vibration suddenly diminished to less than a minute, so that the needle appeared quite stationary; at the same time the declination increased so rapidly, that its alteration in the course of a minute of time amounted to $6'$ of arc, a quantity which at this season of the year is hardly traversed by the needle in the course of an entire day.

The influence of the disturbing forces usually affects all the elements at the same time, but it also happens not unfrequently, particularly in minor disturbances, that their effect only reaches the most sensitive of the three elements, i. e. the declination; and some cases, though rare ones, have occurred in which the time of vibration of the horizontal needle underwent considerable alteration without the declination being affected at the same time; an example of this occurred on the 15th of November 1837 between 6 and 10 a.m.

20. The greatest change of declination during a perturbation yet observed in Milan was on the 14th of November 1837. It amounted to $1^{\circ} 11'$, which is nearly ten times the mean daily alteration in this month: by reason of its great variability this element returns to its usual value sooner than the other elements. More examination is still required to manifest, whether the general tendency of the perturbations is to increase or diminish the average amount of declination, and whether their occurrence is connected with the hour of the day.

21. The time of vibration of the horizontal needle is always increased by a disturbance, i. e. the force is lessened; but even in this respect there are such fluctuations, especially soon after the beginning of a disturbance, that sometimes very small times of vibration occur, though this is only of very short continuance. The greatest change of this kind was observed on the 17th of January 1838, when it amounted to nearly $0''\cdot3$, the time of one vibration being $22''\cdot3$. In the greater disturbances it is sometimes the second or third day before the time of vibration returns to its previous value.

22. During a disturbance the inclination always seems to be greater, but it is subject to as great fluctuations as the other elements. The greatest alteration which we observed was on the 21st of February 1838, when it amounted to $8' 45''$; whereas the mean diurnal alteration in this month is only $1' 6''\cdot6$. The dipping-needle usually returns to its ordinary direction on the following day, but sometimes not for a few days.

23. The disturbances likewise increase the times of vibration of the dipping-needle, showing that their influence ex-

tends also to the total force. The needle in our inclinatorium completes one vibration in nearly 14 seconds: during the disturbance of the 17th of January 1838, it underwent an alteration of $0''.074$. In February of the same year the mean of the observed times of vibration for the month was $13''.874$, but for the 21st day of the month it was 13.932 . I need not remark how important it would be to examine such kindred phænomena as those of the atmosphere and auroras, with the same exactness which is now applied to magnetic investigation.

I must mention one other phænomenon which claimed our attention in a very high degree, and which perhaps may deserve that of other observers. I mean the vertical oscillations which show themselves so often in the dipping-needle, and which also appear to be connected with determinate laws. At least they are much more frequent in November and December than at other seasons, and they occur most commonly in wet weather. For this reason I do not think that they can be ascribed to a tremulous motion of the building, from which the part of it which contains the apparatus is quite free. Neither can they be attributed to the effect of currents of air, because they occur less frequently at the time of the equinoctial gales and other storms than at the above-named times. Perhaps they arise from very weak shocks of earthquake, which may be revealed to us by this highly sensitive apparatus, and which probably occur much more frequently than the more considerable shocks which are recognised by our senses, and by other effects. At least the greatest vertical oscillations of the needle have almost always coincided with considerable earthquakes, often having their seat in remote countries. One striking instance of this kind, after we had experienced other similar ones, occurred on the 23rd of January 1838. Between $7^h 33^m$ and $7^h 47^m$ p.m., Milan mean time, the needle began to oscillate so strongly that its arc of vibration appeared, by the vertical scale attached, to amount to 27 millimeters, or nearly 10 minutes of arc; there were no other indications of an earthquake of any kind. Twenty days afterwards the newspapers contained accounts of considerable damage caused by an earthquake on the same evening at Bucharest, Jassy, Odessa, and other places. According to these accounts the shock was felt at Jassy at $7^h 42^m$, and at Odessa at $7^h 45^m$ Milan mean time, agreeing with the beginning of the phænomenon observed by us.

Milan, Jan. 9, 1839.

XLV. *Observations on the supposed Formation of inorganic Elements during Fermentation.* By Mr. J. DENHAM SMITH.*

IN the October Number of last year's Phil. Mag., [present series, vol. xv. p. 329] there appeared an abstract of various papers read at the meetings of the Royal Society, one of which, entitled "Additional Experiments on the Formation of Alkaline and Earthy Bodies by chemical action when carbonic acid is present, by Robert Rigg, Esq., F.R.S.", attracted my particular notice, from the novel and most extraordinary nature of the results announced in it.

Although the Royal Society has always carefully disclaimed any participation in, or support of, the theories and observations brought before or published by it, yet the circumstance of a paper being read to the first learned society of these kingdoms, and the author of that paper a Fellow of the Society, gives weight and sanction to the observations adduced. This circumstance was one of the chief reasons which led me to make the following experiments on the subject of this novel formation of inorganic elements by catalytic action.

I much regret that in the abstract of the paper which appeared in this Magazine, no details of any of the experiments are given; the substances used, the apparatus, and the results obtained, only being mentioned. It appears that "the author gives a detailed account of several experiments in which sugar, water, and yeast only were employed, and from which he deduces the conclusion that alkaline and earthy matters are formed by chemical action. In one set of experiments, some of which were made in silver, others in china, and others in glass apparatus, after the vinous fermentation had gone on during five days, the quantity of ashes obtained was, *in the silver apparatus eighteen, in the china nineteen, and in the glass fifteen times greater than the previous quantity*†. A further examination of these ashes showed that they consisted of potash, soda, lime, and a residue not acted upon by muriatic acid."

Thus having no data of the respective quantities of sugar, water, and yeast Mr. Rigg used, I may not have employed these substances in the proportions with which he experimented; if this should be the case, I presume, however, that this circumstance will not in any way tend to vitiate the results I have obtained; the question being whether inorganic matter is produced during vinous fermentation.

* Communicated by the Author.

† These sentences are not printed in italics in the original.

To satisfy myself respecting the correctness of Mr. Rigg's statement that *the quantity of inorganic matter in a liquid is increased from fifteen to nineteen times when carbonic acid is present*, I dissolved 1500 grs. of the best refined sugar in $1\frac{1}{2}$ pint of distilled water, and added 200 grains of risen beer yeast, then thoroughly mixed them by agitation. This solution was passed through fine cambric to separate any insoluble impurities which the solution contained, and divided into three exactly equal portions. Of these, two portions were respectively placed in German glass jars, and immediately covered with unglazed paper covers; the paper was of a close texture, and was carefully gummed down round the exterior of the jars, to prevent any inorganic matter, as dust, &c. getting into the solutions. These jars were placed in a warm situation in the laboratory, the temperature varying from 60° to 70° Fahrenheit, and the fermentation allowed to proceed. The third portion was then put into a flask and boiled, occasionally adding pure nitric acid; this acid left no stain when a portion was evaporated to dryness in a porcelain capsule; the flask was kept in an oblique position, to prevent any of the liquid being ejected by the action of the nitric acid. During the ebullition of the liquid, nitrous acid fumes were slowly formed and the solution assumed a primrose yellow colour; numerous spherules of liquid were formed, apparently on the surface of the boiling fluid, and coursed about hither and thither with great velocity, the larger spherules seemingly attracting the smaller; and when by this union the globule had attained about the size of a coriander seed, it disappeared, being again united to the bulk of the boiling solution. I imagine that this singular and interesting phænomenon is owing to small portions of the liquid being ejected from its bulk, by the rapid action of the nitric acid on the organic matter in the liquid; and that these particles on again approaching the surface of the fluid, there meet with a stratum of nitric oxide gas or a mixture of this gas and steam, which prevents their contact with the subjacent liquid, and upon which stratum of vapour they float, until by the increase of size, and consequently of weight, the buoyant power of this stratum of gas or vapour is insufficient to prevent their coming into contact with the mass of the liquid, and that they then reunite with it, disappearing instantaneously.

Oxalic acid was formed, and then decomposed by the continued action of the nitric acid; and the residue of the liquid, after evaporation to dryness in a platinum crucible, weighing 630.4 grains, was ignited to redness in a gas furnace, with the occasional addition of a few drops of nitric acid; an ash

of a light buff colour remained, weighing, with the crucible, 631.97 grs. — $630.4 = 1.57$ gr. of inorganic matter contained in 500 grs. of sugar and 66.6 grs. of yeast before fermentation. On examination, this ash was found to consist of an alkaline carbonate, traces of a chloride and of a sulphate, phosphates of lime and magnesia in large proportions, and minute traces of silica and oxide of iron.

At the expiration of six days, one of the portions which had undergone the vinous fermentation, and which presented the agreeable odour accompanying this stage of fermentation, was evaporated in a mode, and with precautions, exactly similar to the above, and the same phænomena were observed during the operation. The residual liquid evaporated to dryness in the platinum crucible weighing 630.38 grs., and ignited over a gas lamp to full redness as in the first experiment, afforded an ash similar in appearance to the former, which with the crucible weighed 631.97 grs. — $630.38 = 1.59$ grs. of inorganic matter yielded by 500 grs. of sugar and 66.6 grs. of yeast, after undergoing the vinous fermentation. This ash was similarly constituted with that obtained in the first instance.

From these experiments we find that whilst 500 grs. of sugar and 66.6 grs. of yeast afford previous to fermentation 1.57 grain of inorganic matter; when fermented they give 1.59 gr., an increase of $\frac{1}{50}$ gr., or of about $1\frac{1}{4}$ per cent.; an increase so trifling that I do not hesitate to refer it to an error of experiment, and not to the formation of inorganic elements during vinous fermentation, which Mr. Rigg asserts is the case. I therefore conclude, contrary to the views entertained by Mr. Rigg on this subject, that *there is no formation of inorganic matter during the progress of vinous fermentation.*

I am at a loss to offer any feasible explanation of the enormous increase of inorganic matter observed by Mr. Rigg; the only mode by which this could have taken place, which at present occurs to me, and that an unlikely one, is that sufficient precautions were not taken to prevent the introduction of foreign matters by securely covering the solutions of sugar and yeast whilst fermenting; and that a quantity of dust, the constant plague of a laboratory, became mixed with his solutions, and thus led Mr. Rigg to suppose that the alkalis and earths were absolutely formed during fermentation. I may remark that the paper covers with which my fermenting solutions were protected from the dust, were so thickly covered with it, that had the precaution of covering the solutions not been taken, I must have obtained a very considerable increase

in the weight of the ash after fermentation, although I do not imagine it would have been to the extent of fifteen to nineteen times the weight of the ash previous to it.

Duke Street, Liverpool, March 1840.

XLVI. Contributions to Electricity and Magnetism. No. III. on Electro-magnetic Induction. By JOSEPH HENRY, LL.D., Prof. of Natural Philosophy in the College of New Jersey, Princeton.

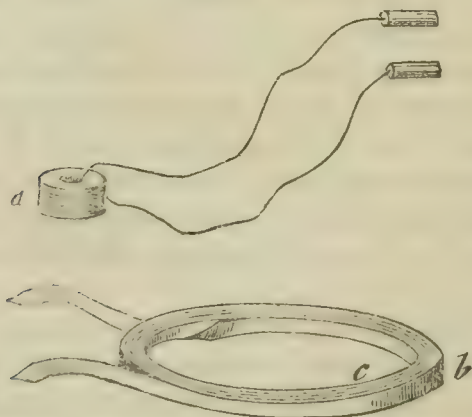
[Continued from p. 210.]

SECTION III.—On the Induction of Secondary Currents at a distance.

45. **I**N the experiments given in the two preceding sections, the conductor which received the induction, was separated from that which transmitted the primary current by the thickness only of a pane of glass; but the action from this arrangement was so energetic, that I was naturally led to try the effect at a greater distance.

46. For this purpose coil No. 1 was formed into a ring of

Fig. 4.



a represents helix No. 4, *b* coil No. 1, in the form of a ring.

about two feet in diameter, and helix No. 4 placed as is shown in the figure. When the helix was at the distance of about sixteen inches from the middle of the plane of the ring, shocks could be perceived through the tongue, and these rapidly increased in intensity as the helix was lowered, and when it reached the plane of the ring they were quite severe. The effect, however, was still greater when the helix was moved from the centre to the inner circumference, as at *c*: but when it was placed without the ring, in contact with the outer cir-

cumference, at *b*, the shocks were very slight; and when placed within, but its axis at right angles to that of the ring, not the least effect could be observed.

47. With a little reflection, it will be evident that this arrangement is not the most favourable for exhibiting the induction at a distance, since the side of the ring, for example, at *c*, tends to produce a current revolving in one direction in the near side of the helix, and another in an opposite direction in the further side. The resulting effect is therefore only the difference of the two, and in the position as shown in the figure; this difference must be very small, since the opposite sides of the helix are approximately at the same distance from *c*. But the difference of action on the two sides constantly increases as the helix is brought near the side of the ring, and becomes a maximum when the two are in the position of internal contact. A helix of larger diameter would therefore produce a greater effect.

48. Coil No. 1 remaining as before, helix No. 1, which is nine inches in diameter, was substituted for the small helix of the last experiment, and with this the effect at a distance was much increased. When coil No. 2 was added to coil No. 1, and the currents from two small batteries sent through these, shocks were distinctly perceptible through the tongue, when the distance of the planes of the coils and the three helices, united as one, was increased to thirty-six inches.

49. The action at a distance was still further increased by coiling the long wire of the large spool into the form of a ring of four feet in diameter, and placing parallel to this another ring, formed of the four ribands of coils No. 1, 2, 3 and 4. When a current from a single battery of thirty-five feet of zinc surface was passed through the riband conductor, shocks through the tongue were felt when the rings were separated to the distance of four feet. As the conductors were approximated, the shocks became more and more severe; and when at the distance of twelve inches, they could not be taken through the body.

50. It may be stated in this connexion, that the galvanic induction of magnetism in soft iron, in reference to distance, is also surprisingly great. A cylinder of soft iron, two inches in diameter and one foot long, placed in the centre of the ring of copper riband, with the battery above mentioned, becomes strongly magnetic.

51. I may perhaps be excused for mentioning in this communication that the induction at a distance affords the means of exhibiting some of the most astonishing experiments, in the line of *physique amusante*, to be found perhaps in the whole

course of science. I will mention one which is somewhat connected with the experiments to be described in the next section, and which exhibits the action in a striking manner. This consists in causing the induction to take place through the partition wall of two rooms. For this purpose coil No. 1 is suspended against the wall in one room, while a person in the adjoining one receives the shock, by grasping the handles of the helix, and approaching it to the spot opposite to which the coil is suspended. The effect is as if by magic, without a visible cause. It is best produced through a door, or thin wooden partition.

52. The action at a distance affords a simple method of graduating the intensity of the shock in the case of its application to medical purposes. The helix may be suspended by a string passing over a pulley, and then gradually lowered down towards the plane of the coil, until the shocks are of the required intensity. At the request of a medical friend, I have lately administered the induced current precisely in this way, in a case of paralysis of a part of the nerves of the face.

53. I may also mention that the energetic action of the spiral conductors enables us to imitate, in a very striking manner, the inductive operation of the magneto-electrical machine, by means of an uninterrupted galvanic current. For this purpose it is only necessary to arrange two coils to represent the two poles of a horseshoe magnet, and to cause two helices to revolve past them in a parallel plane. While a constant current is passing through each coil, in opposite directions, the effect of the rotation of the helices is precisely the same as that of the revolving armature in the machine.

54. A remarkable fact should here be noted in reference to helix No. 4, which is connected with a subsequent part of the investigation. This helix is formed of copper wire, the spires of which are insulated by a coating of cement instead of thread, as in the case of the others. After being used in the above experiments, a small discharge from a Leyden jar was passed through it, and on applying it again to the coil, I was much surprised to find that scarcely any signs of a secondary current could be obtained.

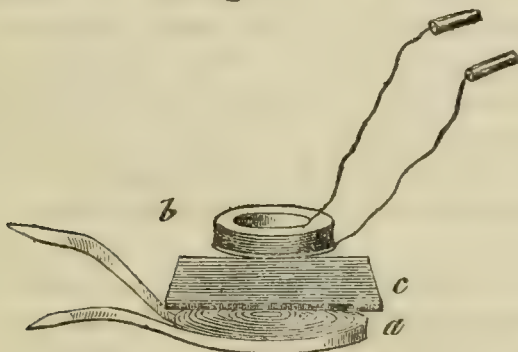
55. The discharge had destroyed the insulation in some part, but this was not sufficient to prevent the magnetizing of a bar of iron introduced into the opening at the centre. The effect appeared to be confined to the inductive action. The same accident had before happened to another coil of nearly the same kind. It was therefore noted as one of some importance. An explanation was afterwards found in a peculiar action of the secondary current.

SECTION IV.—*On the Effects produced by interposing different Substances between the Conductors.*

56. Sir H. Davy found, in magnetizing needles by an electrical discharge, that the effect took place through interposed plates of all substances, conductors and nonconductors*. The experiment which I have given in paragraph 51 would appear to indicate that the inductive action which produces the secondary current might also follow the same law.

57. To test this the compound helix was placed about five inches above coil No. 1, fig. 5, and a plate of sheet iron, about

Fig. 5.



a represents coil No. 1, *b* helix No. 1, and *c* an interposed plate of metal.

$\frac{1}{10}$ th of an inch thick, interposed. With this arrangement no shocks could be obtained; although, when the plate was withdrawn, they were very intense.

58. It was at first thought that this effect might be peculiar to the iron, on account of its temporary magnetism; but this idea was shown to be erroneous by substituting a plate of zinc of about the same size and thickness. With this the screening influence was exhibited as before.

59. After this a variety of substances was interposed in succession, namely, copper, lead, mercury, acid, water, wood, glass, &c.: and it was found that all the perfect conductors, such as the metals, produced the screening influence; but nonconductors, as glass, wood, &c. appeared to have no effect whatever.

60. When the helix was separated from the coil by a distance only equal to the thickness of the plate, a slight sensation could be perceived even when the zinc of $\frac{1}{10}$ th of an inch in thickness was interposed. This effect was increased by increasing the quantity of the battery current. If the thickness of the plate was diminished, the induction through

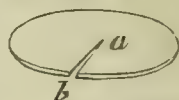
* Philosophical Transactions, 1821. [or Phil. Mag., First Series, vol. lviii.]

it became more intense. Thus a sheet of tinfoil interposed produced no perceptible influence; also four sheets of the same were attended with the same result. A certain thickness of metal is therefore required to produce the screening effect, and this thickness depends on the quantity of the current from the battery.

61. The idea occurred to me that the screening might, in some way, be connected with an instantaneous current in the plate, similar to that in the induction by magnetic rotation, discovered by M. Arago. The ingenious variation of this principle by Messrs. Babbage and Herschel, furnished me with a simple method of determining this point.

62. A circular plate of lead was interposed, which caused the induction in the helix almost entirely to disappear. A slip of the metal was then cut out in the direction of a radius of the circle, as is shown in fig. 6. With the plate in this condition, no screening was produced; the shocks were as intense as if the metal were not present.

Fig. 6.



63. This experiment however is not entirely satisfactory, since the action might have taken place through the opening of the lead; to obviate this objection, another plate was cut in the same manner, and the two interposed with a glass plate between them, and so arranged that the opening in the one might be covered by the continuous part of the other. Still shocks were obtained with undiminished intensity.

a represents a lead plate, of which the sector *b* is cut out.

64. But the existence of a current in the interposed conductor was rendered certain by attaching the magnetizing spiral by means of two wires to the edge of the opening in the circular plate, as is shown in fig. 7. By this arrangement the latent current was drawn out, and its direction obtained by the polarity of a needle placed in the spiral at *b*.

Fig. 7.



65. This current was a secondary one, and its direction, in conformity with the discovery of Dr. Faraday, was found to be the same as that of the primary current.

a represents a lead plate, *b* the magnetizing spiral.

66. That the screening influence is in some way produced by the neutralizing action of the current thus obtained, will be clear, from the following experiment. The plate of zinc before mentioned, which is nearly twice the diameter of the helix, instead of being placed between the conductors, was

put on the top of the helix, and in this position, although the neutralization was not as perfect as before, yet a great reduction was observed in the intensity of the shock.

67. But here a very interesting and puzzling question occurs. How does it happen that two currents, both in the same direction, can neutralize each other? I was at first disposed to consider the phænomenon as a case of real electrical interference, in which the impulses succeed each other by some regular interval. But if this were true the effect should depend on the length and other conditions of the current in the interposed conductor. In order to investigate this, several modifications of the experiments were instituted.

68. First a flat coil (No. 3) was interposed instead of the plates. When the two ends of this were separated, the shocks were received as if the coil were not present; but when the ends were joined, so as to form a perfect metallic circuit, no shocks could be obtained. The neutralization with the coil in this experiment was even more perfect than with the plate.

69. Again, coil No. 2, in the form of a ring, was placed not between the conductors, but around the helix. With this disposition of the apparatus, and the ends of the coil joined, the shocks were scarcely perceptible; but when the ends were separated, the presence of the coil has no effect.

70. Also when helix No. 1 and 2 were together submitted to the influence of coil No. 1, the ends of the one being joined, the other gave no shock.

71. The experiments were further varied by placing helix No. 2 within a hollow cylinder of sheet brass, and this again within coil No. 2 in a manner similar to that shown in fig. 12, which is intended to illustrate another experiment. In this arrangement the neutralizing action was exhibited, as in the case of the plate.

72. A hollow cylinder of iron was next substituted for the one of brass, and with this also no shocks could be obtained.

73. From these experiments it is evident that the neutralization takes place with currents in the interposed or adjoining conductors of all lengths and intensities, and therefore cannot, as it appears to me, be referred to the interference of two systems of vibrations.

74. This part of the investigation was, for a time, given up almost in despair, and it was not until new light had been obtained from another part of the inquiry, that any further advances could be made towards a solution of the mystery.

75. Before proceeding to the next section, I may here state that the phænomenon mentioned, paragraph 54, in reference to helix No. 4, is connected with the neutralizing ac-

tion. The electrical discharge having destroyed the insulation at some point, a part of the spires would thus form a shut circuit, and the induction in this would counteract the action in the other part of the helix; or, in other words, the helix was in the same condition as the two helices mentioned in paragraph 70, when the ends of the wire of one were joined.

76. Also the same principle appears to have an important bearing on the improvement of the magneto-electrical machine; since the plates of metal which sometimes form the ends of the spool containing the wire, must necessarily diminish the action, and also from experiment of paragraph 72 the armature itself may circulate a closed current which will interfere with the intensity of the induction in the surrounding wire. I am inclined to believe that the increased effect observed by Sturgeon and Calland, when a bundle of wire is substituted for a solid piece of iron, is at least in part due to the interruption of these currents. I hope to resume this part of the subject, in connexion with several other points, in another communication to the Society.

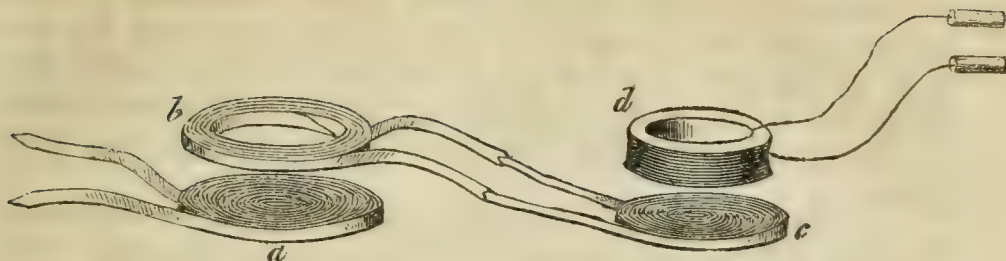
77. The results given in this section may, at first sight, be thought at variance with the statements of Sir H. Davy, that needles could be magnetized by an electrical discharge with conductors interposed. But from his method of performing the experiment, it is evident that the plate of metal was placed between a straight conductor and the needle. The arrangement was therefore similar to the interrupted circuit in the experiment with the cut plate (62.), which produces no screening effect. Had the plate been curved into the form of a hollow cylinder, with the two ends in contact, and the needle placed within this, the effect would have been otherwise.

SECTION V.—*On the Production and Properties of induced Currents of the Third, Fourth, and Fifth Order.*

78. The fact of the perfect neutralization of the primary current by a secondary, in the interposed conductor, led me to conclude that if the latter could be drawn out, or separated from the influence of the former, it would itself be capable of producing a new induced current in a third conductor.

79. The arrangement exhibited in fig. 8 furnishes a ready means of testing this. The primary current, as usual, is passed through coil No. 1, while coil No. 2 is placed over this to receive the induction with its ends joined to those of coil No. 3. By this disposition the secondary current passes through No. 3; and since this is at a distance, and without

Fig. 8.



a coil No. 1, *b* coil No. 2, *c* coil No. 3; *d* helix No. 1.

the influence of the primary, its separate induction will be rendered manifest by the effects on helix No. 1. When the handles *a*, *b* are grasped a powerful shock is received, proving the induction of a tertiary current.

80. By a similar but more extended arrangement, as shown in fig. 9, shocks were received from currents of a fourth and fifth order; and with a more powerful primary current, and additional coils, a still greater number of successive inductions might be obtained.

81. The induction of currents of different orders, of sufficient intensity to give shocks, could scarcely have been anticipated from our previous knowledge of the subject. The secondary current consists, as it were, of a single wave of the natural electricity of the wire, disturbed but for an instant by the induction of the primary; yet this has the power of inducing another current, but little inferior in energy to itself, and thus produces effects apparently much greater in proportion to the quantity of electricity in motion than the primary current.

82. Some difference may be conceived to exist in the action of the induced currents, and that from the battery, since they are apparently different in nature; the one consisting, as we may suppose, of a single impulse; and the other of a succession of such impulses, or a continuous action. It was therefore important to investigate the properties of these currents, and to compare the results with those before obtained.

83. First, in reference to the intensity, it was found that with the small battery a shock could be given from the current of the third order to twenty-five persons joining hands; also shocks perceptible in the arms were obtained from a current of the fifth order.

84. The action at a distance was also much greater than could have been anticipated. In one experiment shocks from the tertiary current were distinctly felt through the tongue,

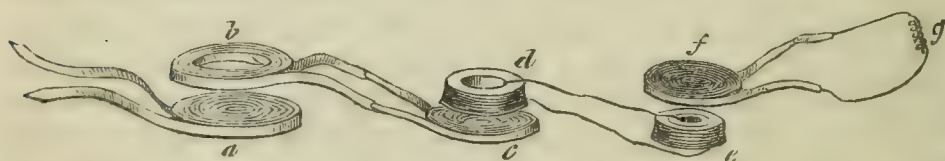
when helix No. 1 was at the distance of eighteen inches above the coil transmitting the secondary current.

85. The same screening effects were produced by the interposition of plates of metal between the conductors of the different orders, as those which have been described in reference to the primary and secondary currents.

86. Also when the long helix is placed over a secondary current generated in a short coil, and which is therefore, as we have before shown, one of quantity, a tertiary current of intensity is produced.

87. Again, when the intensity current of the last experiment is passed through a second helix, and another coil is placed over this, a quantity current is again produced. Therefore in the case of these currents, as in that of the primary, a *quantity current can be induced from one of intensity, and the converse*. By the arrangement of the apparatus as shown in fig. 9, these different results are exhibited at once. The induction from coil No. 3 to helix No. 1 produces an intensity current, and from helix No. 2 to coil No. 4 a quantity current.

Fig. 9.



a coil No. 1, *b* coil No. 2, *c* coil No. 3, *d* helix No. 1, *e* helix No. 2 and 3, *f* coil No. 4, and *g* magnetizing spiral.

88. If the ends of coil No. 2, as in the arrangement of fig. 8, be united to helix No. 1 instead of coil No. 3, no shocks can be obtained; the quantity current of coil No. 2 appears not to be of sufficient intensity to pass through the wire of the long helix.

89. Also, no shocks can be obtained from the handles attached to helix No. 2, in the arrangement exhibited in fig. 10. In this case the quantity of electricity in the current from the helix appears to be too small to produce any effect, unless its power is multiplied by passing it through a conductor of many spires.

90. The next inquiry was in reference to the direction of these currents, and this appeared important in connexion with the nature of the action. The experiments of Dr. Faraday would render it probable, that at the beginning and

Fig. 10.



a coil No. 2, *b* helix No. 1, *c* coil No. 3, and *d* helix No. 2.

ending of the secondary current, its induction on an adjacent wire is in contrary directions, as is shown to be the case in the primary current. But the whole action of a secondary current is so instantaneous that the inductive effects at the beginning and ending cannot be distinguished from each other, and we can only observe a single impulse, which, however, may be considered as the difference of two impulses in opposite directions.

91. The first experiment happened to be made with a current of the fourth order. The magnetizing spiral (11.) was attached to the ends of coil No. 4, fig. 9, and by the polarity of the needle it was found that this current was in the same direction with the secondary and primary currents*. By a too hasty generalization, I was led to conclude, from this experiment, that the currents of all orders are in the same direction as that of the battery current, and I was the more confirmed in this from the results of my first experiments on the currents of ordinary electricity. The conclusion, however, caused me much useless labour and perplexity, and was afterwards proved to be erroneous.

92. By a careful repetition of the last experiment, in reference to each current, the important fact was discovered, that *there exists an alternation in the direction of the currents of the several orders commencing with the secondary.* This result was so extraordinary, that it was thought necessary to establish it by a variety of experiments. For this purpose the direction was determined by decomposition, and also by the galvanometer, but the result was still the same; and at this stage of the inquiry I was compelled to the conclusion that the directions of the several currents were as follows:

Primary current	+
Secondary current	+

* It should be recollected that all the inductions which have been mentioned were produced at the moment of breaking the circuit of the battery current. The induction at the formation of the current is too feeble to produce the effects described.

Current of the third order	. . .	—
Current of the fourth order	. . .	+
Current of the fifth order	. . .	—

93. In the first glance at the above table, we are struck with the fact that the law of alternation is complete, except between the primary and secondary currents, and it appeared that this exception might possibly be connected with the induced current which takes place in the first coil itself, and which gives rise to the phænomena of the spiral conductor. If this should be found to be *minus*, we might consider it as existing between the primary and secondary, and the anomaly would thus disappear. Arrangements were therefore made to fully satisfy myself on this point. For this purpose the decomposition of dilute acid and the use of the galvanometer were resorted to, by placing the apparatus between the ends of a cross wire attached to the extremities of the coil, as in the arrangement described by Dr. Faraday (ninth series): but all the results persisted in giving a direction to this current the same as stated by Dr. Faraday, namely, that of the primary current. I was therefore obliged to abandon the supposition that the anomaly in the change of the current is connected with the induction of the battery current on itself.

94. Whatever may be the nature or causes of these changes in the direction, they offer a ready explanation of the neutralizing action of the plate interposed between two conductors, since a secondary current is induced in the plate; and although the action of this, as has been shown, is in the same direction as the current from the battery, yet it tends to induce a current in the adjacent conducting matter of a contrary direction. The same explanation is also applicable to all the other cases of neutralization, even to those which take place between the conductors of the several orders of currents.

95. The same principle explains some effects noted in reference to the induction of a current on itself. If a flat coil be connected with the battery, of course sparks will be produced by the induction, at each rupture of the circuit. But if in this condition another flat coil, with its ends joined, be placed on the first coil, the intensity of the shock is much diminished, and when the several spires of the two coils are mutually interposed by winding the two ribands together into one coil, the sparks entirely disappear in the coil transmitting the battery current, when the ends of the other are joined. To understand this, it is only necessary to mention that the induced current in the first coil is a true secondary current, and it is therefore neutralized by the action of the secondary

in the adjoining conductor; since this tends to produce a current in the opposite direction.

96. It would also appear from the perfect neutralization which ensues in the arrangement of the last paragraph, that the induced current in the adjoining conductor is more powerful than that of the first conductor; and we can easily see how this may be. The two ends of the second coil are joined, and it thus forms a perfect metallic circuit; while the circuit of the other coil may be considered as partially interrupted, since to render the spark visible the electricity must be projected, as it were, through a small distance of air.

97. We would also infer that two contiguous secondary currents, produced by the same induction, would partially counteract each other. Moving in the same direction, they would each tend to induce a current in the other of an opposite direction. This is illustrated by the following experiment: helix No. 1 and 2 were placed together, but not united, above coil No. 1, so that they each might receive the induction; the larger was then gradually removed to a greater distance from the coil, until the intensity of the shock from each was about the same. When the ends of the two were united, so that the shock would pass through the body from the two together, the effect was apparently less than with one helix alone. The result, however, was not as satisfactory as in the case of the other experiments; a slight difference in the intensity of two shocks could not be appreciated with perfect certainty.

[To be continued.]

XLVII. *On the Natural Products which originate from the action of the Atmosphere on Iron Pyrites.* By TH. SCHEERER*.

IT is a well-known fact, that iron pyrites, in the finely divided state in which it occurs in alum slates, is easily oxidized by the atmosphere, causing the parts exposed to acquire a reddish brown colour; nevertheless, the products of this decomposition are seldom to be found, from the rain washing them away. In a spot near Modum in Norway, I met with a cavity in the mountains where they were deposited as incrustations, safe from all destructive influences. Three distinct layers were evident.

The first and upper layer is a dark brown massive mineral with which the slate is impregnated: A.

* An extract obligingly communicated by the author from the original paper published in Poggendorff's *Annalen*, vol. xlv. p. 188.

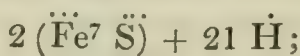
The second layer forms a light yellow mass distinctly separated from the first, and forming incrustations similar to those which occur in (dolomitic) limestone caverns: B.

The third is clothed with a layer of small white crystals: C.

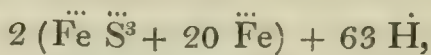
The analysis of A gave:

80.73	peroxide of iron
6.00	sulphuric acid
13.57	water
<hr/>	
100.30	

which corresponds to a combination of 14 atoms of peroxide of iron, 2 atoms of sulphuric acid, and 21 atoms of water, which is expressed by the formula,



or,

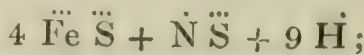


according to the manner in which the formula is written. This iron-salt may be called, after the nomenclature of Berzelius, the twenty-fold basic sulphate of the peroxide of iron; it is the most basic salt as yet known. The oxygen of the oxide amounts to the double of that in the water. It is perfectly insoluble in water.

Two analyses of the substance B gave the following results:

1	2
49.37	49.89 peroxide of iron
32.42	32.47 sulphuric acid
5.03	5.37 soda
13.13	13.09 water
<hr/>	
99.95	100.82

The soda was found in both analyses to contain a small quantity of potash, which, however, is of no importance for the formula, which may be thus expressed,



namely 4 atoms of peroxide of iron, 5 atoms of sulphuric acid, 1 atom of soda, and 9 atoms of water.

The substance C was found to be pure gypsum.

In explaining the commencement and continuation of this process, it must be supposed that the sulphate of the protoxide of iron was at first formed by the oxidation of the iron pyrites; this became gradually oxidized, and was deposited

as the first brown layer of the salt A described. Yet the deposition of this basic iron-salt must have happened under singular circumstances, for it is a well-known fact that a solution of iron-vitriol, oxidized by the atmosphere, is precipitated as a *five-fold* basic salt. It is likewise difficult to explain how the yellow layer containing alkali *suddenly* succeeded the dark-brown; it may indeed be supposed, that at the commencement of this decomposition of the iron pyrites the alum-slate resisted for some time all action, until it was attacked, and its alkali dissolved by the sulphuric acid, which commenced the formation of a new salt. But if this mode of explanation has much appearance of probability, the *sudden* cessation of the one product of decomposition, and the commencement of the second, is a strange fact. That the gypsum, as the more easily soluble substance, is found on the inferior part of the ceiling of the cavern is, on the other hand, easily conceived. The lime in it undoubtedly acted no unimportant part at the deposition of the iron salts described, aiding in their precipitation by saturating the acid.

XLVIII. *Experiments and Observations on Light which has permeated coloured Media, and on the Chemical Action of the Solar Spectrum.* By ROBERT HUNT.*

M. GAY-LUSSAC, when speaking of the beautiful discovery of M. Daguerre, said, "The palette of the painter is not very rich in colour, black and white compose the whole. The image in its natural and varied colours may remain long, perhaps for ever, a thing hidden from human sagacity †."

However, the production of a coloured picture of the spectrum by Sir John Herschel, and some effects produced by Mr. Talbot, together with some delicate tinting which I observed, when, during the summer of 1839, I was engaged in copying some flowers of Nature's richest painting, led me to think coloured photographs within the range of probabilities, and induced me to pursue a train of experiments from which, although little has resulted to heighten my first hopes, I have gathered much that is curious and certainly instructive.

Photographic Papers.

1. By saturating paper with different chlorides and mu-

* Communicated by the Author.

† "The History and Practice of Photogenic Drawing, &c., by L. J. M. Daguerre. Translated by J. S. Memes, LL.D."

riates, always keeping in view the definite proportion required for the quantity of the nitrate of silver used; it will be found that almost every variety of shade, from a rich dark purple to a full red, and a few other tints, may be produced at pleasure.

2. The effects of light, passing through coloured glasses on various papers, are singularly diversified. The following are a few of the most striking results. (The glasses are, a deep cobalt blue, a full laurel green, an amber yellow, and a rich orange red. They are so framed that all the papers can be exposed at the same time to the solar influence.)

Colour of Glass.

Blue. Green. Yellow. Red.

Salt used.	Effects produced.			
a. Chlor. of sodium.	Purple.	Blue.	Violet.	Chocolate.
b. Chlor. of potassa.	Light purple.	Sky blue.	Light violet.	Tinted red.
c. Muriate of lime.	Rich violet.	Faint blue.	Blue.	Reddish.
d. Muriate of iron.	Red.	Colourless.	Faint red.	Leaden hue.
e. Mur. of peroxide of iron.	Blue.	Yellowish.	Straw color.	Yellow brown.
f. Mur. of baryta ...	Purple red.	Lilac.	Chocolate.	Pink.
g. Muriate of manganese.....	Rich browu.	Reddish.	Rose hue.	Yellow.
h. Mur. of ammonia	Olive brown.	Pale brown.	Brown.	Dull orange.

3. I have found but a modified action from the interference of coloured fluids. In a few instances, under a solution of carmine in ammonia, I have obtained the richest crimson dye; but I cannot, by any means I have used, succeed in fixing the colour on the paper.

4. A paper prepared, by first washing it with a solution of twelve grains of the iodide of potassium in one ounce of water, and then with a solution of ten grains of the crystallized nitrate of silver in the same quantity of fluid, is very sensitive. When exposed beneath a solution of the ammonia-sulphate of copper to sunshine, it changes to a rich *light blue*. Acetate of copper produces a *brown*. Muriate of the peroxide of iron imparts a *green tinge*, and solutions of carmine a *brown red*.

5. The paper *f* becomes *red*, when acted on by rays passing through nitrous acid gas, and is tinged *yellow*, by the light which has been subjected to the interference of chlorine and its protoxide.

6. To have as full a volume as possible of iodine and bromine vapour, carefully closed vessels containing a small portion of these bodies, were placed upon a plate of copper warmed by water.

The paper *h* was laid beneath them, and exposed to luminous influence. Under the bromine it was unchanged, but

beneath the iodine the paper became richly iridescent. The colours changed to a uniform violet tint upon a few minutes' exposure to direct sunshine.

7. Papers already darkened by sunlight during prolonged exposure to the influence of the dissevered rays of the spectrum, assume a variety of colours. The same changes may be effected by carefully arranging glasses, and placing the photographic preparations beneath them. I shall copy exactly the memoranda of my journal.

Dec. 12, 1839.—I placed under blue, green, yellow, and red glass the following papers :

A. *Muriate of ammonia*, with two washings of solution of the nitrate of silver, darkened by exposure to a rich chocolate.

B. *Muriate of manganese*. Silver, two washings, darkened to a full brown.

C. *Iodide of potassium*. Silver, one washing, darkened to a yellow brown.

D. *Iodide of potassium* and silver, two washings, darkened to a red brown.

E. *Chloriodic acid*. Silver two washings, darkened to a rich bronze.

F. *Chloriodic acid* with *Liquor potassæ*. Silver, two washings, darkened to a blue-brown.

Dec. 13.—After twelve hours exposure to the dull light of rainy weather, the paper E has become blue under the blue glass. No change is apparent on the others.

Dec. 27. Colours of Glass.

	Blue.	Green.	Yellow.	Red.
A. has become	Olive.	Deep green.	Dirty yellow.	Red.
B.	Deep brown.	Bat colour.	Blue brown.	Red.
C.	Do.	Darkened.	No change.	Red brown.
D.	Black.	Light brown.	Rich brown.	Brick red.
E.	Blue black.	Darkened.	Darkened.	Dusky red.
F.	Black brown.	Dull plum.	Bluish.	Reddened.

Jan. 2, 1840.—All the papers go on increasing the distinctness of their colours, except E and F, which have assumed different shades of blackness.

(E and F were removed, and a paper G, prepared with *muriate of baryta* and two washings of silver, darkened to a chocolate, substituted.)

Feb. 7. Colours of Glass.

	Blue.	Green.	Yellow.	Red.
A.	Rich olive.	Green.	Yellow.	Purple.
B.	Black.	Chocolate.	Light brown.	Red.
C.	Do.	Red brown.	Do.	Brown.
D.	Chocolate.	Umber brown.	Black.	Red brown.
G.	Bright olive.	Yellow brown.	Pale olive.	Reddish.

The two papers A and G exhibit much more sensitiveness to luminous influence than any others I have yet tried.

8. The paper A, when washed with a weak solution of the hydriodate of baryta, gives under the pencil of light a beautiful picture, whether used in the camera or for surface drawings.

These pictures exhibit the peculiarities mentioned by Mr. Talbot at the British Association*. Sunshine changes "the colour of the object delineated from reddish to black with great rapidity." This gentleman adds, "after which no further change occurs." I much regret I have not been fortunate enough to succeed thus far in fixing my drawings. The continued influence of light in a few months obliterates the impression.

A singular change follows the exposures of these pictures to coloured light.

If placed under vessels containing coloured fluids (4.) and exposed either to sunshine or to diffused light, in a few days the picture becomes a full *red* under the blue; a *rose hue* under the green; a *light blue* under the yellow, and a *deep blue* under the red. These colours after deepening for some time gradually change to different shades of *green* under the blue and green fluids, to a *pink* under the yellow, and a *red* under the red fluid (25.). After this, the colours alter no more, and the picture bears exposure to light much better than at first; but I doubt if it is rendered perfectly permanent, for the dull light of January and February has spread a downiness, like a mist, over those photographs which have been constantly exposed.

Daguerreotypes.

9. Exposing a plate, over which some lace was carefully placed, under four coloured glasses (2.) for three minutes to diffused light, I obtained, under the blue glass a beautiful copy; no trace of a drawing beneath the green; a tolerable impression beneath the yellow; but the mercury would not attack the space beneath the red.

10. A plate similarly arranged beneath four bottles of coloured fluid (4.) exposed to diffused light for fifteen minutes, was found on being acted upon by the mercurial vapour to present the same appearance as above (9.), excepting that a faint design was evident over the space the carmine fluid had covered.

11. I arranged a dark chamber, to which no other rays could pass but such as had permeated *two inches* of coloured fluid.

* Athenæum, No. 618.

Having filled my trough with a saturated solution of the bichromate of potassa, I exposed a plate for five minutes to its influence in full sunshine. *There was not the slightest action.*

12. In one hour on a similar plate, under the same circumstances, I obtained a faint, but still defined outline of a dried fern.

13. I exposed a bare iodidated plate for two hours to the same influence. On removing it from the chamber no difference was apparent; but I found it was no longer sensitive to light, and the iodide adhered more closely to the metal than it did (28.).

This is a reverse action, for after the exposure of a prepared daguerreotype plate to light, the sensitive film is most easily rubbed off* (28.).

14. Red solutions impart a very decided rose hue, or more strictly speaking the influence of red light on the iodidated plate occasions that peculiar arrangement of the mercurial particles, which is necessary to the production of red colour.

15. Green solutions act with more or less effect in obstructing the passage of the so-called chemical rays according to their depth of colour. But in no instance have I found them to produce that close combination, which the yellow and sometimes the red fluids do, of the iodide and the under surface of unattacked silver (28.). By examining the effects produced by green media (2, 7, 16.) a peculiar order of interference will be remarked (19.).

Germination and the growth of Plants.

16. I planted in a box some curled cress seed, and so arranged bottles of carmine fluid, chromate of potassa, acetate of copper, and the ammonia sulphate, that all but a small space of the earth was exposed to light which had permeated three-fourths of an inch of these media.

For some days the only apparent difference was that the earth continued damp under the green and blue fluids, whereas it rapidly dried under the red and yellow. The plumula burst

* On this principle I now polish my silvered plates, by which the troublesome process with nitric acid and punice is got rid of. I wash the surface of silver over with a solution of the iodide of potassium holding a little iodine free, and rub it lightly until all the parts are equally attacked. I then expose the plate to light for a few minutes, and polish off with dry cotton. In five minutes by this process the most perfect lustre may be given to the silver, and it has the advantage of rendering the plate more susceptible to the influence of the iodine vapour.

the cuticle in the blue and green lights, before any change was evident in the other parts.

After ten days, under the blue fluid there was a crop of cress, of as bright a green as any which grew in full light, and *far more abundant*.

The crop was scanty under the green fluid and of a pale unhealthy colour (15.).

Under the yellow solution but two or three plants appeared, yet they were less pale than those which had grown in green light. Beneath the red bottle the number of plants which grew was also small, although rather more than in the spot the yellow covered. They too were of an unhealthy colour.

17. I now reversed the order of the bottles, fixing the red in the place of the blue, and the yellow in that of the green. After a few days' exposure the healthy cress appeared blighted, while a few more unhealthy plants began to show themselves, from the influence of the blue rays, in the spot originally subjected to the red.

It is evident from this that the red and yellow rays not merely retard germination, but positively destroy the vital principle in the seed. Prolonged exposure uncovered, with genial warmth, free air, and indeed all that can induce growth, fails to revive the blighted vegetation.

I have repeated the experiment many times, varying the fluids, but the results have been the same. At this time I have the above facts strikingly exemplified where the space covered by the bichromate of potassa is without a plant.

These results merit the attention of those who are engaged in the study of vegetable œconomy. Do they not point at a process by which the productions of climes more redolent of light than ours may be brought in this island to their native perfection?

Dr. Draper's "experiments" (Philosophical Magazine, Feb. 1840, pres. vol. p. 81) appear at variance with mine.

Under the influence of a nearly tropical sun permeating half an inch of solution of the bichromate of potassa, cress grew of a green colour, whilst it took five days to give a sensitive paper a faint yellow green colour. From this Professor Draper argues the existence of two classes of rays, a different class being necessary to produce the green colouring of vegetable foliage from that which darkens chloride of silver.

With submission to one whose facilities for such inquiries are so much greater than my own, I would suggest a repetition of the experiments with some of the recently discovered photographic preparations. The papers *f* and *h*, both under

coloured glass and great thicknesses of yellow fluid are deepened to a plum-brown in less than an hour *.

Under three inches of the bichromate of potassa the paper, *f*, became in eight hours sunshine of a full blue-brown.

18. The fact of cress and pea plants growing green, under the influence of such powerful light as penetrated Professor Draper's yellow media, will not appear at all surprising when we examine the rays which pass through such fluids.

This I have done by forming a spectrum, interposing the coloured body between the prism and the sun. The following are the effects of a February sun at Devonport.

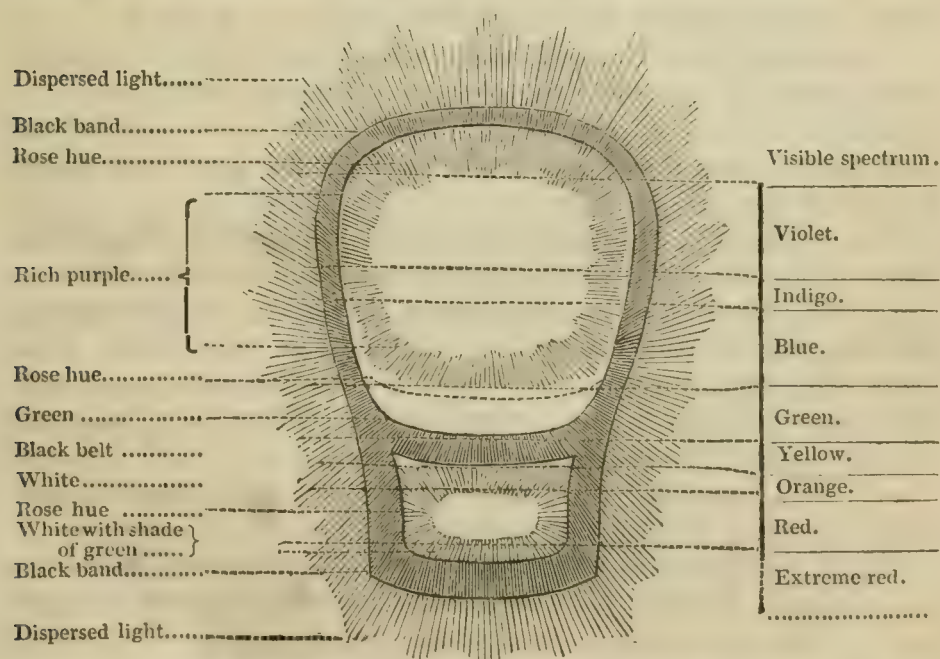
Through a deep blue solution of the ammonia-sulphate of copper, the violet, indigo, blue, and a portion of the green rays pass.

Through solutions of the muriate, acetate and nitro-muriate of copper with iron, the green ray, and a considerable portion of the yellow; a trace of the blue also is evident.

Through solutions of the bichromate and chromate of potassa, the chloride of gold and decoction of turmeric, the red, the yellow and the green rays are seen, *and by taking their impression on a daguerreotype plate a line of the blue is distinctly marked.*

Through nitro-muriate of cobalt in ammonia, carmine in ammonia, and sulphuric acid and decoction of cochineal, the red and yellow rays alone appear to penetrate.

THE SPECTRUM.



* The papers which accompany this article were exposed under the glasses and three-fourths of an inch of fluids for forty minutes. The order of interference and consequent colouring is plainly shown.

19. It will be observed, that the light which has passed through a green medium (2, 7, 9, 10, 15, 16) acts less powerfully in darkening photographic papers, and occasions vegetable leaves to be even paler than that which has been subjected to the interference of a yellow medium.

I am led to suspect that the band of rays formed by the meeting of the yellow and the green has an influence similar to the extreme red, in neutralizing the powers of the other adjacent rays, as was first noticed by Sir John Herschel, (22.), (23.), (26.).

20. The figure on the preceding page represents the solar spectrum, as it impresses itself on a daguerreotype plate, not in shadows merely, but in colours, which have the peculiar appearance of the down upon the nectarine.

The most refrangible portion of the spectrum is represented in full colours, shading from indigo to a delicate rose, which is lost in a band of pure white.

21. Beyond this a protecting influence is powerfully exerted, and notwithstanding the chemical effect produced over the plate, by the dispersed light, a line is formed free of mercurial vapour, and which consequently appears black.

22. The green portion of the spectrum is represented in its true colour, but it is considerably less in size than the space occupied by these rays.

23. The yellow rays are without action, or rather they do not prepare the silver for the reception of the mercury, and consequently a black belt marks the space on which they fell, and extends a little beyond it into the green (19.).

24. A white line marks the place of the orange light.

25. The red is represented by a well-defined rose colour, bounded, as were the more refrangible rays, by a white line, shaded at the lower extremity with a green.

This passing of the red into a green and of the blue into a rose colour (20.) is strikingly similar to the effect produced, by the interference of coloured media, on some photographic drawings (8.).

26. The lowest dark space on the picture is a beautiful illustration of the influence of the extreme red rays in protecting the silver from luminous action (19.) (21.).

27. What appears more surprising to me than even the detection of the *negative*? rays at each end of the prismatic spectrum, is the continuation of the dark line throughout *its whole length*, evidently showing the influence of the same cause as is so effective at the least refrangible extremity.

This band is not equally defined throughout its entire circumference. It is the most strikingly evident from the ex-

treme red to the green; it fades in passing through the blue and increases in intensity as it leaves the indigo, until, beyond the invisible chemical rays, it is nearly as strong as it is at the calorific end of the spectrum.

Does not this protected surrounding band appear to indicate the existence of rays of a peculiar and unknown order, proceeding from the extreme edge of the sun?

28. By lightly rubbing a daguerreotype picture of the prismatic rays, it is obliterated, except over the space of the yellow and red portion. This effect corresponds with my experiments on media of these colours (11. 12. 13).

Until we have more experience than we now have of the effects of the solar rays individually and collectively, we can offer no satisfactory explanation of the process in action, on a daguerreotype plate, by which the subtle painter LIGHT impresses such delicate designs.

The existence of two iodides of silver, is, I think, certain. In my photometric experiments I have always observed the formation of an iodide which speedily darkens, and of another portion which is unalterable by light*.

The sensitive film on the silver plate appears to be the former of these iodides. Throughout the range of the chemical spectrum, *particularly so called*, the iodide is I imagine converted into an oxide of silver; that a partial oxidation takes place numerous experiments have rendered certain; whilst the influence of the rays of least refrangibility is to form the unchangeable iodide of silver. Experiments, however, are wanting to prove this satisfactorily.

An attentive consideration of the facts I have enumerated, will, I think, satisfy all, that we can no longer with propriety attach the name of chemical to the most refrangible rays only. Every ray has its particular chemical office, either of composition or of decomposition; and although Seebeck has attributed the acquirement of a rose hue by chloride of silver when put into the red ray, to the heating power of that portion of the spectrum, it is now proved to be dependent upon some other influence, for where it has been shown the most calorific rays exist this salt undergoes no change.

Devonport, February 29, 1840.

* [See Mr. Talbot's account of the processes employed in Photogenic Drawing, Lond. and Edinb. Phil. Mag., vol. xiv. p. 210 (2).—EDIT.]

XLIX. *On the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c.* By THOMAS WEAVER, Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.

“Men believe that reason governs their words, but words have often power enough to react upon reason.”—*Bacon*.

THE following remarks, bearing on the mineral structure of a considerable portion of the south of Ireland, are drawn from me by certain representations made by Mr. Griffith on the same subject, respecting which my first impression was that I might be content to suffer them to pass without public comment, persuaded that no intelligent geologist, who would duly sift and weigh the evidence adduced on both sides, could be at a loss in coming to a correct decision. But as it is not every reader who will take this trouble, and as unquestioned assertions not unfrequently pass as established truths with the unwary, further reflection has taught me that I owe it both to the Geological Society and to myself not to remain silent. However adverse to controversy, and however irksome the task of entering upon it, a man should always be ready to give a reason for the faith that is in him.

It has so happened that my Memoir on the Geological Relations of the south of Ireland* (which in its more material points was communicated to the Geological Society in the year 1830), and Mr. Griffith's Outline of the Geology of Ireland, with its accompanying Geological Map†, were brought before the public eye much about the same time, namely, in the early part of 1838. The marked discrepancies observable not only between our respective maps of the south of Ireland, but also in Mr. Griffith's own map itself, could not fail to strike other geologists as well as myself; while, on referring to the written outline, so far from finding an elucidation of the discordances, the outline itself appeared at variance with the map. I have reason to know that other geologists felt equally unable to discover the precise purport and extent of Mr. Griffith's meaning, and hence some explanation seemed necessary. Doubtless, aware of such an impression, Mr. Griffith has been led to give that explanation, accompanied by many changes in his views: 1st, in a communication made to the British Association at Newcastle, in August, 1838‡; 2nd, in a letter addressed to the Rev. Dr. Buckland, Pres. Geol.

* Geol. Trans., vol. v., second series, 1837.

† Appendix to the Report of the Irish Railway Commissioners, 1838.

‡ Report of the Eighth Meeting of the British Association, 1839.

Soc., in May, 1839*; and, 3rd, in a paper read before the Geological Society of Dublin, on the 13th June, 1839, the last-mentioned being accompanied by two sections, one referring to the south-eastern portion of the island, and the other to a part of the extreme west, in the county of Kerry†. The views of Mr. Griffith thus appearing in an authenticated form before the public, and the author having in his last production, while attempting to explain his own positions, found it necessary to assail mine, the time has fully arrived for adverting to those views. Being thus put on my defence, I shall proceed to consider, in connexion, the Outline with its Geological Map, and the three later written communications, with the two sections supplied by the author; and in so doing, I shall feel no difficulty in showing that many of Mr. Griffith's representations are not only irreconcilable with the facts, but that those representations contradict each other‡.

The question at issue lies more particularly between the older stratified rocks of the south of Ireland and the old red sandstone properly so called, which occurs in different portions in that quarter of the island.

In the Outline (at p. 7), Mr. Griffith professes to distinguish the older stratified rocks as consisting of an older and a newer transition series, the latter, which is coloured purple, being said usually to repose unconformably on the former, which is coloured grey; or, as it is later expressed in another place§, usually resting unconformably on the greywacke slate or Silurian series; and the old red sandstone, which is said to succeed, being distinguished partly by a reddish-brown and

* Proceedings of the Geological Society, 22nd May, 1839.

† Journal of the Geological Society of Dublin, vol. ii. part 1., 1839.

‡ This paper was drawn up before I had seen another Geological Map of Ireland put forth by the author, on a large scale, in 1839^a; and to this latter map, it appears, Mr. Griffith's two last-mentioned communications refer also in part. If the discrepancies in the map appended to the "Outline" were startling, the numerous arbitrary alterations introduced in the new map are no less striking; and it may fairly be inquired what reliance is to be placed on either of them, disagreeing largely as they do with each other. I can perceive, in the new map, an approximation in some parts to my own views, but an utter discordance in other parts. As, however, the alterations which have been made in the new map do not materially interfere with the course of my argument, which in the first instance bears directly on the map attached to the "Outline," I have left the text unchanged, merely adding incidentally a few notes in reference to the new map, for the purpose of continuing the comparison. The discrepancies between Mr. Griffith's two maps and my own map of the south of Ireland, will thus become doubly apparent.

§ Journal of Geol. Soc. of Dublin, vol. ii. p. 85.

^a Hodges and Smith, Dublin, and James Gardner, London.

partly by a yellowish-brown colour. Such is the statement. Now, how are these positions established?

Mr. Griffith admits* the correctness of the view, which I gave more than twenty years since†, of the relative position of the old red sandstone, as occurring in detached portions in the county of Wexford, on both banks of Waterford harbour up to the confluence of the Barrow and the Suire, and again along the valley of the latter river west of Waterford, flanking both its sides, that on the north extending into the counties of Tipperary and Kilkenny, and that on the south bordering the river and constituting higher up the Monavoullagh group; in all these cases reposing unconformably on the older stratified rocks. They are thus all placed in the same class as the old red sandstone formation: and this being the case, it may be asked, why is the reddish-brown colour, as indicative of old red sandstone, confined alone to those portions of it which occur in the county of Wexford, extending to the eastern side of Waterford harbour, while on the opposite or western side of that harbour and of the river Barrow, all the other districts specified, and acknowledged to be old red sandstone, are coloured purple, as indicative of a newer transition series? Here is a direct contradiction, both in colours and terms, as well as in the reputed order of succession.

The author having thus in the first place admitted that the old red sandstone of the Monavoullagh range is of the same age as that of the valley of the Suire, of the Barrow at its confluence, and on the eastern and western confines of Waterford harbour, &c., proceeds next to show that it belongs to a newer transition series: and how is this accomplished? by representing the old red sandstone of the Monavoullagh range, the stratified structure of which is admitted to vary only a few degrees from the horizontal‡, as dipping in its southern prolongation suddenly to the south at a high angle, and extending down to the coast, namely, to Ballyvoil Head; thus meaning to identify the old red sandstone of the Monavoullagh with the sandstone conglomerate, sandstone, and red slate which occur on that part of the coast in association and interstratified with transition rocks at a high angle. Here, doubtless, lies the main source of Mr. Griffith's misconception, and the consequent train of errors and inconsis-

* Journal of Geol. Soc. of Dublin, vol. ii. pp. 85, 86.

† Geol. Trans., vol. v., first series, part I, 1819. Memoir on the East of Ireland.

‡ Eighth Report of the British Association—Transactions of the Sections, p. 82.

encies into which it has led him; and on the faith of which he ventures to state* that, “had Mr. Weaver, who represented the conglomerate of Monavoullagh as a mountain cap resting on greywacke slate, made a careful section of the strata, either from Monavoullagh or Ballyvoil Head, he would have been convinced of his error, and probably have arrived at the same conclusion as mine;” a singular conclusion certainly, inasmuch as there is no apparent connexion whatever between the horizontal sandstone conglomerate of the Monavoullagh range and those beds of conglomerate, sandstone, and red slate of the coast which extend eastward from the vale of Dungarvan in several separate discontinuous bands interstratified with other transition rocks; all these dipping throughout at a high angle, chiefly to the south, but also to the north, which latter position may be seen in the red sandstone conglomerate and the associated rocks in Tranamoe head, within a short distance of Bonmahon river. I have described elsewhere that these rocks of the coast are connected on the north and west with varieties of clayslate (black, blue, green, yellow, red, and purple), alternating with greywacke, quartz rock, hornstone, red sandstone, and conglomerate, and comprising also subordinate beds of greenstone with porphyritic varieties of the rocks which I have enumerated†. This statement was not lightly given, having carefully examined the interior in many directions, as well as the whole line of coast extending from Dungarvan harbour on the west to Waterford harbour on the east; and having in the course of my researches (in 1824) discovered transition fossils in the series, I had the greater pleasure in exploring the district, and in ascertaining with exactness the composition and structure of the rocks, as I had just completed and published my account of the Tortworth transition district in Gloucestershire‡.

But it is not necessary to rely on my own testimony alone. The interesting remarks of Mr. Holdsworth§ on the eastern part of the county of Waterford, extending from the Bonmahon coast to the Monavoullagh range on the west, illustrated by a map, come in aid of my views; and they may be considered the more valuable as proceeding from an unbiassed observer. And had Mr. Griffith fully attended to them,

* Journal of Geol. Soc. of Dublin, vol. ii. p. 86.

† Geol. Trans., vol. v., second series—Memoirs on the south of Ireland. §§ 15 to 20 inclusive.

‡ Geol. Trans., vol. i., second series, 1824.

§ Journal of Geol. Soc. of Dublin, vol. i., part 2. 1834.

(putting my own descriptions out of the question), he must have seen that Mr. Holdsworth places in the map and speaks in the text of the clayslate formation, as being observable in different places along the eastern border of the Monavoullagh range of conglomerate; and especially as being of considerable extent in the vicinity of Stradbally, where it is in a highly-inclined position, and in places nearly vertical, stretching thence to the westward toward the vale of Dungarvan, and there coming in contact with the carboniferous limestone of that valley. In its range, therefore, the clayslate formation evidently occupies the district lying between Ballyvoil head on the south and the Monavoullagh range of conglomerate on the north; and Mr. Holdsworth observes*, that in this part the base of the Monavoullagh mountain range approaches close upon the clayslate formation. With respect to the Monavoullagh conglomerate itself, he remarks†, that having examined this mountain range in several places along its line from north to south (which extends many miles), and from top to bottom, it nowhere appeared as a conformable rock. Now, let us return to the coast, stretching eastward from Ballyvoil Head. What is the purport of Mr. Holdsworth's observations in this quarter, and more inland, in reference to the occurrence of red sandstone conglomerate and red slate as forming beds included in the clayslate formation? He says, that in various places along this line of coast, and particularly around Bonmahon, he met with the conglomerate usually accompanied by red micaceous slate, both being in an inclined position, and having the appearance of being a conformable formation, inasmuch as these strata, where they occur, appear bounded on each side by the common rocks of the coast. And the conglomerate is described as being both coarse and fine-grained, the latter passing by insensible gradations into the red sandstone slate; and a coarse sandstone with ferruginous marks also occurring. The red sandstone slate passes, he says, sometimes into grey slate, portions of which occur abundantly in the composition of the conglomerate of the Monavoullagh range. Mr. Holdsworth, in concluding, touches upon the question as to the source and origin of the conglomerate range of the Monavoullagh and those beds of conglomerate which are conformably associated with the common rocks of the coast and in the interior, which latter he also terms, "detached dyke-like masses;" and he inquires whether they may have been contemporaneously

* Journal of Geol. Soc. of Dublin, vol. i., part 2, p. 89. † *Ibid.*, p. 97.

raised to their present level; a speculation into which I do not think it necessary to enter, the established fact of the difference of position and association, and consequently of date as to origin, being sufficient for my present purpose. Other matter is to be found scattered through Mr. Holdsworth's observations, which comes very well in corroboration of the general view which I have given of the structure and composition of that district in my memoir on the south of Ireland; although Mr. Holdsworth nowhere draws the distinction himself between the transition conglomerate and sandstone, and the overlying conglomerate and sandstone of the Monavoullagh range, further than by considering the former as a conformable, and the latter as an unconformable formation. On the contrary, he gives it as his opinion that all the conglomerates and sandstones which he has noticed in the eastern part of Waterford are identical, excepting perhaps those occurring on the north-west of the city of Waterford and those around Dunmore on the western side of Waterford harbour, which he conceives may be of more recent formation, but no reason is assigned for this opinion. The identity spoken of, however, seems mainly to refer to the similarity of mineral composition, respecting which there can be no difference of opinion.

But to return to Mr. Griffith. It is clear from the preceding that he has confounded together the old red sandstone formation, properly so called, with the red conglomerate, sandstone, and slate, which occasionally occur as constituent beds in a transition country.

The horizontal position of the Monavoullagh sandstone conglomerate is quite in accordance with what is observable in the detached portions of the same formation which are studded over the northern border of the clayslate table-land, overlooking from the south the valley of the Suire, and where in the year 1814, when first exploring that country, I found the sandstone conglomerate reposing unconformably on the truncated edges of the subjacent clayslate. Mr. Holdsworth appears to have made a similar observation in a quarry adjacent to the road between Kilmachthomas and Portlaw, where the junction of the clayslate and the conglomerate is very distinctly marked, the slate there being thrown up nearly perpendicular*.

Indeed, when we consider the long-drawn range of the Monavoullagh from north to south, being nearly at right angles with the eastern and western strike of the bordering older stratified rocks, both on its eastern and western con-

* Journal of Geol. Soc. of Dublin, vol. i., part 2, p. 95.

finer, whose dip varies from south to north, how could we expect any conformity between them?

If we now pass to the westward of the Monavoullagh sandstone conglomerate range by ascending from the vale of the Blackwater, from Lismore for example, to the table-land on the north, on which are planted the Knockmildown masses of the old red sandstone formation, we traverse in our course the older stratified rocks, still possessing the eastern and western strike, and for the greater part the southerly dip, and in which occur numerous slate quarries. The Monavoullagh sandstone conglomerate range extends nearer to the declivity leading down to the vale of Dungarvan than the Knockmildown sandstone conglomerate range does towards the Blackwater valley, from which it recedes considerably to the north, and nowhere does that formation approach and reach the vale until we enter upon the region of the Kilworth mountain and the adjacent hills, where it descends toward that valley and supports the carboniferous limestone; while in the defiles and ravines by which that mountain is furrowed, and whose course is toward the Blackwater, the clayslate formation is exposed to view in strata nearly vertical, supporting the old red sandstone formation in unconformed position*. But Mr. Griffith represents the conglomerate of the Monavoullagh as underlying these older stratified rocks, although they lie mainly to the west rather than to the south of the Monavoullagh range; so that, according to his view, the whole of the schistose country lying to the west of the Monavoullagh range, and extending to Kilworth, together with the superimposed ranges of sandstone and conglomerate of that region, are swamped in one general formation, coloured purple, which colour is made to extend as far south as to a line drawn from near Ringabella inlet, adjacent to the southern entrance of Cork harbour, to the head of Bantry bay, and thence to the western coast opposite to Dursey island, the said colour denoting a newer transition series, while the included bands of limestone are considered as be-

* Memoir on the South of Ireland, in Geol. Trans., vol. v., second series, § 47, in which I have remarked that the formation, as there exhibited, is quite analogous to the old red sandstone of England, presenting the same varieties in colour and composition, and consisting of compact and slaty beds of firm sandstone, associated with others of a looser texture, with sandstone conglomerate, and with beds of indurated clay and slaty clay; the reddish-brown colour predominating in the series. Similar relations are also well displayed in the old red sandstone of Kerry Head and the Slieve Meesh range, as well as in the ranges of Knockfeernagh, Kilcruaig, and Kilmeady, taken in connexion with the chain of the Seefin, Slieve Riagh, Slievenamuck, and Gaultees mountains. *Ibid.* §§ 48, 49, 50. The parallel might be continued still further north, *Ibid.* § 69, as well as in the various quarters indicated in my memoir on the east of Ireland, Geological Transactions, vol. v. first series.

longing to the carboniferous series, although it is admitted that the adjacent clayslate is interstratified occasionally with the limestone, such slate, however, to meet the occasion, being called a "carboniferous slate*."

Nor does the dilemma end here. The eastern part of Waterford is coloured grey, as indicative of an older transition series, and the rest of the schistose and conglomerate tract, extending to the western extremity of Kerry and north of the line already noticed, are coloured purple, as signifying a newer transition series†.

Now, in the year 1824 I discovered (as before mentioned) transition fossils in the south-eastern part of the county of Waterford, and in 1833 Mr. Holdsworth observed the same: and in 1829 I found other transition fossils in the western part of the Dingle peninsula in Kerry; and in 1837 Mr. C. W. Hamilton discovered them also in another adjacent locality, to the list of which Mr. Griffith has since added others. Those collected by myself appeared chiefly referable to such as are found in what have since been denominated in England the Cambrian and Silurian systems, and the same inference appears deducible from those later collected. These tracts therefore clearly belonging to an ancient transition series, it may reasonably be inquired why is the Waterford district coloured grey as an older transition series, and that in Kerry purple as a newer transition series‡?

Yet in reference to Kerry and part of Cork Mr. Griffith also contends §, that "the red conglomerate of Cahirconree and Carrantoohill (Gurrane Tual) mountains, together with the coarse red slate of which Tomies and Glenaa mountains at Killarney are composed, belong to the old red sandstone series; and that the limestone of Killarney, Kenmare, and Bantry, belongs to the carboniferous and not to the transition series." Having advanced so far, if Mr. Griffith had gone a step further (which he might have done with just as much propriety,) and included the patches of limestone near Skibbereen and Courtmacsherry bay also in the carboniferous series, the result according to Mr. Griffith's view would be

* In the new map it will be seen that in the eastern quarter of this tract the purple colour is now converted into reddish-brown, as indicative of old red sandstone, and in the western quarter chiefly into grey, as denoting greywacke slate.

† But remodelled in the new map, with reddish-brown as old red sandstone in the eastern quarter, and grey as greywacke slate in the western, as already mentioned.

‡ In the new map, the greater part of Kerry and a small part of Cork are now coloured grey, as greywacke slate.

§ Eighth Report of the British Association, pp. 83, 84.

that we have no transition limestone at all in the south of Ireland *; and by parity of reasoning, that all the schistose and conglomerated rocks associated with these bands of limestone in that part of the island, belong to the old red sandstone formation, since he cannot deny, and indeed admits, that those bands of limestone are interstratified on their confines with the adjacent rocks.

But in another place†, Mr. Griffith observes, “the schistose strata of the counties of Waterford, Cork, and Kerry, which form the base of the entire district, consist of greywacke and Silurian rocks;” while in a third place he remarks‡, “I am of opinion that eventually the greater part, if not the whole, of the schistose rocks of the counties of Kerry and Cork coloured greywacke slate on the map, will prove to be Silurian, with the exception of a narrow stripe of black clayslate which extends from the western base of Cahirconree mountain to Ballinguard bay east of Dingle harbour.”

This and the preceding quoted passage, when compared with the map of the “Outline,” are unintelligible, as the only part of Kerry coloured as greywacke slate is this very band of blackish-grey clayslate, and the only part of Cork so coloured lies south of the line already noticed, as drawn by Mr. Griffith from the coast opposite Dursey island to the head of Bantry bay and thence toward the southern entrance of Cork harbour§.

The blackish-grey clayslate to which Mr. Griffith seems to attach so much importance, is only a member of the general transition series, like the greenish-grey, the reddish-brown, and other coloured slates, all which may be readily found in

* In the new map this is now accomplished, the limestone being included and merged in what he is pleased to call “carboniferous slate,” supported by “yellow and old red sandstone,” which assuredly do not exist there.

† Journal of Geol. Soc. of Dublin, vol. ii. p. 81.

‡ *Ibid.*, p. 82.

§ The two passages noticed doubtless have reference to the new map, from the different colouring of which they can alone receive illustration; and here it is to be observed, that the southern part of Cork coloured in the map of the “Outline” as greywacke slate, is now in the new map most gratuitously distributed into parallel bands of what Mr. Griffith is pleased to call “old red sandstone” and “carboniferous slate,” to which appellations the strata in question have certainly no pretension. Take, as an example, the region of the Audley copper mines in the county of Cork, described by me in §§ 38, 39, of my Memoir on the south of Ireland, which is coloured by Mr. Griffith as old red sandstone. With equal justice might the region of the copper mines in the county of Wicklow be designated as old red sandstone, since in both cases the constituent rocks are clayslate, quartz clayslate, and quartz-rock. This is one instance, among many, in which hypothetical views are permitted to supersede the dictates of common sense.

Kerry, Cork, and Waterford, and of which I have given sufficient descriptions elsewhere*.

In following Mr. Griffith's progress thus far, what is the result? It amounts to this: that with the exception of the eastern part of Waterford lying between the Monavoullagh range and the coast, and the southern part of Cork lying south of the line drawn from near Dursey island past the head of Bantry bay to Ringabella inlet, both of which (coloured grey) are referred to an older transition series or to a Silurian series; all the rest of the schistose and conglomerated rocks, extending from the county of Waterford on the east through that of Cork into and through Kerry on the west (coloured purple), are viewed, and designated by Mr. Griffith in different parts of his communications, as the old red sandstone formation, as a newer transition series, as a Silurian series, (all these three being also called an old red sandstone series), and lastly, as an older transition series; while all the bands of limestone included in these schistose and conglomerated rocks, from the limestone band in the valley of the Bride on the north, to that occurring in Bantry bay on the south, are referred to the carboniferous limestone, although admitted to be in direct association with the rocks by which they are bounded†.

All this is undoubtedly sufficiently perplexing to a geological inquirer, since it leaves him no secure footing anywhere.

I now turn to the sections; yet another anomaly claims previous attention. All the old red sandstone tracts lying north of the city of Limerick, as those of the Bilboa and Slieve Bloom mountains, similar districts in the counties of Clare and Galway, and others yet more north, are coloured reddish and yellowish brown; and with this I do not quarrel further than that the adoption of two colours to designate the same formation seems superfluous, making that to appear complex which is simple, it being well known that both the colours and composition of the old red sandstone formation are very variable; and as to occasional interstratification on the borders with the carboniferous limestone, that may be very well expressed by words. The anomaly I advert to is this, that the old red sandstone of the extensive range of the Gaultees should be coloured as a newer transition series (purple), while

* Geol. Trans., vol. v., second series, Memoir on the South of Ireland, generally in §§ 8 and 15, and in particular in §§ 7 to 33, and §§ 34 to 41.

† These are the legitimate inferences deducible from the "Outline," with its map and the three later written communications. The various modifications and alterations given further to these views, as designated by the new map, I have already adverted to in preceding notes.

the Slievenamuck and the other ranges more west, which rise in the vale of Limerick (although in fact all connected and belonging to the same formation), are alone coloured as old red sandstone (reddish and yellowish-brown*.)

A few remarks here upon the construction of sections may not be misplaced. In my sections through the south of Ireland I have endeavoured to combine two objects, namely, to give outlines of the actual forms of the surface presented by nature, and to represent faithfully the relative position of the successive mineral masses, employing as nearly equal scales for heights and distances as the engraving would permit, the difference being only as four to three; but I have not attempted to introduce the differently modified structure of their respective stratification, and for two reasons: 1st, that to do it correctly upon any given transverse line according to scale and measure, would require considerable labour, and even when accomplished might not exactly accord with the results of an examination conducted on parallel lines in different longitudes, in a country where the stratification is more or less fluctuating; 2nd, that unless done correctly, such sections, instead of conveying precise information, tend rather to mislead the judgement. In such cases, it appears more judicious to supply the deficiency by adequate description.

Now, it is for the second of these reasons that I must object to Mr. Griffith's sections. They appear to me in many respects drawn rather according to the conceptions of their author than the occurrences in nature†. As some proof of this, I will in the first instance take the section which is drawn through the south-eastern part of the island, beginning at the valley of the Suire (where it first intersects the carboniferous limestone), and following it to its extremity in the

* In the new map the Gaultees are now also coloured reddish brown, as old red sandstone.

† The same remark applies to the sections drawn in Kerry by Mr. C. W. Hamilton also, but especially so to the section No. 3 in his "Outline of the Geology of part of the county of Kerry," in the Journal of the Geological Society of Dublin, vol. i. pp. 276 to 285, in which there is nowhere drawn a clear distinction between the conglomerates and sandstones of the transition series and those of the old red sandstone formation, the one being confounded with the other. Moreover, in No. 3 section, the old red sandstone and the coal measures are confounded together, the latter (north of Tralee) being made to underlie and break through the carboniferous limestone instead of reposing upon it, and the text in p. 284 is to the same effect. This is the paper referred to by Prof. Sedgwick and Mr. Murchison in the Lond. and Edin. Phil. Mag. in April 1839, at p. 260, and to which I had occasion to advert in the same Magazine for August, 1839, at p. 122. Two later sections in Kerry, given by Mr. C. W. Hamilton in the Lond. and Edin. Phil. Mag. for Dec. 1839, are not only open to the

county of Cork. In this section the vertical scale is to the horizontal scale as twelve to one; which necessarily produces great distortions in the relative position of the mineral masses*.

Let us first consider that portion of the section which lies between the valley of the Suire and the vale of Dungarvan, the latter of which extends westward to the Blackwater. With respect to the relative position of the carboniferous limestone of the valley of the Suire as reposing on old red sandstone, both on its northern and southern sides, there is no question. South of the river, the old red sandstone, which there forms a narrow border, reposes unconformably on the clayslate formation, the former dipping north and the latter south. These relations may be observed in many places, and are well exemplified in passing from the river Suire up the defile which leads to the Glenpatrick slate quarries, about three miles to the east of Clonmell, and which unconformable position is admitted by Mr. Griffith himself†. The same relative position is to be observed upon the range eastward also, and to the south of Carrick, through which Mr. Griffith's section passes; but Mr. Griffith there makes the clayslate of the hill of Carrick, as well as the clayslate extending towards the Monavoullagh range, to dip north, though just contrary to the fact, the dip being south; and by interposing a bed of conglomerate, all north of this, extending to the limestone of the valley, is called an "old red sandstone series," than which assuredly nothing can be more incorrect. For the details I have given, as well as general views affecting this region, I must refer to the 144th section of my Memoir on the east of Ireland‡. I have already remarked that the older stratified rocks on which the Monavoullagh range rests, dip generally south, yet are subject to local inflections to the north; but Mr. Griffith in his section represents the whole of these older rocks as vertical in the centre, from which the strata are made to incline on the southern side to the south, and on the northern side to the north, the latter being, as I have already stated, contrary to the fact. I have also

same objection, but they are at variance with themselves, the author representing what he terms "old red sandstone" to dip in fig. 1 generally south, and in fig. 2 generally north. They are also at variance with his former sections, given in the Journal above referred to, but especially so with the matters of fact. An unrestrained indulgence of fancy, with a loose application of the terms *Cambrian*, *Silurian*, *old red sandstone*, &c., can tend little to the advancement of Geology.

* The deceptive effect of such sections has been ably shown by Mr. De la Beche in his Sections and Views illustrative of Geological Phænomena.

† Journal of Geol. Soc. of Dublin, vol. ii. p. 86.

‡ Geol. Trans., vol. v., first series, part 1. 1819.

observed that the Monavoullagh conglomerate range approaches so far towards the vale of Dungarvan, that the schistose and conglomerated rocks lying south of the Monavoullagh range, and north of the carboniferous limestone of the valley, do not reach to the surface to any great extent; but Mr. Griffith brings into his section as south of the Monavoullagh the older stratified rocks which lie west of the latter, namely, such as we traverse north of Lismore when proceeding from the Blackwater toward the Knockmildown chain, and which in fact are merely the western continuation of the older stratified rocks that are exposed on the eastward of the Monavoullagh range. In the series of strata north of Lismore, and which he designates as the newer transition slate series*, there occur, he observes, in some localities, abundant marine exuviae, and even vegetable remains, e. g. calamites; and he adds that it is possible the whole may belong to the Silurian system†; and yet this series is represented as in direct association with the unquestioned carboniferous limestone of the Blackwater valley, which is quite contrary to my observations‡. The appearance of vegetable remains here is interesting, as bearing analogy to a similar occurrence in the transition series at Dunmore in the county of Kerry, observed by the late Mr. A. Nimmo, and as elsewhere remarked since by Mr. Griffith, which will be noticed in the sequel.

The ridge intervening between the limestone of the vale of the Blackwater and of Dungarvan, and that of the river Bride, is said to partake of a similar composition to that of the northern side of the Blackwater near Lismore, namely, that of green clayslate, yellow sandstone, and coarse red slate, forming in the centre of the ridge an anticlinal axis, the sandstone containing calamites§. The anticlinal axis I have not seen, the dip which I observed being throughout to the south; while in the composition of the ridge I found moreover numerous varieties of greywacke, slate, quartz-rock, and sandstone, frequently of a reddish-brown hue, also of a yellowish and whitish cast, and many coarse conglomerates, containing fragments and pebbles of considerable size||.

Having arrived at the valley of the Bride, it is time to advert more directly to the narrow stripe of brownish-yellow,

* But which in the new map is now called "old red sandstone."

† "Outline," p. 7, note, and Eighth Report of the British Association, p. 82.

‡ Both in the valley of the Blackwater and that of the Suire, Mr. Griffith appears to me to have confounded together the slate clay of the carboniferous limestone with the clayslate of the transition series.

§ Eighth Report of the British Association, pp. 82, 83.

|| Memoir on the South of Ireland, Geol. Trans., vol. v., second series, § 26.

which (as indicative of old red sandstone) is made everywhere to surround in symmetrical order the isolated bands of limestone which occur in the south of Ireland, (save and except those of Bantry bay, Skibbereen, and Courtmacsherry *), and which is also introduced in the vale of the Blackwater and Dungarvan, as underlying the limestone there. Old red sandstone is not within my cognizance in the positions indicated; and the occurrence of a grey or yellow sandstone with some *calamites* is another question. For I contend that the old red sandstone formation, in the legitimate sense of that term, nowhere passes to the south of the river Blackwater; and the two districts of that formation which occur in Kerry, namely, in the Slieve Meesh range and Kerry head, lie north of that parallel.

Doubtless influenced by similar systematic views, Mr. Griffith represents the whole series occurring between the valley of the Suire and Cork head as consisting of a regular sequence; which taken in an ascending order between the Suire and the Blackwater, is said to be composed of greywacke and slate, conglomerate, quartz-rock, red slate, yellow sandstone and sandstone-slate, green slate and limestone; and that in traversing the country further south, between the vale of the Blackwater and Cork head, we meet only with a repetition of the same succession between the different bands of limestone encountered in our progress†. That all these rocks are to be found in the sequence there is no doubt; but I know of no such regularity of order as is proposed, and excepting wholly the old red sandstone formation from the series, south of the Monavoullagh range. I must also remark that the disposition of the rocks as given does not correspond with the results of my researches, and that anticlinal and synclinal lines appear to be introduced where there is no proof of their existence. I advert in particular to the three bands

* But in the new map, these limestones are also bounded by the brownish-yellow stripe, which likewise is made a border to what is designated as reddish-brown "old red sandstone," wherever the latter is arbitrarily introduced, e. g., at the heads of Kenmare and Bantry bays, &c., where certainly no old red sandstone has been seen by me. Again, on the western side of the Lower Lake of Killarney, extending westward on the south side of the river Laune, Mr. Griffith confounds with the old red sandstone, the red conglomerate, sandstone, and red slate of the transition series, which prevail in that quarter, and are well exposed in the pass to Dunloe Gap, all dipping south, yet subject to some inflections^a.

† Proceedings of the Geol. Soc., May 22, 1839, p. 137; and Journal of the Geol. Soc. of Dublin, June 13, 1839, pp. 86 to 88.

^a Geol. Trans., vol. v., second series, Memoir on the South of Ireland, § 10.

of limestone, with their bounding schistose and conglomerated rocks, which in their range traverse Cork harbour, and which according to my observations are all in parallel position, with a dip to the south; but Mr. Griffith represents them as forming successive troughs, with corresponding anticlinal and synclinal dips, of which I certainly have no knowledge. In one essential point, however, we are agreed, namely, that these parallel bands of limestone show, wherever exposed, an interstratification with the adjacent clayslate, both rocks exhibiting in general the same organic remains; which are also contained in the clayslate still further south, on the north side of Ringabella inlet, and again on the same parallel to the west, adjacent to the road leading from Cork to Brandon. But in the clayslate and sandstone subordinate to it are found also vegetable remains, showing a further analogy to similar rocks north of the Bride, to those north of the Blackwater, and at Dunmore head in Kerry. Mr. Griffith has given us a list of transition fossils which he had collected at Ferriter's cove; and another list of those he had obtained from the limestone and clayslate of Cork harbour, at Rostellan, Rinniskiddy, &c. Of the latter Mr. Sowerby has pronounced some species to be the same as occur in the carboniferous limestone, and others to resemble such as are found in the older stratified rocks of Devonshire; and it might be added also such as occur also in Ferriter's cove and in the Silurian system, e. g., *Leptaena (Producta) lata*, *Leptaena depressa*. I may also add, as a further analogy, that in the older stratified rocks of Devon and Cornwall, vegetable remains are likewise met with; e. g., in Devon as observed by Major Harding, the Rev. D. Williams, Professor Sedgwick, and Mr. Murchison; and in Cornwall by Professor Sedgwick, as noticed by Mr. Ansted in his paper on *Endosiphonites* * (the *Clymenia* of Count Münster.)

Every addition to our knowledge is valuable, and I trust Mr. Griffith will continue to employ hands in the south of Ireland in collecting fossils, for which he possesses so many opportunities.

I have now to advert to Mr. Griffith's section in the Dingle peninsula, in the west of Kerry, which in no part exactly accords with my observations. In this section the vertical scale is to the horizontal scale as five to one. But the portion which more immediately claims attention is that which extends from the summit of the old red sandstone of the Slieve Meesh range † to the carboniferous limestone in the

* Cambridge Phil. Trans., vol. vi. 1838.

† Called Cahirconr  e by Mr. Griffith, but which denomination I conceive to apply strictly to the mountain range stretching to the west of Bartrigoun.

vicinage of Castle island. The former is represented as constituting merely a cap or sheet formed upon an inclined plane from west to east, the strata corresponding, and succeeding each other in that direction to the junction with the carboniferous limestone. I know of no such arrangement. On the contrary, the strata of the old red sandstone are accumulated to a great depth, and certainly in some quarters at least to the level of the sea, if not deeper, being disposed in a gently arched form from north to south, as may be well observed in the defiles and glens which penetrate from the north into the interior of that mountain range*. In this series I have not observed a general dip to the east, the strata even in the most eastern quarter (including beds of red clay and red slaty clay) still preserving the flat arched arrangement from north to south; and it is only at the eastern foot of Slieve Meesh that other beds appear dipping to the east of south, and which from their dissimilarity altogether to the old red sandstone formation of the Slieve Meesh range, I could only view as a protruding portion of the subjacent transition rocks continued from the west. I know of no organic remains to invalidate this conclusion. The *Spiriferæ*, *Productæ*, *Terebratulæ*, and *Crinoidea* that I met with were too indistinct to admit of determining the species, but I am mistaken if there be not an *Orthis* among the number; while the *Favosites* which I noticed I apprehend to be *F. fibrosa*. This eastern foot of the Slieve Meesh is represented by Mr. Griffith as composed of a succession of beds of yellow and grey quartzzy sandstone, dark grey clayslate, sandstone, dark grey clayslate, grey quartzzy sandstone, alternating limestone and greenish clayslate, against which the carboniferous limestone is exhibited as abutting in unconformable position†. The sandstone is stated to contain calamites, and the general series to abound with the casts of fossils, whose distinctive characters, it is admitted, it is difficult to recognise, but among them are *Productæ*, *Spiriferæ*, *Terebratulæ*, *Crinoidea*, and *corals*; and the upper beds of the greenish grey clayslate are said to be identical with those which occur alternating with limestone in the peninsula of Muckruss. I confess I have not traced any such uninterrupted succession as is here described, the country in general being well covered up; but if we suppose it to be correct, there is no direct proof that such succession belongs to the carboniferous series. From the quarry which I have

* Memoir on the South of Ireland, §§ 10, 13, 49, in Geol. Trans., vol. v., second series.

† Journal of Geol. Soc. of Dublin, vol. ii, pp. 82, 83, and the lower section in plate iv.

described at Riversville on the right bank of the Maine *, a low ridge crosses that river and extends some distance beyond it, in which I could discover traces only of the grey-wacke formation, and around which the carboniferous limestone of the vale appears to sweep on the north, east, and south, in nearly horizontal position, wherever exposed in the adjacent quarries †. For these several reasons my view necessarily differs from that taken by Mr. Griffith.

From the preceding it will be inferred that I do not consider Mr. Griffith's representations as in any respect invalidating the conclusions to which I have been led, as exhibited in my memoir on the south of Ireland. The day is past when it might be authoritatively pronounced that such and such a limestone is carboniferous merely because it contains some fossils that are common to the latter. I have entered at some length into this subject in my memoir on the south of Ireland and in the Lond. and Edin. Phil. Mag. for August 1839, to which I beg to refer. But I cannot avoid noticing in this place, as a case in point, the communication made by Mr. Austen to the British Association, at Birmingham, in August, 1839, respecting the fossil remains of the limestones and slates of South Devon ‡, which in its general views so well corresponds with the tenor of my publication in the Lond. and Edin. Phil. Mag. of the same month, although our respective observations and inferences were made independently of each other. Mr. Austen conceives that a great identity of species can be established between the *Radiaria*, *Mollusca*, and *Crustacea*, of a portion of the Rhine and those of South Devon; and he states that both districts present many forms of animal structure, such as in this country we should call carboniferous, and that of *forty* species which were enumerated, some were hardly, and some not at all, to be distinguished from those of our mountain or carboniferous limestone. Mr. Austen considers as the geological equivalents of the slates and limestones of South Devon those of the Rhine and Eifel, and that strata in the south of Ireland are of the same age; observing also that many of the same fossils occur at Nehou and St. Sauveur in Lower Normandy. He remarks also that *old red sandstone* is an unfit name to designate the limestones and roofing slates of South Devon, or the white sandstones of Lower Normandy and Brittany. To which I may add, that it is also inapplicable to the older stratified rocks of Ireland.

* Memoir on the South of Ireland, § 13. † *Ibid.*, § 51.

‡ See Athenæum of August 31, 1839, p. 661.

I may here appropriately advert to the remarks made by Capt. Portlock, R.E., late President of the Geol. Soc. of Dublin, on Mr. Griffith's arrangement of the strata in the south of Ireland, as well deserving of the attention of the latter*. He observes, that Mr. Griffith's transference of those strata which were formerly designated as of the transition epoch to the old red sandstone, must be considered as springing from the generalizations in Devonshire of Professor Sedgwick and Mr. Murchison. To those generalizations I have objected in the paper referred to above in the Lond. and Edin. Phil. Mag. for August 1839; and I have already stated that Mr. Austen started similar objections in the same month. But we may proceed a step further, and show that evidence is not wanting to prove that the older stratified rocks of Devon and Cornwall belong to an ancient transition series. The genus *Clymenia*, discovered and determined by Count Münster in the Fichtelgebirge, occurs also in Cornwall and Devon according to the observations of Mr. Ansted, Professor Sedgwick, Mr. Murchison, Mr. De la Beche, and Professor Phillips. In the Fichtelgebirge the *Clymeniæ* are accompanied by *Goniatites* also, and Count Münster enumerates fourteen species of the former and twenty-six species of the latter, together with one hundred and eighteen species of other fossils; namely, *Trilobites* 14 species, *Serpula* 1, *Bellerophon* 3, *Orthoceras* 22, *Gasteropoda* 31, *Conchifera* 43 (among which are species of *Orthis* and *Terebratula*, but no *Spirifera*, according to M. Von Buch†), *Crinoidea* 4. This tract is referred by Count Münster to an ancient transition series, the *Clymeniæ* being confined to it, and not occurring in the upper strata of the country, namely, in the carboniferous limestone‡. Von Buch also considers this tract and the environs of Prague as belonging to the more ancient strata of the transition epoch, and as being perhaps the oldest of that class to be found in Germany§. And M. Beyrich makes the general observation, that *Clymeniæ* appear restricted to the older transition strata, not having hitherto been met with

* See pp. 25 to 27 of the President's Address, February 14, 1839, in Journal of Geol. Soc. of Dublin, vol. ii. To Capt. Portlock we are indebted for the discovery of a small transition district in the county of Tyrone, which embodies several fossils common both to the Silurian region and to certain transition tracts in North America, *Ibid.* pp. 28, 29.

† *Bulletin de la Société Géologique de France—Séance de Mars, 1836.* Tome vii. p. 156.

‡ Bayreuth, 1832; and *Annales des Sciences Naturelles.* Séconde série. Tome ii. 1834.

§ *Bulletin de la Société Géologique de France—Séance de Mars, 1836.* Tome vii.

either in the later transition limestones or in the carboniferous limestone; while *Goniatites* on the other hand, he remarks, are widely spread in strata both of the transition and carboniferous epochs, extending in the latter series into the coal formation*. Now, at South Petherwin in Devon, Professor Phillips has ascertained that at least four species of *Clymenia* occur, undoubtedly belonging to the same group as is found in the Fichtelgebirge, and among these one is identified as the *Clymenia lævigata* of Count Münster. With these he notices also two species of *Goniatites*, one of which belongs to the same group that occurs in the Fichtelgebirge†. But we need not rest here: other decided transition fossils are met with in the older stratified rocks of Devon and Cornwall, e. g., various species of *Terebratula* and *Atrypa*, namely, such as occur in the transition tracts of Sweden, in Gothland, &c., together with species of *Orthis*, *Pterinea*, *Leptæna lata*, &c., for the general list of which I refer to the work of Mr. De la Beche already cited, as well as to the observations of Professor Sedgwick and Mr. Murchison‡. I cannot doubt that more extended researches in Devon and Cornwall will vastly increase the list, and yet bring to light many facts of high interest§.

While on the subject of the distribution of organic remains, I will add a few remarks on that of *Goniatites* in particular,

* *Ueber die im Rheinischen Uebergangsgebirge vorkommenden Goniatiten*, p. 22 (Beiträge, 1837). See also a translation of this Memoir in the *Annals of Natural History* for March and May, 1839.

† See Mr. De la Beche's *Geological Report of Cornwall, Devon, and West Somerset*, pp. 59, 60.

‡ *Proceedings of Geol. Soc.*, May 1838; and *Lond. and Edinb. Phil. Mag.*, April 1839.

§ It is remarkable that Prof. Sedgwick formerly considered the strata containing the fossils that have been indicated (and which include also *Trilobites* and *Orthoceratites*), as the lowest fossiliferous rocks in Devonshire and Cornwall^a. And yet, according to a later view, originating in the suggestion of Mr. Lonsdale, Prof. Sedgwick and Mr. Murchison now include all the older stratified rocks of Devon and Cornwall under the head of *old red sandstone*; and to this view it appears Mr. Greenough has been led to conform, as shown by the colouring of those tracts in the new edition of his *Geological Map of England and Wales*. Viewing the subject, as I do, with the eyes both of a geologist and a miner, I must here, with all respect, repeat my protest against any such generalization, such appearing to me contrary to all analogy, and as only tending to confound subjects essentially distinct. The designation as *Devonian* might not be equally objectionable, as that does not necessarily bind us to more than what we actually encounter within the limits of the older stratified rocks of Devon and Cornwall, respecting which, however, we yet require much additional information.

^a See Mr. Ansted in *Cambridge Phil. Trans.*, vol. vi. p. 422, 1836; and Prof. Sedgwick in *Proceedings of the Geol. Soc.*, vol. ii. p. 683, May 1838.

in transition and carboniferous strata. If we compare together the labours of M. Von Buch, Count Münster, M. Beyrich, and Professor Phillips, on this subdivision of the tribe of the Ammonites, the general result is that in the older transition rocks the Goniatices met with possess a *simple* dorsal lobe, whilst those which occur in the carboniferous series have a *divided* dorsal lobe. If this observation were rigidly exact, it might be adopted as a distinctive character between strata of the transition and carboniferous epochs respectively; and it might also be inferred that limestones which contain Goniatices, some of which possessed a simple and others a divided dorsal lobe, might be considered as occupying an intermediate station, that is, as belonging to the later portion of the transition series. But in neither case does the rule appear to hold without exceptions. Thus, of the 26 species of Goniatices noticed by Count Münster in the Fichtelgebirge, the 22 (four being doubtful) which are strictly defined and figured, have all a simple dorsal lobe; but two other species are figured by M. Von Buch, as derived from thence, with a divided dorsal lobe, namely, *Ammonites inæquistriatus*, tab. ii. fig. 10, and *A. semistriatus*, tab. ii. fig. 12*. Of 8 other species described and figured by Von Buch, and referred to but not figured by M. Beyrich, 5 are found in transition tracts, and four of these have a simple dorsal lobe, and one a divided dorsal lobe; while of the three species found in the carboniferous series, two have a simple and one a divided dorsal lobe. M. Beyrich has described 18 species of Goniatices, and 14 of these are figured, but in 12 only are the dorsal and lateral lobes given, and of 10 of these which are derived from transition tracts, five have a simple and five a divided dorsal lobe; while in the two found in the carboniferous series the dorsal lobe is divided. M. Beyrich enumerates (including the eight new species designated by himself) altogether 42 species of Goniatices, the greater number being derived from the researches of Count Münster and M. Von Buch, and a few from the descriptions given by Professor Goldfuss and MM. Martin and Sowerby. Of this list four or five species only are referred to the carboniferous series. Professor Phillips enumerates 33 species of Goniatices as occurring in the carboniferous system of Great Britain and Ireland†. Of these he has represented the dorsal and lateral lobes of 24 species, out of which 21

* Von Buch, *Ueber Goniaticen*, in the Transactions of the Royal Academy of Sciences of Berlin, December 15, 1831.

† Illustrations of the Geology of Yorkshire, part ii. 1836. Mountain Limestone District.

possess a divided dorsal, and three a simple dorsal lobe. Of the 33 species, 30 are first made known to us by Professor Phillips; only three of them having been previously noticed by authors, namely, *Goniatites sphaericus* (Mart.), *Gon. Listeri* (Mart.), and *Gon. Henslowi* (Sow.)

I have no doubt, that Professor Phillips, who is so well qualified for the task, will give due attention to this subject in Devon and Cornwall; and it will be interesting to learn in what degree the *Goniatites* of the carboniferous limestone in North Devon, near Barnstaple (e. g., at Swimbridge, where they abound), differ from those found in the transition districts of South Devon and Cornwall. The same subject deserves attention in Ireland*.

To M. Von Buch we owe the first precise distinction between *Nautili* and *Ammonites*†. The range of the former extends, it is well known, from the oldest to the most recent of the fossiliferous strata, the genus being still in existence. The *Ammonites*, on the other hand, though equally ancient in origin, do not in their range pass beyond the limits of the cretaceous group. To the same distinguished naturalist we owe the distribution of the *Ammonites* into 14 families‡, the first and oldest of which, the *Goniatites*, are characteristic of the transition and carboniferous epochs, not extending beyond the coal formation; the second, the *Ceratites*, appear confined to the muschelkalk; while the remaining 12 families are distributed through the series of formations extending from the lias to the chalk inclusive. But we are indebted, primarily, namely, in 1828, to Professor Bronn of Heidelberg, for the important observation so conducive to the distinction of formations, that no *Ammonites* with denticulated lobes have been found in strata of the transition and carboniferous æras, such being confined to the formations of a later origin. This remark was publicly made known by Von Buch in 1829, nearly at the same time that M. Elie de Beaumont was preparing to announce a similar observation made by himself; and the remark was shortly after confirmed and generalized by Count Münster§.

* Count Münster considers his *Goniatites ovatus*, found at Gottendorf and Schleitz in the Fichtelgebirge, to be the same as the *Ellipsolites* (*Nautilus*) *ovatus* of Sowerby, which occurs in the Cork band of limestone. And he inquires whether some other species described as *Nautili* by Mr. Sowerby might not prove to be *Goniatites*.

† *Annales des Sciences Naturelles*, tome vii. première série, 1829.

‡ *Annales des Sciences Naturelles*, tome viii. 1829; and Transactions of the Royal Academy of Sciences of Berlin, April 1, 1830, *Über die Ammoniten in den älteren Gebirgs-Schichten*.

§ Von Buch, *über Goniatiten*, December 15, 1831, in the Transactions of

M. Deshayes, however, has lately remarked that *true Ammonites* (with denticulated lobes, I presume) have been found in strata more ancient than the coal formation, namely, in the environs of Tournay *.

In reference to my successive memoirs on the east and south of Ireland, I will here add the general remark, that it has been my endeavour in both to embody in few words the results of practical experience and observations conducted at intervals in that country during a lapse of more than forty years; and in laying them before the public, it has been my object, in both cases, to exhibit through the medium of condensed abstracts a compendious view of the relations of the tracts described, thus relieving the reader from the labour and tedium of passing through the progressive steps of extended researches. That a minute examination of the ground which I have trodden may lead to further discoveries, particularly in respect of the distribution and number of the organic remains, I am far from disbelieving, and I shall be ready to hail their appearance with pleasure; but as in the development of the main relations of the mineral masses I have endeavoured to be exact, I may be excused if I do not anticipate any very great accession to our knowledge in the latter respect.

[To be continued.]

L. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron.* By Dr. C. SCHAFHAEUTL, of Munich.

[Continued from p. 50.]

I SHALL now describe another action of acid bodies on iron, which has much resemblance to that which is exerted by the action of sea water on it.

I poured over a parallelopipedic fragment of tilted but very tender and indifferent razor-steel, in a tea-cup, concentrated hydrochloric acid. After the action had for the most part ceased, I changed it for fresh acid, and all visible action of this fresh acid had ceased the next day. The cup was preserved in this state without being disturbed for nearly two months. After this lapse of time I found the fragment, apparently unchanged,

the Royal Academy of Sciences of Berlin, published, with the preceding Memoir, in 1832.

* *Bulletin de la Société Géologique de France—Séance de 19 Février, 1838.*

surrounded by a ring of black sediment, about a quarter of an inch distant from it; this ring, which was about three-quarters of an inch broad, was formed of rays tending towards the centre of the cup and having scarcely any connexion between them; the opposite end of the fragment, which lay in the magnetic meridian, was covered with fibrous bundles of the black sediment like the pole of a magnetic needle dipped into iron filings. I decanted the liquid in order to wash the remaining fragment and to free it from its black covering; but on touching it, I found it converted into a soft black plastic substance. Exposed to the air, it soon changed its black appearance to that of brown, and attracted very rapidly moisture from the atmosphere, until it became nearly liquid. Hydrochloric acid heated did not appear to act upon it; nitric acid first evolved some nitric oxide gas, converting it into a brown pulpy mass of a greater volume; nitro-muriatic acid at a boiling heat dissolved it almost completely; it melted before the blowpipe, after disengaging copious fumes, into a globule of silicate of iron, strongly acting upon the magnet. Twenty grains of it burnt with chromate of lead and chlorate of potassa, developed carbonic acid gas, azote, and water. The greatest part consisted of iron, but only a trace of silica was separated, too small to be weighed.

This leads me to the consideration of a paradoxical phenomenon consisting in the insensitiveness of iron to the action of nitric acid under certain circumstances; a phenomenon which has hitherto excited so much attention, and has been attributed to an electrical inactivity of the iron itself, or to a film of hydrogen or binoxide of hydrogen protecting the surface of the iron. We have seen that when a considerable mass of iron is attacked by acids a black skeleton always remains, which very quickly changes colour when exposed to the air. We have further seen, that cast iron, which is a mixture of carburet of iron and silicon, is even attacked by hydrochloric acid on the surface only; that is to say, that as soon as the surface is coated with a pellicle of that black or brown residuum which we shall soon more accurately describe, all action of the acid and all evolution of hydrogen apparently cease; but we learn from the latter experiment that in time an invisible action always takes place, by which the mass of iron is slowly decomposed, and the remainder assumes a different shape according to the concentration or the different chemical properties of the acid itself.

When the liquid in which the iron is immersed contains free acid, the decomposition of water stops as soon as the iron is covered with a pellicle, which prevents the close contact of

a mass of iron with a mass of acid, and allows only a separated molecular contact between the iron and the acid.

When the liquid contains a great deal of *free acid*, after the evolution of hydrogen has ceased, and the air has free access to it, the residuum, after the following invisible action has ceased, will be more or *less plastic*. If the liquid contains very little free acid and the air is excluded, a basic protochloride of iron is formed by the liquid attacking only the carburets of iron, leaving the sulphurets of iron, the carburets of silicon, and the higher carburets of iron, as a skeleton behind; a similar effect takes place when the solutions contain only chlorides of magnesium and sodium. When the air has access, the metal becomes oxidized by the air contained in the liquid, and forms invariably basic salts with the chlorides in solution, which, when washed away (as is the case when iron is immersed in the sea), the skeleton of carburets of iron, silicon, &c. remain behind, which, from its highly spongy nature, condenses and compresses in its pores, when exposed to the atmosphere, atmospheric air in great quantity; part of the iron is oxidized, and when the iron is in very large masses, the temperature is generally increased to the boiling point.

The action of oxygen, by the presence of acid and water, on a solution of chlorides, presents one of the cases of chemical decomposition by which two or more chemical bodies enter into a new combination by the sole presence of another chemical body, which during the whole decomposition remains entirely unchanged. Berzelius explained this by a reference to a new force, which he called *catalytic force*. I shall, in another place—in my theory of “*the final action of chemical forces*,”—endeavour to demonstrate, that in fact *no chemical combinations of two bodies* takes place without the catalytic presence of a third, or in fact, that *all chemical combinations and decompositions* are produced by *catalytic force*.

All cast iron, when dissolved in acids, provided the fragments retain their form after the action has ceased, show, after being washed on a filter, the same property of condensing air in their pores, and of increasing the temperature by the oxidation of the iron left in a lower state of carburation. If the liquid contained very *little free acid*, very soon a basic salt is formed; and where the access of air is prevented during the progress of the solution, the remainder, when placed on a filter, oxidates extremely rapidly, and a hydrate of oxide of iron goes with the water through the filter, even after a week's incessant washing. Similar and often highly-interesting results are obtained by a peculiar sort of chemical action, where one body slowly separates from others, by combining with a third, with-

out setting the remaining parts of the compound in perfect freedom, and enables them to replace the lost body by another chemical equivalent, or to allow their molecules to follow their own attractive tendency, and to form, after being set free, a new individual body for themselves.

The action of bodies in their utmost molecular division is quite otherwise than when their molecules had been sufficiently moveable to follow, after separation, their own mutually attractive forces and to neutralize them in one centre.

A body whose chemical force had been neutralized by another, now deprived of that neutralizing body, without having sufficient freedom of its molecules to follow their forces of mutual attraction set at once at liberty, is in the state of a body surrounded by an atmosphere of electricity and in a different relation and condition of tension to the surrounding bodies.

This relation is visible in all bodies placed under such circumstances in contact with others: the instance of the reduction of iron at a low temperature by a current of hydrogen is well known; the following fact seems less so. A lump of malleable, hammered, or rolled iron raised to a white heat, over which water is poured, exerts a very feeble decomposing power on the water. Let us now take a similar mass of iron out of the puddling furnace, just ready to be placed under the hammer, and pour a basin of water upon it. No hissing noise is perceptible, no generation of steam visible*, and the water is at once decomposed, the flames of hydrogen enveloping the white-hot ball and rising very often to a height of four or five feet. I need scarcely remark, that the iron taken out of the puddling furnace is in the same state as iron treated with acids; a skeleton of each grain of cast iron remains, with this difference only; that with iron treated with acid, electro-positive metals are partially removed from the compound; but by treating cast iron in a puddling furnace, the relative electro-negative bodies are taken instead, without destroying the mechanical texture. A few strokes of the hammer destroy this property of the white-hot iron.

When a constituent of a decomposed body is separated from another, in a solid state, by chemical action, the so-separated body is never left in the same state as that in which it existed when forming a corresponding part of another body; and each body set at once free by chemical action, with per-

* On the subject of the subitaneous formation of steam by red-hot or white puddling slag, oxide of iron, and malleable iron, I mentioned some curious instances in my paper "On the conversion of water into steam at the higher degrees of temperature," &c., in the *Mechanics' Magazine*, vol. xxx. pp. 138, 294, and 339.

fect liberty of its molecules, combines with other bodies or takes a new form of very different quality, under which all precipitates, separated from the solvent liquid, exist.

In my theory of "*The final action of chemical forces*" I shall endeavour to show, that one of *the first principles* of the formation of the productions of animated life is in fact only an action of the before-mentioned chemical forces *under different circumstances*, which may be expressed in the short sentence: *extremely fine division* (molecular separation), during an uninterrupted motion.

When very gray cast iron is treated by hydrochloric acid, the residuum is a white substance, mixed with black scales of graphite, which Karsten considers as being mechanically mixed with the mass of the iron before solution, and contrasts it with the black precipitate which separates from white cast iron, which has a dull earthy appearance; but when this so-called dull and earthy powder of white cast iron is produced by a tolerably diluted acid, and viewed, while still in suspension in the liquid, under a certain angle of vision, all the small particles appear to consist *likewise of scales*, as we shall hereafter perceive, which reflect the light with the same power as the scales of graphite, their only difference from them consisting in a different chemical composition. They are therefore more easily decomposed, and apt from their softer nature to adhere rather closer to each other, in which state their scaly or foliaceous nature is not so easily to be discovered.

To ascertain the relative combinations of the elementary constituents of the different sorts of iron of commerce, the treatment by means of acids is indispensable, and I prefer the hydrochloric and nitric acids to all others.

The quantity and in part the quality of the different combinations obtained by treatment of the iron with hydrochloric acid, depends a great deal on the density of the bodies brought into chemical contact, or, in other words, on their specific gravity.

If irons of different specific gravity are dissolved in hydrochloric acid of the same specific gravity, the residuums will have a different appearance.

If filings of Swedish iron, for example, double bullet iron of the specific gravity 7·810, is dissolved in hydrochloric acid of the specific gravity of 1·169 in comparison with English iron of the specific gravity 7·60, the residuum of Swedish iron will appear in distinct heavy grayish scales, that of English iron in brown flocculent *rags*, partly remaining on the bottom of the phial and partly suspended in the liquid. But if the specific gravity of the acid is increased, the resi-

dium of the Swedish iron will assume the same appearance as the English, and so in the reverse case; this circumstance explains why anchors made of English iron, although tougher than those made of Swedish iron, are almost twice as soon destroyed by the action of sea water.

The same rule holds good, when iron, with other chemical agents, is treated in the dry way. For instance, at the same degrees of heat as those in which malleable iron of the specific gravity 7·4 has combined with the greatest quantity of carbon necessary for it to become liquid,—at the same degree of temperature Swedish iron of the specific gravity 7·7 (called Hoop L), or 7·8 (called double bullet), just begins to crystallize, forming steel, and sinking down to a specific gravity of 7·5.

The quantity of the residuums of iron of the same description bears a very remarkable relation to the specific gravity of the acid, as is evinced by the following table, showing the different quantity of residuums of 35 grains of Welsh iron from the Maesteg iron-works, specific gravity 7·407.

Specific gravity of the acid.	Residuum.	Sulphuret of Lead.
1·160 . . .	6·770 . . .	1·73
1·140 . . .	9·244 . . .	1·71
1·103 . . .	13·711 . . .	1·703.

Mottled iron from the forges at Alais, departement du Gard, in the south of France:

Specific gravity of the acid.	Residuum.	Sulphuret of Lead.
1·16 . . .	7·51 . . .	6·06
Acid very much diluted	11·42 . . .	0·6475
Acids still further diluted	21·49 . . .	1·015

In the fourth example I poured water over the fragments of iron in a retort, the beak of which was connected with two bottles of a solution of nitrate of lead, and I dropped only into the retort so much hydrochloric acid as to cause a very moderate evolution of hydrogen. In the last sample I put only a few drops of acid into the retort, just sufficient to cause a very slight attack of the iron. The third column of all the samples contains the quantity of sulphuret of lead obtained. The evolution of gas continued for several days, and when it ceased, the addition of acid had no further visible effect*.

* As I repeated the last experiment but one, I put some fragments of the cast iron in the upper part of the beak of a new retort in order to witness the effect of the evolving gas on iron fragments. The next day I found the fragments converted into a black crumbly and rather unctuous mass, soaked with a yellowish liquid, which fastened it to the glass. On nearer

By treating iron with *diluted* hydrochloric acid, the sulphur escapes *almost* entirely as sulphuretted hydrogen; the same is the case with antimony escaping as antimoniuiretted hydrogen. On the contrary, scarcely any portion of the arsenic escapes with the hydrogen.

The case is different when iron is treated with nitric acid. When fragments of iron are treated with nitric acid of such a specific gravity that the iron is moderately attacked in a retort whose beak is connected with a solution of carbonate of barytes and acetate of copper or lead, and only a slight evolution of binoxide of nitrogen takes place, the nitric oxide is then first absorbed by the air in the uppermost part of the retort, and a partial vacuum is produced which makes the liquid rise in the beak of the retort several inches. A few hours afterwards pure azote is evolved, acting neither on the carbonate of barytes nor on the acetates or nitrates of metals. Shortly after, the evolution of gas again ceases, and after the action of the acid on the iron stops, a new partial vacuum is produced and the liquid is found to stand in the beak of the retort several inches above its level.

For example, I took 20 grains of dead gray cast iron from the forges of Creuzot, departement de Saone et Loire, treated it with hydrochloric acid specific gravity 11.5, and left residuum

inspection I found those lumps covered with stalks of a black vegetation, exactly like the mould on ink or sour starch paste, and differing only in colour; the stalks were from about a half to three-quarters of a line in height, and terminated with a little knob on the top. The next day the little knob on the top assumed an orange red, and on further exposure to the air, the stalks became of the same colour.

Viewed through the microscope, these little mosses appeared opaque, of a varnished yellow colour like the petals of Everlasting, and having a long curved cylindric capsule on the end. The capsule was alternately expanded and contracted very similar to the duodenum of animals, and curved into a circle, so that the end almost met the point when the capsule was attached to the stalk; and when viewed under the microscope superficially, the knobs appeared to consist of a perfect ring fixed to the stalk. I found only one stalk, which shot forth two of these capsules from the same point, curved up just like the horns of a ram. No operculum could be discovered; the end of the capsule was found perfectly rounded like a globule with rather a narrow neck, and when pressed with a fine needle, the capsule burst, emitting extremely fine seeds, exactly like the genus *Phascum*. No traces of leaves could be detected, unless we reckon some entangled and interwoven black filaments on the surface of the decomposed iron. Not being sufficiently acquainted with the cryptogamic branch of botany, I am not able to decide on the generic character of these small vegetations.

On the retort being packed up in moss and afterwards washed with rain water, the seeds of the moss must have been derived from one of these sources; but the most extraordinary circumstance is their becoming fixed in these decomposed fragments of cast iron, serving as a very fertile mould, and growing rapidly in hydrochloric acid gas and sulphuretted hydrogen.

= 7.08 grains. The same quantity of the same iron treated in the above-mentioned manner with nitric acid of 1.10 specific gravity left residuum 3.24 grains; no trace of sulphuric acid could be discovered in the liquid. By boiling the remainder in a platinum crucible with strong nitric acid, the greatest part of the sulphur was separated, which adhered slightly as a yellow riband to the side of the crucible, and no traces of sulphuric acid were to be found in the solution. The sulphur thus separated from 1.55 grains of the residuum weighed 0.180 grains, and this burnt on a platinum foil left 0.100 sulphuret of iron. That which the nitric acid had dissolved during the boiling was precipitated with caustic ammonia and consisted of

Iron	0.443
Phosphorus	0.155
And left residuum .	0.31
Humine	0.33

We see therefore that the sulphur had been in combination with the iron, approaching in this formula $F^3 S$, that is in a certain well-known state of chemical proportions.

Another remarkable circumstance is the difference of the products of solution according to the difference of the specific gravity of the acid, which throws a light on several chemical products, which arise from different degrees of concentration of chemical agents, and the varying products obtained by the various degrees of heat.

In hydrochloric acid we have a juxtaposition of atoms of chlorine and hydrogen intermixed with atoms of hydrogen and oxygen; and the interposition of a certain quantity of atoms of water betwixt a certain number of atoms of chlorine and hydrogen, alone determines how many atoms of the iron shall combine with chlorine to form protochloride of iron, and how many atoms of hydrogen shall form compounds with carbon and azote.

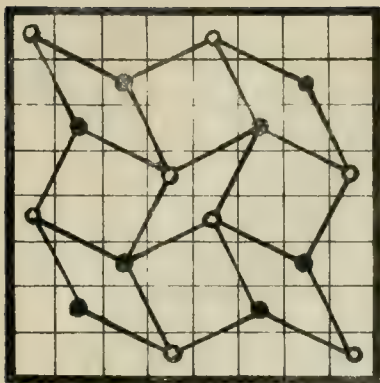
The description of the residuums left by treating iron with acid was always a matter of great speculation. Berzelius says the remainder of the solution of iron in sulphuric acid was black, in hydrochloric acid gray, and sometimes white.

The colour of the residuum, as shown, depends partly on the specific gravity of the iron and partly on its chemical composition.

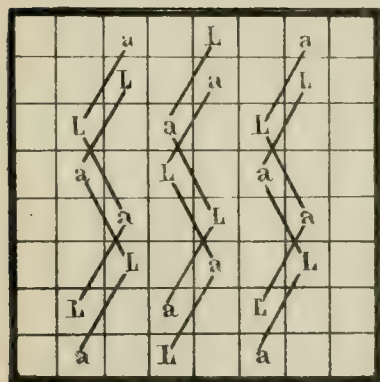
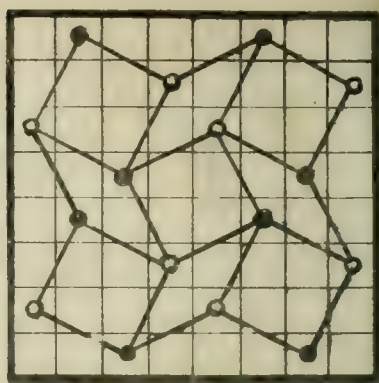
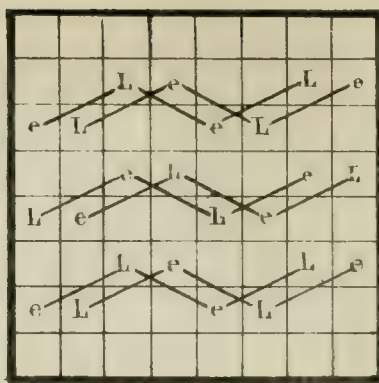
The residuum of white cast iron prepared with coke as fuel, in hydrochloric acid is always brown, yellowish brown or greenish brown; the remainder of very *gray coke iron* is always light gray, in different shades approaching to white.

[To be continued.]

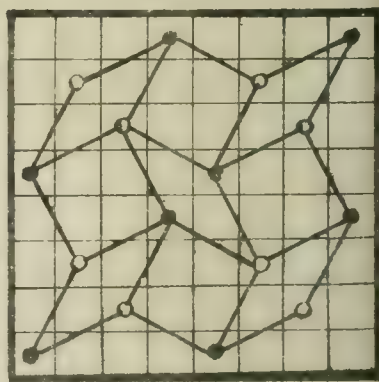
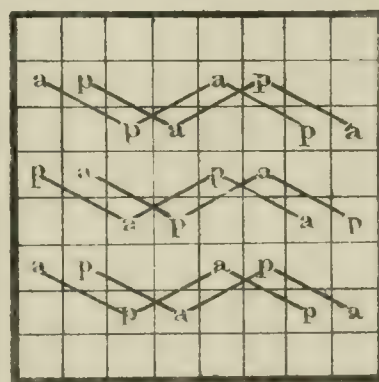
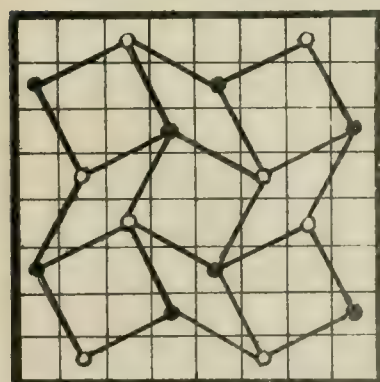
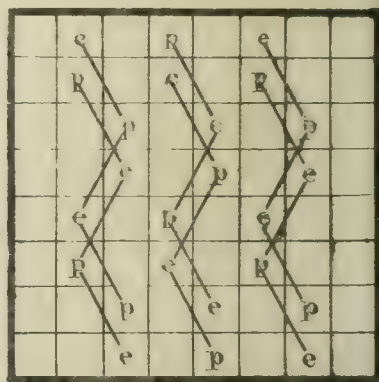
I.



F



L	e	a	p	L	e	a	p
a	p	L	e	a	p	L	e
e	L	p	a	e	L	p	a
p	a	e	L	p	a	e	L
L	e	a	p	L	e	a	p
a	p	L	e	a	p	L	e
e	L	p	a	e	L	p	a
p	a	e	L	p	a	e	L

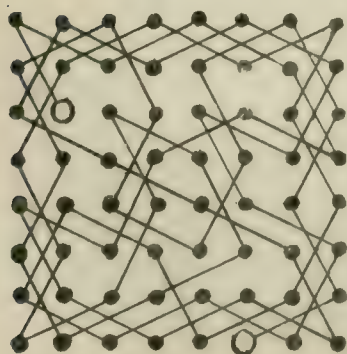


A

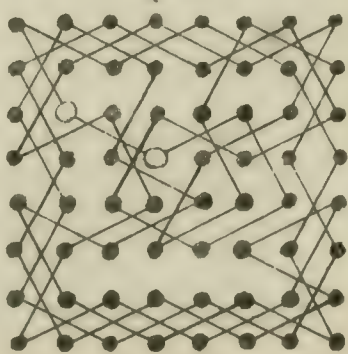
P

Examples.

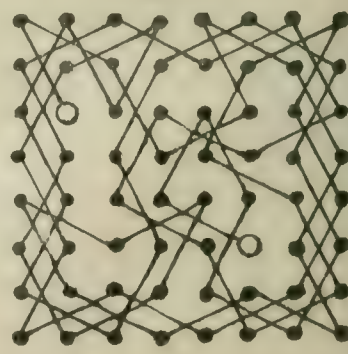
1



人



3



1 hour 10 min

LI. *Description of a Method of moving the Knight over every square of the Chess-board, without going twice over any one ; commencing at any given square, and ending at any other given square of a different colour. By P. M. ROGET, M.D., Sec. R.S.*

[Illustrated by Plate I.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE problem of carrying the knight, in a course of his own moves, over every square of the chess-board, without going twice over any square, has engaged the attention and exercised the ingenuity of mathematicians during the last hundred years*. Even the great Euler condescended to put forth a portion of his giant strength in grappling with the difficulties it presents, and bringing it within the grasp of his powerful analysis†. Vandermonde attempted to construct a general algebraic formula for its solution‡. Others, confining their efforts to the attainment of mere practical results, have contented themselves with the search of particular methods of resolving the problem in limited cases, and under the simplest conditions only; such as that of being obliged to commence the journey of the knight from a given square; one of the corner squares having usually been selected for that purpose§. The next step was the contrivance of methods fulfilling a further condition, namely, that the square at which the tour of the knight terminates shall be so situated as to be one move from the square from which it was begun. It is evident that whenever this has been accomplished, we have obtained a recurring or circular course, which the knight might again traverse by continuous moves: so that such a course gives us the power of commencing with any given square whatsoever, and of traversing through the whole series of 64 squares, until the entire circuit is completed.

Various circuits of this kind have been devised, and described in different memoirs, which have, from time to time, been published ||; and the problem, under this form, has been

* See Ozanam, *Récréations Mathématiques et Physiques, nouvelle édition*, Paris, 1750, tom. i. p. 260, where De Montmort, De Moivre, and De Mairan, are quoted as having treated this subject.

† *Mémoires de l'Académie de Berlin* for 1759, p. 310.

‡ In a paper entitled "*Remarques sur les Problèmes de Situation*," in the *Mémoires de l'Académie Royale des Sciences*, 1771, p. 566.

§ This is the point at which the problem is left by Ozanam.

|| See *Journal of Science and the Arts*, iii. 72. March, 1817; and also *Edin. Phil. Journal*, iv. 397, ix. 236.

deemed to possess sufficient interest to induce those who are curious in these matters to bestow pains in inventing expedients for impressing some particular circuit on the memory, so as to enable the possessor of this clue to guide the knight through the mazes of his devious route, without reference to chart or compass. In a memoir, which has appeared in Frazer's Magazine for the present month*, which I have just now seen, a method is recommended for attaining this object, which consists in designating each square in the board by a different syllable, composed of certain consonants and vowels, indicating the horizontal and vertical columns in which it stands. The whole series of these 64 arbitrary syllables, joined into 16 words, pointing out the sequence of the squares in the circuit, but void of any other meaning, is required to be learned by heart; by an effort similar, and not less distressing than that by which we strive to gain possession of the chronological epochs of the kings of England, when committing to memory the barbarous cacophonies of Grey's *Memoria Technica*.

It does not seem to have occurred to any of those who have hitherto favoured the world with the results of their speculations, that the problem in question would be rendered more general, and consequently more curious, by imposing, in addition to the unlimited assignment of any square for the commencement of the moves, the further condition that they shall terminate at any other given square of an opposite colour †. A great many years ago, I contrived a method by which the problem, in this new and extended form, may be resolved with the greatest ease; and the attention of the public having now been again called to the subject by the last-mentioned paper, I have thought that the communication of my method might not be unacceptable; especially as it depends on a principle which not only furnishes the means of constructing an incalculable number both of recurrent and of non-recurrent circuits, but also admits of very general application to the problem of the knight's move. It is founded on the following considerations.

Conceive the chess-board to be divided into four quarters by a vertical and horizontal line, both passing through its centre,

* Entitled "Chess without a Chess-board, by a Chess Player," p. 316.

† That the initial and the terminal squares must, of necessity, be of opposite colours, will be evident from the consideration that, as the total number of squares, namely 64, is an even number, and as the knight's moves are always alternately from white to black, and from black to white, the terminal square must be one of a different colour from that at which the moves commenced.

as shown in the central diagram in the group marked L E A P, Plate I. and let the squares in each quarter be considered as grouped together into four sets, designated severally by the letters L, E, A, P; thus,

L	E	A	P
A	P	L	E
E	L	P	A
P	A	E	L

It is here to be observed, that each of these sets of squares, marked with the same letter, constitutes a recurrent circuit of four moves of the knight.

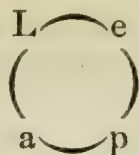
The squares in the other three quarters being similarly designated, as shown in the central diagram already referred to, it will be found that the several sets in each admit of being connected by knight's moves with the corresponding sets, similarly designated in the adjacent quarters. This is shown in the corner diagrams, L, E, A and P, where the connexions among the squares of each set are marked by oblique lines joining their centres*. The sets, thus connected, constitute four separate systems, of 16 squares each; and it will also be found that these 16 squares are so disposed that the knight may, in each system, perform the circuit of all its squares, beginning from any one given square, and ending at any other of a different colour. A few trials will soon satisfy the learner that, in every case, this may very easily be accomplished, and generally in a great variety of ways.

It will next be perceived that the knight can always pass from any of the squares, (excepting those situated at the corners of the board) of one system denoted by a consonant, to those of a system denoted by a vowel, and contrariwise; (as is shown by the diagonal lines in the four diagrams intermediate to the former); but not from vowel to vowel, or from consonant to consonant. From the corner squares, the move can only be made to squares belonging to the same system.

The solution of the proposed problem includes three cases :

1. If the given initial and terminal squares belong, the one to a system denoted by a consonant, and the other to a system denoted by a vowel, then, following the order of the letters when arranged in a circle, thus :

* The white squares have a circle, and the black a dot in their centres.



and proceeding either to the right or left, as the case may be, the circuit of each system must be gone over in succession, according to that order: beginning with that system to which the initial square belongs, and ending with that of the terminal square; taking care, however, for the reason above given, to avoid ending the intermediate circuits at a corner square. It will be advisable also to avoid ending these circuits at a square situated on the borders of the board, for they will not always admit of a transition being made from them to the next system into which we have to enter.

2. If both the given initial and terminal squares belong to the same system, omit, while going over that system, the terminal square, and also one in immediate connexion with it *; and fixing on some square in another system, which may be connected with it, proceed as before, taking care to end at this last-mentioned square; whence, when the rest of the entire circuits are completed, the two omitted squares may be attained, and the conditions of the problem satisfied.

3. If the initial and terminal squares belong, both of them, either to systems denoted by consonants, or to systems denoted by vowels, the same course with that just described must be pursued when the system to which the terminal square belongs is gone over, and with the same ultimate result.

Examples of each of these cases are given in the three lower diagrams, the path of the knight in his course over the board being traced by oblique lines joining the centres of the squares he traverses; the commencement and end of each course, which are supposed to be previously given, being marked by a small circle. I have made the second an example of a recurrent circuit, in order to show that this condition adds no new difficulty, and makes no difference in the mode of proceeding.

In these examples, the given initial square is the same in all of them, and belongs to the system L. In No. 1, the terminal square belongs to the system A. Here, we first go over the whole sixteen squares of system L; thence, passing over to system E, we traverse all the squares of that system. We next enter system P, covering in succession all the

* The omission of this second square is not absolutely necessary, but will generally be found to facilitate the subsequent operations.

squares belonging to it; and we, lastly, come to system A, where we find no difficulty in ending the course at the given terminal square.

In No. 2, where the terminal square belongs to system L, as well as the initial, we must, in going over that system, omit that square, and also one connected with it. With this exception, we are to proceed as before, traversing in succession the systems L, E, P and A; and taking care to end at a square of the latter, connected by a knight's move with the other omitted square of system L.

In No. 3, where the terminal square belongs to system P, the same series of courses is to be pursued, excepting that the squares to be omitted will belong to that system.

Your obedient servant,

39, Bernard Street, Russell Square,
March 20th, 1840.

P. M. ROGET.

LII. *Observations on the Precipitation of Copper by Voltaic Electricity.* By EDWARD SOLLY, Jun. In a Letter to Richard Taylor, Esq. &c.

THE beautiful discoveries of Professor Jacobi and Mr. T. Spencer have, as it were, laid the foundation of an entirely new art, namely, that of copying works in metal, without heat, without pressure, and at a very small expense. As many of your readers may not have seen either Mr. Spencer's very interesting and ingenious pamphlet, or any detailed account of the process which he has so successfully employed in copying medals, copper-plates, &c., I will briefly sketch the principles of the process of voltaic precipitation, and describe the apparatus required for the purpose, introductory to a short account of some experiments on the subject which I have made.

When a piece of tin or other similar metal is immersed in a strong solution of sulphate of copper or blue vitriol, it soon becomes coated with metallic copper, which is said to be precipitated or reduced; the oxide of copper being decomposed by the more oxidizable metal having a stronger affinity for oxygen than the copper itself has. In this way the tin becomes oxidized and dissolved, and the copper is reduced and precipitated in the form of a thin film. If the whole surface of the tin were to become coated with copper, of course this action would cease, because the former being entirely cased in copper, would remain inert, and in fact represent a plate of that metal. From the mode in which this precipitation is caused, it follows that the metal precipitated must be everywhere in

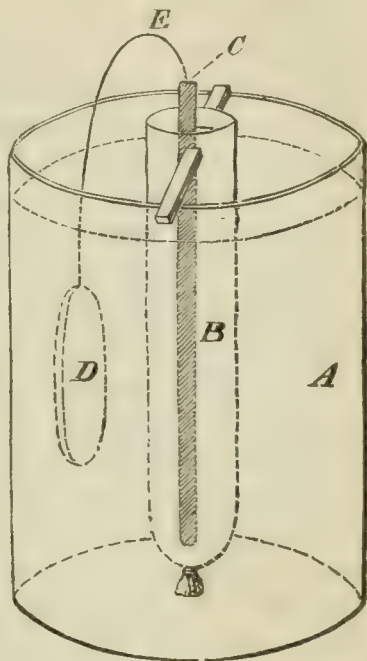
perfect contact with the surface on which it is thrown down; and thus we here have the first element of the process of copying, or obtaining a cast by precipitation, but extremely crude and imperfect. The film of copper so thrown down would be so thin and fragile that it would be impossible to remove it from the surface of the tin; or if it should acquire any thickness it would of course do so at the expense of a considerable quantity of tin, because for every portion of copper precipitated a corresponding quantity of tin must have been dissolved; the smoothness of the surface would become destroyed; and as this corrosion does not act equally all over the surface, if any design had been traced upon it, it would be much weakened, and in some parts wholly obliterated.

Both these difficulties are easily overcome in consequence of the facilities which electricity gives us, of, as it were, making chemical action portable; of generating it in one place, conveying it along metallic conductors, and making use of its power in another. Hence we are enabled to employ at pleasure one of the most powerful known deoxidizing agents, hydrogen. By means of electricity, we are able to give to any piece of metal, the power of evolving hydrogen, under the most favourable circumstances, from its surface, whilst immersed in a cupreous solution; and to continue that action, until the coat of deposited copper, precipitated by the action of the nascent hydrogen, has acquired any degree of thickness.

In order to effect this, a piece of some highly oxidizable metal, such as zinc, is connected by a wire soldered to its one end with the plate of lead, tin, or other metal, on the surface of which is engraved the design proposed to be copied. A vessel of any shape and material is divided into two portions by a partition or diaphragm of membrane, unglazed earthenware, or any other porous substance; the one division being filled with a strong solution of sulphate of copper, and the other with dilute sulphuric acid. The zinc is then placed in the dilute acid, and the mould or form to be copied, in the solution of copper. The zinc continues to dissolve in the acid, and generate the power, which being conveyed through the solutions to the surface of the mould, there causes the precipitation of copper, whilst the wire joining the zinc with the metallic mould, forms the connexion necessary to complete the galvanic circle.

The apparatus for this purpose is exceedingly simple, cheap, and easy of management; and when once arranged, and set in action, requires no further attendance until the operation is complete, when the mould is to be removed and the copy separated from it.

A very convenient form and arrangement is the following: *A* is a glass or earthen vessel containing a quantity of a saturated solution of sulphate of copper; *B* a piece of gut or tubular membrane formed into a bag by being tightly tied at the lower end and secured in a vertical position in the middle of the jar *A*, by means of a stick, which passing through two holes in it, rests upon the top of the jar. This bag is filled with dilute sulphuric acid, and contains the zinc rod *C*, which is likewise supported by the stick. *D* is the mould to be copied, and *E* the metallic wire connecting it with the zinc. Matters being thus arranged the precipitation goes on rapidly, and all that is requisite is to take care that the solution of sulphate of copper does not become too weak; when this happens, the copper comes down in a pulverulent and finely divided state, without any cohesion; falling off from the surface of the mould in the form of a bulky powder, and rapidly reoxidizing. This effect also happens when the surface on which copper is to be precipitated is very small, compared with the size of the zinc and the strength of the acid. In the first case it is easily prevented by always keeping excess of undissolved sulphate of copper in the solution to supply that which is decomposed.



When the deposited metal has acquired sufficient thickness, it is easily removed from the surface of the mould, by gently loosening its edge all round with any sharp instrument, after which it may be readily separated. Mr. Spencer has ingeniously availed himself of the different expansibility by heat of different metals, in removing the deposited metal from the mould. When copper is to be precipitated in a copper mould, he recommends rubbing into the surface of the mould a very small quantity of beeswax, the copper being previously warmed. In similar cases I have found that a small quantity of plumbago well rubbed over the surface completely prevented adhesion.

The form of apparatus above described, has I believe an advantage over that first proposed by Mr. Spencer, in the vertical position of the mould. When the mould is placed horizontally beneath the zinc rod, it is more liable to become

fouled with dust and impurities of various kinds which can hardly be kept out of the sulphate of copper, and which becoming gradually covered over by reduced copper, cause in the face of the metal when finished, the appearance of black specks; this is avoided in the form described. Another convenience is that two, three, or even four moulds may be operated upon at once, and any one may be removed at pleasure without disturbing the others. I have been thus minute in describing the apparatus requisite for the process, because it is very commonly supposed, that as the process is called "Voltaic precipitation," a powerful voltaic battery, complicated and expensive apparatus, and a complete knowledge of electricity is requisite for its performance; whilst in fact the great beauty of the process consists in its extreme simplicity, requiring only a slight acquaintance with the most elementary laws of that science.

When the mould employed is perfectly clean and sharp, and the process has been properly conducted, the copy obtained is of pure and brightly metallic copper, usually of a pink colour. If a copy of a medal thus taken, and after having been removed from the mould have a small quantity of copper precipitated upon its face, it assumes a most beautiful dead silky lustre, which with very little if any injury to the sharpness of the work gives it a very beautiful play of light and shade.

The colour of the precipitated copper appears to be very much influenced by the nature and condition of the mould; and by paying attention to this circumstance, it may be obtained of a great variety of shades of colour. I have sometimes thought, that the colour of a voltaic cast of a medal is dependent on the nature of the metal, of which the original medal is composed; because I frequently observed that copper precipitated in a fusible metal mould made from a silver medal, had a remarkable whiteness, whilst those similarly made from copper medals were red, and from gold had a yellow colour. On endeavouring to ascertain whether these effects were really dependent on the nature of the original medal, I found that so many little causes seemed to influence the results, that it was almost impossible to draw any certain conclusions with regard to these curious peculiarities of colour. The surface of the deposited copper is exceedingly apt to tarnish from exposure to air, frequently becoming partially bright orange, and sometimes even of a brilliant red colour. When heated up to nearly a red heat it acquires a uniform iron grey colour which is perfectly permanent. The precipitated metal is rather brittle, though very elastic; but by

heating it and allowing it to cool slowly it becomes tough and flexible.

In this process it is evident that a metallic surface is requisite for the commencement of any precipitation of copper; the arrangement in fact forms a single cell of a Daniell's battery, and is incomplete with the presence of the surface of the second metal in the sulphate of copper.

Mr. Spencer has shown, however, that moulds for the precipitation of copper may be made of any substance by gilding them or otherwise covering their surface with a thin film of metal which affords a conducting surface for the first portions of copper to be precipitated upon. My attention was early directed to this part of the process, because it seemed to open a wide field for new and beautiful applications. I was induced to pay particular attention to the deposition of copper upon non-metallic surfaces, and in consequence made numerous experiments to ascertain the circumstances most favourable to its precipitation under these conditions. My first experiments were made on surfaces of plaster of Paris, which I endeavoured to coat with copper, so as in fact to convert plaster casts into bronzes. I commenced by gilding the surface with different metals in the manner proposed by Mr. Spencer, but I found it exceedingly difficult to get a perfectly smooth and uniform surface; the process succeeded best with gold-leaf, but even that had its objections, and was besides very expensive. Subsequently I tried metals, such as bismuth and antimony in a state of very fine division, ground up with water and glutinous matters; these attempts were however not much better than the first trials.

In the course of these experiments I observed a curious fact, which I had not at all anticipated, and which very materially assisted me in attaining the objects which I had in view. When I had endeavoured to precipitate lead from a solution of one of its salts, in the same way that I had been doing with copper, I found that small grey crystals of lead soon formed upon the most prominent parts of the metallic mould I was employing, and which happened to be a leaden cast of a medal: these crystals rapidly increased in size, extending towards the membrane bag containing the zinc, which was about 3 inches distant from the mould. As soon as the crystals reached the surface of the membrane they bent about in various directions, crossing and recrossing each other until they had completely enveloped the membrane in a net-work of reduced lead. Again, when silver is precipitated from the fused nitrate, by electricity, the crystals formed at the one electrode extend across the fused electrolyte, until having

reached the opposite electrode they complete the metallic circuit and prevent further decomposition. In the same way I expected that when copper was deposited against a badly-conducting surface, it would increase much more rapidly in the direction towards the zinc rod, and that it would have but very little tendency to increase sideways; but I found, on the contrary, that the deposited copper had a remarkable property of extending by its edges far more rapidly than it increased in thickness; seeming to creep along or cling to the surface of the plaster or other non-conducting substance, against which it was being precipitated: and even when the plaster surface was placed at an angle of 45° to the zinc rod, and the deposition commenced in the centre, where a piece of gold-leaf had been applied, the copper extended equally all round and quite as fast on the side, where it receded from the zinc as on that part where by increasing it approached it. Following out this circumstance, I soon found that by very slightly improving the conducting power of the surface of the plaster or other non-conducting substance, I was enabled to precipitate copper without any gold-leaf or other metallic surface for commencement. The degree of conducting power requisite for this purpose was very slight, all that was necessary being to wash the surface over once or twice with a solution of nitrate of silver or muriate of gold, drying and well blacking each successive coat by exposure to light, the surface having previously been well rubbed over with a small quantity of plumbago. When thus prepared and placed in the solution of sulphate of copper, it was sufficient to touch any part of it with the wire attached to the zinc to cause the precipitation; a small ring of copper soon formed on the blackened surface round the wire, which increasing in size, in time covered the whole surface which had been prepared. When the deposit of copper had reached the edge of the prepared surface it still continued to increase, but more slowly, extending around the edges, even on to the back of the plaster, and accommodating itself to all the inequalities of its surface almost as perfectly as if it were metallic. In this manner I have caused it to be precipitated along the surface of card, paper, and on a variety of the most delicate and easily destructible organic substances. Indeed I have frequently seen it, when arrested by an air-bubble, gradually surround and envelope it, and thus form a perfect cast; the process going on with sufficient rapidity, and yet without disturbing so frail a form.

Some of these experiments are interesting when viewed in relation to certain phenomena of fossilization, not merely in

cases where organic matters are replaced by pyrites, but also in those where silica and other earthy substances are concerned.

By this process I was easily enabled to cover the surface of any article moulded in plaster, sulphur, wax, or any other substance; but it usually happened that by the time that the whole surface was covered some parts had begun to throw up little mammillated excrescences of copper which destroyed the smoothness and regular appearance of the surface. Although I thus failed in my original purpose, yet I saw sufficient reason to feel no doubt that plaster may be covered with a uniform coating of copper. The surface of the copper precipitated against the plaster is of course smooth, and therefore the process might be conveniently employed in any case where one or only a few copies are required of any metallic surface. Thus finger plates for doors, and all kinds of thin ornamental metal work, may be copied with great perfection.

A very beautiful effect is produced by coating the surface of facsimiles of medals or casts, made of lead or fusible metal, with a thin film of reduced copper; they then exhibit the beautiful silky dull appearance which I have before alluded to as being possessed by the precipitated metal. If these could be preserved from tarnishing by the application of any varnish or lacquer, exceedingly beautiful and cheap ornaments might be made in this manner, such as clock cases, &c.

I have likewise been engaged in a series of experiments on the precipitation of other metals by similar means, and shall probably, when sufficiently at leisure, prepare a short account of them.

38, Bedford Row, March 2, 1840.

LIII. *On the Galvanic Properties of the Metallic Elementary Bodies, with a description of a new Chemico-Mechanical Battery.* By ALFRED SMEE, Esq.*

LAST May a number of experiments were performed upon the galvanic properties of the non-metallic elementary bodies, and these were attended with the acquisition of some curious information†, but till lately no opportunity has presented itself of extending the series of investigations then conducted: now, however, that I believe that I can lay before

* Communicated by the Author.

† The results connected with this part of the subject will be given in a future number.

the public a valuable battery, no time is lost, that others may extend and improve the new principle about to be detailed.

With regard to the metallic elementary bodies, their properties have been investigated so frequently, and to such an extent, that it may seem unnecessary to draw attention again to them; but two circumstances influencing their action have never been noticed. It is well known that the positive metal should be the most readily acted upon by the solution, and the negative the least, and the further these are apart, the more forcible will be the battery: thus, *cæteris paribus*, platinum and zinc are more powerful than iron and zinc; but if a circuit be made of a piece of smooth platinum and zinc it will sometimes happen that the effect is less than when a circuit is formed by a similar piece of iron. Now this appears at first sight paradoxical, though it can in many instances be easily explained; for if the platinum be carefully examined, it will be seen that the acid solution does not really wet the platinum, but runs off from the greater part of the surface, as metallic mercury does from glass. In this state, a piece of platinum having a surface of thirty-two square inches, formed into a battery with amalgamated zinc and connected with a magnet, supported three-quarters of a pound through five thicknesses of paper; when the same piece of platinum was heated or dipped in nitric acid and afterwards well washed, it supported a similar weight through twelve thicknesses of paper, thus being less powerful than iron in the first instance, and more so in the second. In the same way, silver supported under the like circumstances, the keeper of a magnet through three layers of paper: on being heated and again wetted, the attractive force was exerted through nine thicknesses of paper, but no additional power was gained by removing the surface of the silver by nitric acid. The metals in these cases appear to become coated with a film of air, which effectually prevents the contact of the fluid. This is also seen in the various forms of charcoal, which after ignition are very powerful, but lose much of their force if long exposed to the air; their energy however is restored upon their being again heated.

As in the experiments just detailed, and in those which I am about immediately to describe, the relative powers of the arrangements have to be considered, it will be proper to mention in what way the results were obtained. A soft iron horse-shoe magnet was suspended, round which covered wire in communication with the poles of the battery was wound: the keeper, which weighed three quarters of a pound, was separated from the poles of the magnet by as many layers of thin blotting paper as could be used without its falling; thus with

a battery of feeble force few layers of paper could be interposed; but with one of greater strength, forty or sixty thicknesses might be used. A similar form of apparatus might easily be devised, which would show by means of a delicate screw the exact distance at which a given weight would be supported by the attractive force of the induced magnet.

The influence of different conditions of surfaces is a subject which has escaped all experimenters. Now this is singular, for many must have noticed, that in a circuit, the greatest quantity of gas is given off at the corners, edges, and points. Following this hint, a piece of spongy platinum, consisting as it does of an infinity of points, was placed in contact with amalgamated zinc, when a most violent action ensued, so that but little doubt could be entertained of its forming a very powerful battery. The fragile nature of this material precludes it from being thus used, and therefore it was determined that another piece of platinum should be coated with the finely-divided metal. This experiment was attended with a similar good result, and the energy of the metal thus coated was found to be surprising. To test the value of this process, a piece of platinum, thus platinized, was placed in dilute acid in contact with amalgamated zinc, and the quantity of gas evolved in a given time was noticed.

		C.I.
Platinized platinum	7 sq. inches	gave off 5 per 1 minute
Platinum heated	ditto	1 per 1 minute
Platinum covered by air	ditto	1 per 6 minutes
Platinized coke	small piece	3 per 5 minutes
Plain coke	ditto	1 per 25 minutes.

In these experiments the contact was made in each cell alike; the same zinc being used, and the distance being the same between the metals. The energy of the metal thus prepared upon the soft iron magnet is very great. A piece of platinum exposing thirty-two square inches of surface, supports three-quarters of a pound through seventeen thicknesses of paper, whilst when smooth and wetted it supported it through eleven layers; and when no care was taken about its being wet, but when simply plunged into the liquid, only through five layers of the same paper.

The cause of this increase of power appears to be the facility given to the evolution of the gas from the number of points, and not from an increase of surface, as but little benefit attends its application in the nitric acid batteries, in which the hydrogen is not evolved, but absorbed by the fluid.

The next point which we have to consider, is whether other finely-divided metals have the same good effect; but no other

of the many metals that I have tried can be used with similar good results, except palladium, which though it has not much effect in the sponge, is found when precipitated on platinum or silver to possess powers, about equal to the finely-divided platinum. Precipitated silver increases the power of the metals, but not nearly to the extent of platinum.

Having ascertained that a solution of platinum must be used for increasing the power of metals in their ordinary state, it becomes a matter of great importance to ascertain whether the platinum may be precipitated upon other metals with advantage; and for this purpose it was deposited upon earthenware, palladium, pure silver, copper plated with silver, nickel, German silver, tin, lead, brass, cast iron, sheet iron, steel, zinc, and charcoal. The platinized earthenware was not found to answer, apparently from the quantity of the metal not being sufficient to carry the electricity. Palladium, silver, and plated silver answered equally well with platinum to receive the precipitated metal, and if there was any difference, I think the silver was rather the best. Plated copper answers very well, but care should be taken to varnish every copper edge, or else that metal is apt to be slightly dissolved, and deposited again upon the platinized silver, which is injurious. Should copper, from any cause, get upon the silver, it may be dissolved by a little muriatic acid, and afterwards by a little strong ammonia. No other metal or alloy besides this answered for the reception of the platinum, except iron, and this was as active as silver for a time, but then a local battery was formed between the platinum and iron—the iron was dissolved and the battery destroyed. In some cases this does not take place so rapidly as in others. Carbon answers admirably for the reception of the platinum, and is improved in like manner.

We have now the elements for the manufacture of a powerful battery; for we have seen that increase of power is obtained by taking care that the negative metal is thoroughly wetted by the fluid, and that this is not only accomplished, but its power materially increased by the numerous points formed by the precipitation of finely-divided platinum. Whatever metal, alloy, or compound may be found hereafter to succeed for the reception of the platinum, or whatever metal may be found to answer instead of the finely-divided platinum, still the principle by which the advantage is gained will be the same. However, the battery which I now propose is to be made of either copper plated with silver, silver, palladium, or platinum. The silver can be rolled to any thinness, and therefore is not expensive. Each piece of metal is to be

placed in water, to which a little dilute sulphuric acid and nitro-muriate of platinum is to be added. A simple current is then to be formed by zinc placed in a porous tube with dilute acid; when, after the lapse of a short time, the metal will be coated with a fine black powder of metallic platinum. The trouble of this operation is most trifling; only requiring a little time after the arrangement of the apparatus, which takes even less than the description. The cost I find to be about 6*d.* a plate of 4 inches each way, or 32 square inches of surface. This finely-divided platinum does not adhere firmly to very smooth metals, but when they are rough is very lasting, and sticks so closely that it cannot be rubbed off. On this account, when either silver is employed, or copper coated with silver, the surface is to be made rough by brushing it over with a little strong nitric acid, which gives it instantly a frosted appearance, and this, after being washed, is ready for the platinizing process.

With regard to the arrangement of the metal thus prepared great diversity exists; it may be arranged in the same way as an ordinary Wollaston's battery with advantage; a battery thus constructed possessing greater power than Professor Daniell's battery: four cells, containing 48 square inches in each cell, decomposed 7 cubic inches of mixed gas per five minutes, whilst four cells of Professor Daniell's, in which 65 square inches of copper were exposed in each cell, gave off only five cubic inches in the same time. However, in my battery thus arranged, the action dropped to 5 cubic inches in five minutes, but it resumed its power after the contact had been broken for a few seconds. This battery also possesses great heating powers, raising the temperature of a platinum or steel wire, 1 foot long and of a thickness similar to that used for ordinary birdcages, to a heat that could not be borne by the finger*. Its magnetic power is not less astonishing, three cells supporting the keeper of a magnet through forty-five, two cells through thirty-two, and one cell through twenty thicknesses of paper. An electro-magnetic engine was made to rotate with great velocity, the combustion of the mercury at the breaking of contact being exceedingly brilliant.

A battery of this construction should be in every laboratory, to be used in most cases where a battery is wanted, and the slight labour attending its operation is scarcely worth mentioning. I have used one for 48 hours consecutively without the slightest alteration either of the fluid, or in the arrange-

* A small pot battery of six cells fairly fused into globules 2 inches of iron wire, and the combustion of different metals was extremely brilliant, when the battery was in combination with a Bachoffner's apparatus.

ment of the metals, and the diminution attending its operation appeared to arise from deficiency of acid, for it was instantly restored by a little strong sulphuric acid in each cell. Where the battery is required to possess the same power for a long period, it might be advisable to separate the metals by a porous earthenware vessel, or what answers the purpose equally well, by a thick paper bag, the joinings of which must be effected by shell-lac dissolved in alcohol. By these means, the sulphate of zinc is retained on the zinc side of the battery. The use of porous tubes, however, appears from observation, as far as my battery is concerned, to be nearly superfluous, at any rate in most cases; for I find, that after a battery arranged as Wollaston's had been at work in the same fluid for forty-eight hours, it had no zinc deposited on the silver. It is worth remarking, that during the last 24 hours contact had not been broken for a single instant. Notwithstanding these experiments, however, it may be as well in an extensive battery to use porous plates.

The battery may be arranged like the pot batteries, but I should greatly prefer the troughs, such as used for Wollaston's batteries, from the convenience of packing, and from a battery of the same surface requiring so small a space. A battery may be constructed to form a most powerful calorimotor. It may also be arranged as a circular disc battery. Or it may be made as a Cruickshanks, each cell being divided or not by a flat porous diaphragm. Whatever arrangement is adopted, the closer the zinc is brought to the platinized metal, the greater will be the power.

The generating fluid which is to be employed is water, with one-eighth of sulphuric acid by measure; and the zinc ought always to be amalgamated in the first instance, as that process will be found very economical from its stopping all local action, and the amalgamation will be found not to require repeating, because there is no fear of copper being thrown down on the zinc, which occasionally happens in the sulphate of copper batteries.

The battery thus constructed is the cheapest and least troublesome in action that has ever been proposed, and from the smallness of its bulk will be found very valuable to electro-magneticians. It is second in power only to the nitric acid batteries, the objections to which have been already noticed. For medical purposes, with a Bachoffner's apparatus, a battery composed of platinized silver two inches each way will be found sufficient.

To recapitulate the processes of the formation of a battery: first the platina, silver, or plated copper must be roughened, the two latter with nitric acid, and afterwards washed. The

metal is next to be placed in an acid solution with a little nitro-muriate of platinum, which metal is to be thrown down by the formation of a simple galvanic circuit; and lastly, the platinized metal is to be formed with amalgamated zinc into a battery, either with a porous tube or paper bag, or without them, according to the fancy of the operator, or the purpose for which it is wanted.

The advantage from this form of battery arises, as I believe, from a mechanical help to the evolution of the hydrogen; and therefore it is proposed to call it the Chemico-mechanical battery. This battery may remain in the acid for a length of time, and neither the amalgamated zinc nor platinized silver will undergo the slightest change, and the whole will be as silent as death. Let only communication be made, the liquid in each cell becomes troubled;—it boils—it bubbles, and produces the effects which have been detailed. The quantity of electricity passing through either wires or liquids may be pretty accurately judged from the action taking place in the battery, for if the communication be made through a liquid of difficult decomposition, or through long small wires (70 or 80 feet), but little gas will be given off from the platinized metal, but when short thick wires are used the action is violent. A galvanometer might be constructed of one cell, similar to the oxygen cell of Professor Daniell, as this would show the exact amount of electricity passing.

The importance of constructing a battery that shall be small in compass, efficient in action, cheap in its operation, and devoid of troublesome manipulation, is important in the highest degree; and I consider that my chemico-mechanical battery will be found frequently a useful means of obtaining gases for the oxy-hydrogen light. Its value for blowing up vessels under water, and exploding powder in mines, is sufficiently obvious.

In conducting the extensive series of experiments, of which this is a summary, the grand features have been rather attended to than very minute results; and in conclusion, it would ill befit me if I did not here mention the valuable assistance I have received by the loan of apparatus, &c. from many individuals, but most especially from Professor Daniell, William Terry, Esq., and Mr. E. Palmer.

Bank of England, Feb. 29, 1840.

LIV. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* By M. DUMAS.*

IN this memoir, I propose explaining and discussing different rules and their consequences which have so often been the subject of important communications to the Academy, for I should think it useless to call upon its kind attention, if the developments, into which I have been obliged to enter, had not given it an unusual length. But the Academy will pardon me, when it knows the importance and the variety of the questions which I have been forced to unite in it, and which are the following:

“ 1. In every combination, can the elements have their place supplied, equivalent for equivalent, by simple bodies or by compound bodies, which act their part?

“ 2. Are not these substitutions often effected, without the general nature of the compound being altered by it; the bodies thus produced belonging to the same *chemical type* as those from which they are derived?

“ 3. In other cases, can these substitutions furnish products entirely distinct in their actions (*réactions*) from those which gave them birth, and is it then right to consider them notwithstanding as belonging to the same *molecular type*?

“ 4. Can the nomenclature of organic substances be remodeled, from the present time, in such a manner that the name of each body shall express the chemical type, or even the molecular type to which it belongs?

“ 5. Do the phenomena of substitution oblige us to modify profoundly the value affixed until lately to the organic radicals?

“ 6. Is not the electric function (*rôle*) attributed to the elements of compounds by the electro-chemical theory, in complete contradiction to the phænomena of substitutions?”

I shall subject each of these questions in succession to an attentive examination, applying myself to what is general and elevated, without entering into technical details, which will take their place in special memoirs.

Law of Substitutions.

Some years ago, M. Gay-Lussac mentioned a very simple experiment in his lectures, which has become a point of departure for an immense succession of inquiries and discoveries. In treating wax by chlorine, said the illustrious professor, I saw this substance lose some of its hydrogen,

* From the *Comptes Rendus de l'Académie des Sciences*, 1840, prem. semest. No. 5, Feb. 3.

and take exactly an equivalent volume of chlorine to that of the hydrogen set free. I had myself subjected oil of turpentine to similar trials, and was convinced, agreeably to M. Devielle's late re-examination of the subject, that it easily loses eight volumes of hydrogen and takes in their place eight volumes of chlorine, thus constituting the compound $C^{40} H^{24} Ch^8$, derived from the original oil of turpentine $C^{40} H^{32}$.

At the same time, I studied the composition of some extraordinary products obtained from alcohol, viz. chloral, chloroform, bromoform, iodoform, of which I gave an exact analysis, and endeavoured to explain their formation.

This work was the occasion of the law of substitutions being developed for the first time. But as it was then believed that certain organic matters, and alcohol in particular, contained water ready formed, the law of substitutions, in the form in which I first presented it, attributed a function to this water, which gave rise to many objections. To return to the details of this point, would be without interest at the present time, for those chemists who admit the reality of substitutions, have in general given up the supposition of the existence of ready-formed water in the compounds in which these substitutions are observed.*

Although the function which I had attributed to the water may be reconciled to the general phænomena of chemistry, as it is now become useless, we must limit the law of substitutions to the following expression: — "When a hydrogenated organic substance is treated with chlorine, bromine, iodine, or oxygen, &c., these bodies generally remove hydrogen from it, and for an equivalent of hydrogen so removed, an equivalent of chlorine, bromine, iodine, or oxygen is substituted in the compound.

Is this phænomenon general? has it a peculiar character? This is what we are about to examine.

At the present time every one knows that in the reciprocal action of bodies certain relations of weight are observed, and that it is not enough to say that sulphur or oxygen combine with or act upon zinc or lead, but that a quantity of sulphur weighing 201, and of oxygen weighing 100, act upon or combine with a quantity of zinc weighing 403, and of lead

* It may however be observed, that when I admitted that chlorine decomposed this water, seized the hydrogen, and left the oxygen in the compound, I made a very logical supposition. An analogous case presents itself when the benzoate of silver is decomposed by bromine, giving bromide of silver, the oxygen of the oxide uniting with the benzoic acid.

When I added that oxygen itself could decompose the water fixed in the compounds, I was guided by the theory of cementation, in which we admit that iron decomposes carburet of iron.

weighing 1294. These quantities are the chemical equivalents of those bodies; all chemical action takes place between them and their multiples.

Now, to say that from an organic compound, an equivalent of hydrogen may be subtracted, and that its place may be taken by an equivalent of chlorine, is manifestly announcing a law in perfect harmony with the general law of the reciprocal action of bodies by equivalents. Every one comprehends, that if a crystallized body could produce another, likewise crystallized, by losing hydrogen and gaining chlorine, which could not be represented by equivalents, we must conclude from this that the theory of equivalents is false. The law of substitutions ought to be in accordance with the theory of equivalents, as moreover the general expression which has been given to it suggests.

But from thence to assert that the law of substitutions has no peculiar character, that it is only a particular case of the theory of equivalents, there is either an equivocation or an immense leap. That this leap was made when the law of substitutions was at first put forth, that nothing allowed the cause of it to be foreseen, in order to connect it with a theoretical principle, I concede without difficulty. This also did not fail to be the case, and amongst the objections of the German chemists to the law of substitutions, it always figures first. The philosophers who some years ago viewed it in this manner, were right without doubt, but they must have been very much surprised to see so many skilful men persist in finding in it a special character.

With regard to myself, if I believed in the future prevalence of the law of substitutions, in its importance, five years ago, when I was the only one who defended it, it is not to be supposed that I can change my opinion, when the most eminent English chemist, Mr. Graham, adopts it without reserve; when M. Liebig, after sharply criticising it, now receives it as admitted in science; when so many labours, undertaken often to combat it, have come within this very circle, to give it a complete consecration; when, lastly, far from seeing in the law of substitutions a simple experimental fact, we are now able to ascend to its cause.

Thus, to assert, as M. Pelouze has done, that the phenomenon of substitution, when it is observed, is only a particular case of the theory of equivalents, is to announce as a novelty two things perfectly known, viz.: first, that in the action of two bodies, substitution does not always take place; secondly, that when it is effected, it takes place by equivalents. This does not hinder the phenomenon of substitution

from possessing a special character, from constituting a case of chemical action so particular, that it was absolutely necessary to distinguish it from every other, as I have done.

To be convinced that the theory of substitutions is not general, there is no need of new facts; it will suffice to read my memoir on chloracetic acid, which has been so often quoted for some time. We there see that besides the chloracetic acid produced by substitution by means of the action of chlorine on acetic acid, oxalic acid and carbonic acid are developed, the production of which by substitution we may be at a loss how to explain, at least for the present.

And better still, it suffices to glance at my memoir relative to indigo: we there see that the white indigo, under the influence of oxygen, loses an equivalent of hydrogen without gaining anything. In this case then there is no substitution; I convinced myself of it. At a later period MM. Liebig and Wœhler observed facts of the same nature in their admirable researches upon uric acid. Quite recently, Mr. Kane observed similar facts in the colouring matter of Heliotrope.

Thus, the phænomenon of substitutions is not general; still more, this is one of its most essential characters, as we shall presently see.

Not only it is not general, because a body may, under the influence of oxygen, lose hydrogen without gaining anything, but it is not general also for the contrary reason. Olefiant gas, for example, may lose 4 equivalents of hydrogen and take 6 of chlorine; every one knows this. Any one who had not analysed all the intermediate degrees of this action, as M. Regnault has done, would, in comparing the first and last term, have found the law of substitutions defective.

At present it may be explained and understood without difficulty, when we say that if white indigo loses hydrogen without gaining anything, it passes into a new molecular type; when we know that olefiant gas may produce a chloride of carbon of the same type as itself, and, by a fresh addition of chlorine, a new chloride of a different type. Thus, the law of substitutions prevails when the bodies preserve their initial type; it is no longer applicable in the contrary case; and, by this very means, it serves to distinguish the bodies which have preserved their molecular type from those which have lost it.

But there is no occasion to return to this explanation, which my wish to express myself clearly has induced me to give *en passant*, to justify the necessity of distinguishing the law of substitutions from other chemical actions (*réactions*).

The law of substitutions expresses, that in an organic body,

we may take away 1, 2, 3 equivalents of hydrogen, and supply their places by 1, 2, 3 equivalents of chlorine, bromine, iodine, or oxygen. It indicates that these substitutions will give birth to new bodies, the properties of which it is often possible to foresee. It makes known that these actions (*réactions*) are the easiest, the most frequent, the least changing (*altérantes*) that a body can undergo.

Before the law of substitutions was published, no one could have foreseen how a hydrogenated body would have acted under the influence of chlorine or oxygen. Now every one knows it, and a chemist performs in a few days, by means of this guide, operations which would have required years of labour before he had learned its use.

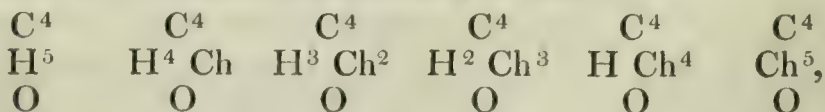
Ask the theory of equivalents what ought to take place when ether is subjected to the action of chlorine, and it will certainly reply that it knows nothing of the matter; or indeed, what comes to the same thing, it will show you a hundred possible cases between which you will have to choose.

For ether may lose in succession the five equivalents of hydrogen which it contains without gaining anything, which gives five new bodies; for it may, without losing anything, absorb 1, 2, 3, 4, 5, and many more equivalents of chlorine besides; which makes ten, twenty, thirty new bodies, if we desire it; for it may, in losing a single, or even two, or three equivalents of hydrogen, absorb equivalents of chlorine more or less in number; and in this third hypothesis, the number of compounds will become almost innumerable.

In fine, we should fall upon almost infinite varieties of combinations, if we add that the oxygen of ether may be eliminated, either free, in the form of water, or in the form of carbonic acid.

Thus the theory of equivalents announces the production of a prodigious quantity of compounds: provided the elements which the ether loses and those which it gains are represented by equivalents, it is sufficient.

It is otherwise with the law of substitutions. With regard to that, when ether loses hydrogen, it must receive chlorine. There are then only five compounds which are possible, of which the composition is perfectly foreseen.



Amongst them, three are already known, and there is not the smallest risk to run when we predict the probable discovery of the two others.

The law of substitutions sees then in these five compounds some of the *nearest*, the *most necessary* modifications of ether. The theory of equivalents sees there any modifications whatever more or less *possible*. The one says, these five bodies must be formed first very easily and very abundantly; the other says, they may be formed, and many others with them.

If the acetic acid is in question, the theory of equivalents would besides announce the possible formation of compounds so numerous, that nothing could guide the observer. The law of substitutions, more precise, foresees and predicts that in losing 1, 2, 3 equivalents of hydrogen, the acetic acid will take 1, 2, 3 equivalents of chlorine, and will thus produce three new compounds. One of them constitutes the so-called chloracetic acid.

In a multitude of possible actions which are nearly equally foreseen by the theory of equivalents, the law of substitutions discovers with certainty those which are about to be produced; it foresees them, predicts them, and up to the present time its help has truly been of invaluable efficacy.

How, without it, should we have been led to discover, one after the other, four or five mixed products, hardly differing from each other, in some actions recently studied? Otherwise, how would it have been possible to perceive, that the action which was to be produced had not been exhausted, if the formulæ, through the impossibility of making them agree with the law of substitutions, had not warned the observer?

Let me make a comparison drawn from a familiar order of ideas. Let us put ourselves in the place of a man overlooking a game at chess, without having the slightest knowledge of the game. He would soon remark, that the pieces must be used according to positive rules. In chemistry, the equivalents are our pieces, and the law of substitutions one of the rules which presides over their moves. And as in the oblique move of the pawns, one pawn must be substituted for another, so, in the phænomena of substitution one element must take the place of another. But this does not hinder the pawn from advancing without taking anything, as the law of substitutions does not hinder an element from acting on a body without displacing or taking the place of any other element which it may contain.

How can we believe that a knowledge of the rules which govern the game of our chess-board, is useless for the explanation of the moves which offer themselves, for the purpose of foreseeing those which are about to arise from the relations of the different pieces, of the various agents placed in contact?

These are the foresights, always justified by experience, which characterize the law of substitutions. If it be connected with the theory of equivalents, it then results that every chemical phenomenon is represented by equivalents, and that the facts of substitution are chemical phenomena; that every possible event in chemistry is translated into the language of equivalents, and that after all a *true* fact must be a *possible* fact. In the same manner that the possible comprehends the true, in like manner, and not otherwise, the theory of equivalents comprehends the law of substitutions.

So far, I have reasoned as if the law of substitutions only applied in reality to the substitution of hydrogen, which has furnished the first examples of it. But chemists know that in an organic substance not only can hydrogen undergo substitution, but also oxygen and azote, of which it would be easy to cite numerous examples.

Still more, we can cause carbon to undergo real substitutions, which sufficiently shows how artificial that classification of organic substances would be, which would rest solely on the permanency of the number of the equivalents of carbon in all the compounds of the same family.

In an organic compound, all the elements may then be displaced, and others substituted for them in succession. Those which disappear most easily, abstraction being caused by certain conditions of stability which we cannot yet foresee, are those of which the affinities are the most energetic. This is why hydrogen is one of the easiest to subtract and have another element substituted for it; this is why carbon is one of the most rebellious, for we know few bodies which can act upon carbon and not upon hydrogen.

In fine, I add that the law of substitutions allows us not only to foresee the disappearance of a certain number, or of all the elements of the organic compound, and new elements being substituted for them, but also the intervention, playing the part of the same elements, of certain compound bodies.

Thus, cyanogen, carbonic oxide, sulphuric acid, the binoxide of azote, nitrous gas, amidogen, and many other compound groups may intervene as the elements would do, take the place of hydrogen, and give rise to new bodies.

The law of substitutions is then an almost inexhaustible source of discoveries. It guides the hand of the chemist who trusts to it, it rectifies his errors by showing him the cause of them; and in a number of possible but uncertain actions, it points out some which are proximate, easy to produce, and of the highest interest.

This future, so rich in facts which may be realised, so full

of accessible discoveries, which the law of substitutions reveals to the eyes of the chemist, justifies a remark of my dear friend M. Ampère, having so warm a heart and a mind so rich in delicate perceptions. When I was speaking to him of the law of substitutions, he also, at first, confounded it with the ordinary equivalent actions; but when I had developed the views, still very incomplete, which I was already endeavouring to attach to them, "Ah! my friend," said he, "how I pity you! you have found work for your whole life." A prediction which would have been realised, if so many minds of a higher order, taking up the law of substitutions, had not given it a flight which makes my part of the work much less necessary.

[To be continued.]

LV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Feb. 13, **T**HE reading of a paper, entitled "Experimental Researches in Electricity, Sixteenth Series." On the source of power in the Voltaic pile. By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded.

The determination of the real source of electrical power in galvanic combinations has become, in the present state of our knowledge of electricity, a question of considerable importance, and one which must have great influence on the future progress of that science. The various opinions which have been entertained by philosophers on this subject may be classed generally under two heads; namely, those which assign as the origin of voltaic power the simple contact of dissimilar substances, and more especially of different metals; and secondly, those which ascribe this force to the exertion of chemical affinities. The first, or the theory of contact, was devised by Volta, the great discoverer of the Voltaic pile; and adopted, since it was promulgated by him, by a host of subsequent philosophers, among the most celebrated of whom may be ranked Pfaff, Marianini, Fechner, Zamboni, Matteucci, Karsten, Bruchardat, and also Davy; all of them bright stars in the exalted galaxy of science. The theory of chemical action was first advanced by Fabroni, Wollaston, and Parret; and has been since further developed by Oersted, Becquerel, De la Rive, Ritchie, Pouillet, Schœnbein, and others. The author of the present paper, having examined this question by the evidence afforded by the results of definite electro-chemical action, soon acquired the conviction of the truth of the latter of these theories, and has expressed this opinion in his paper, published in the *Philosophical Transactions* for 1834.

The author, after stating the fundamental doctrine laid down by

Volta, proceeds to give an account of various modifications in the theory introduced by subsequent philosophers ; and also of different variations in the views of those who, in the main, have adopted the chemical theory. Being desirous of collecting further and more decisive evidences on this important subject, he engaged in the series of experimental researches which are detailed in the present memoir.

It is assumed, he observes, by the advocates of the contact theory, that although the metals exert powerful electromotive forces at their points of mutual contact, yet in every complete metallic circuit, whatever be the order or arrangement of the metals which compose it, these forces are so exactly balanced as to prevent the existence of any current ; but that, on the other hand, fluid conductors, or electrolytes, either exert no electromotive force at their place of contact with the metals, or, if they do exert such a power, the forces called into play in the complete circuit are not subject to the same law of compensation as obtains with circuits wholly composed of metallic bodies. The author successfully combats this doctrine, by bringing forward a great number of instances, where certain fluids, which have no chemical action on the metals with which they were associated in the circuit, are in themselves such good conductors of electricity, as to render evident any current which could have arisen from the contact of the metals, either with each other or with the fluid ; the evidence of their possessing this conducting power being their capability of transmitting a feeble thermo-electric current from a pair of plates of antimony and bismuth. The following he found to be fluids possessing this property in a high degree ; namely, a solution of sulphuret of potassium, yellow anhydrous nitrous acid mixed with nearly an equal volume of water, very strong red nitric acid, and a mixture of one volume of strong acid with two volumes of water. By employing the solution of sulphuret of potassium as an electrolyte of good conducting power, but chemically inactive with reference to either iron or potassium ; and associating it with these metals in a circuit, formed by two test-glasses containing the solution, into one of which was immersed a plate of platina and a plate of iron, and in the other two plates of platina ; and the circuit being completed by wires of the same metals respectively, joining the iron-plate in the first glass with one of the platina-plates in the second, while the other two platina-plates were united by platina wires, interrupted at one part by a short iron wire which joined their ends ;—it was found by the test of an interposed galvanometer, that, as no chemical action took place, so no electric current was produced ; yet the apparatus thus arranged could transmit a very feeble thermo-electric current, excited by slightly raising the temperature of the wires at either of their points of contact. Hence, the inference may be drawn, that the contact of iron and platinum is of itself productive of no electromotive force. On the other hand, the author shows, that the interposition in the circuit of the smallest quantity of an electrolyte, which acts chemically on either of the metals, the arrangement remaining in all other respects the same, is imme-

diately attended with the circulation of an electrical current far more powerful than the thermo-electric current above-mentioned. A great number of combinations of other metals were successively tried in various ways, and they uniformly gave the same results as that of iron and platina. Similar experiments were then made with various metallic compounds, and also with other chemical agents; and in all cases the same general fact was observed; namely, that when no chemical action took place, no electrical current was excited; thus furnishing, in the opinion of the author, unanswerable arguments against the truth of the theory of contact. The only way in which it is possible to explain these phenomena on that theory, would be by assuming, that the same law of compensation as to electro-motive power is observed by the sulphuret of potassium, and the other fluids of corresponding properties, as obtains in the case of the metals, although that law does not apply to the generality of chemical agents; and in like manner, different assumptions must be made in order to suit the result in each particular combination, and this without any definite relation to the chemical character of the substances themselves; assumptions, which no ingenuity could ever render consistent with one another. At the conclusion of the paper, the author describes some remarkable alternations in the phenomena which occur, when pieces of copper and silver, or two pieces of copper, or two of silver, form a circle with the yellow sulphuretted solution; and which lead to the same conclusion as the former experiments. If the metals be copper and silver, the copper is at first positive, and the silver remains untarnished; in a short time the action ceases, and the silver becomes positive, at the same time combining with sulphur, and becoming coated with sulphuret of silver; in the course of a few minutes, the copper again becomes positive; and thus the action changes from one side to the other in succession, and is accompanied by a corresponding alternation of the electric current.

March 5.—The reading of a paper entitled, “On the Chemical Action of the Rays of the Solar Spectrum on Preparations of Silver and other Substances, both metallic and non-metallic; and on some Photographic Processes;” by Sir John F. W. Herschel, Bart., V.P.R.S., &c., was resumed and concluded.

The object which the author has in view in this memoir is to place on record a number of insulated facts and observations respecting the relations both of white light, and of the differently refrangible rays, to various chemical agents which have offered themselves to his notice in the course of his photographic experiments, suggested by the announcement of M. Daguerre’s discovery. After recapitulating the heads of his paper on this subject, which was read to the Society on the 14th of March, 1839, he remarks, that one of the most important branches of the inquiry, in point of practical utility, is into the best means of obtaining the exact reproduction of indefinitely multiplied facsimiles of an original photograph, by which alone the publication of originals may be accomplished; and for which purpose the use of paper, or other similar materials, appears to be essentially requisite. In order to avoid circumlocution, the author employs the

terms *positive* and *negative* to express, respectively, pictures in which the lights and shades are the same as in nature, or as in the original model, and in which they are the opposite; that is, light representing shade; and shade, light. The terms *direct* and *reverse* are also used to express pictures in which objects appear, as regards right and left, the same as in the original, and the contrary. In respect to photographic publication, the employment of a camera picture avoids the difficulty of a double transfer, which has been found to be a great obstacle to success in the photographic copying of engravings or drawings.

The principal objects of inquiry to which the author has directed his attention in the present paper, are the following. First, the means of fixing photographs; the comparative merits of different chemical agents for effecting which, such as hyposulphite of soda, hydriodite of potash, ferrocyanate of potash, &c., he discusses at some length; and he notices some remarkable properties, in this respect, of a peculiar agent which he has discovered.

2. The means of taking photographic copies and transfers. The author lays great stress on the necessity, for this purpose, of preserving, during the operation, the closest contact of the photographic paper used with the original to be copied.

3. The preparation of photographic paper. Various experiments are detailed, made with the view of discovering modes of increasing the sensitiveness of the paper to the action of light; and particularly of those combinations of chemical substances which, applied either in succession or in combination, prepare it for that action. The operation of the oxide of lead in its saline combinations as a mordant is studied; and the influence which the particular kind of paper used has on the result, is also examined, and various practical rules are deduced from these experiments. The author describes a method of precipitating on glass a coating possessing photographic properties, and thereby of accomplishing a new and curious extension of the art of photography. He observes, that this method of coating glass with films of precipitated argentine, or other compounds, affords the only effectual means of studying their habitudes on exposure to light, and of estimating their degree of sensibility, and other particulars of their deportment under the influence of reagents. After stating the result of his trials with the iodide, chloride, and bromide of silver, he suggests that trials should be made with the fluoride, from which, if it be found to be decomposed by light, the corrosion of the glass, and consequently an etching, might possibly be obtained, by the liberation of fluorine.

As it is known that light reduces the salts of gold and of platinum, as well as those of silver, the author was induced to make many experiments on the chlorides of these metals, in reference to the objects of photography; the details of which experiments are given. A remarkable property of hydriodic salts, applied, under certain circumstances, to exalt the deoxidating action of light, and even to call into evidence that action, when it did not before exist, or else was masked, is then described.

4. The chemical analysis of the solar spectrum forms the subject of the next section of his paper. It has long been known that rays of different colours and refrangibilities exert very different degrees of energy in effecting chemical changes; and that those occupying the violet end of the spectrum possess the greatest deoxidating powers. But the author finds that these chemical energies are distributed throughout the whole of the spectrum; that they are not a mere function of the refrangibility, but stand in relation to physical qualities of another kind, both of the ray and of the analysing medium; and that this relation is by no means the same as the one which determines the absorptive action of the medium on the colorific rays. His experiments also show that there is a third set of relations concerned in this action, and most materially influencing both the amount and the character of the chemical action on each point of the spectrum; namely, those depending on the physical qualities of the substance on which the rays are received, and whose changes indicate and measure their action.

The author endeavoured to detect the existence of inactive spaces in the chemical spectrum, analogous to the dark lines in the luminous one; but without any marked success. The attempt, however, revealed several curious facts. The maximum of action on the most ordinary description of photographic paper, namely, that prepared with common salt, was found to be, not beyond the violet, but about the confines of the blue and green, near the situation of the ray F in Fraunhofer's scale: and the visible termination of the violet rays nearly bisected the photographic image impressed on the paper: in the visible violet rays there occurred a sort of minimum of action, about one-third of the distance from Fraunhofer's ray H, towards G: the whole of the red, up to about Fraunhofer's line C appears to be inactive; and lastly, the orange-red rays communicate to the paper a brick-red tint passing into green and dark blue. Hence are deduced, first, the absolute necessity of perfect achromaticity in the object-glass of a photographic camera; and secondly, the possibility of the future production of naturally coloured photographs.

5. The extension of the *visible* prismatic spectrum beyond the space ordinarily assigned to it, is stated as one of the results of these researches; the author having discovered that beyond the extreme violet rays there exist luminous rays affecting the eyes with a sensation, not of violet, or of any other of the recognised prismatic hues, but of a colour which may be called *lavender-grey*, and exerting a powerful deoxidating action.

6. Chemical properties of the red end of the spectrum. The rays occupying this part of the spectrum were found to exert an action of an opposite nature to that of the blue, violet, and lavender rays. When the red rays act on prepared paper in conjunction with the diffused light of the sky, the discolorating influence of the latter is suspended, and the paper remains white; but if the paper has been already discoloured by ordinary light, the red rays change its actual colour to a bright red.

7. The combined action of rays of different degrees of refrangibi-

lity is next investigated; and the author inquires more particularly into the effects of the combined action of a red ray with any other single ray in the spectrum; whether any, and what differences exist between the joint, and the successive action of rays of any two different and definite refrangibilities; and whether this action be capable, or not, of producing effects, which neither of them, acting alone, would be competent to produce. The result was that, although the previous action of the less refrangible rays does not appear to modify the subsequent effects produced by the more refrangible; yet the converse of this proposition does not obtain, and the simultaneous action of both produces photographic effects very different from those which either of them, acting separately, are capable of producing.

8. In the next section, the chemical action of the solar spectrum is traced much beyond the extreme red rays, and the red rays themselves are shown to exercise, under certain circumstances, a blackening or deoxidating power.

9. The author then enters into a speculation suggested by some indications which seem to have been afforded of an absorptive action in the sun's atmosphere; of a difference in the chemical agencies of those rays which issue from the central parts of his disc, and those which, emanating from its borders, have undergone the absorptive action of a much greater depth of his atmosphere; and consequently of the existence of an absorptive solar atmosphere extending beyond the luminous one.

10. An account is next given of the effect of the spectrum on certain vegetable colours, as determined by a series of experiments, which the author has commenced, but in which the unfavourable state of the weather has, as yet, prevented him from making much progress.

11. The whitening power of the several rays of the spectrum under the influence of hydriodic salts, on paper variously prepared and previously darkened by the action of solar light. The singular property belonging to the hydriodate of potash of rendering darkened photographic paper susceptible of being whitened by further exposure to light is here analysed, and shown to afford a series of new relations among the different parts of the spectrum, with respect to their chemical actions.

12. The Analysis of the Chemical Rays of the Spectrum by absorbent media, which forms the subject of the next section, opens a singularly wide field of inquiry; and the author describes a variety of remarkable phenomena which have presented themselves in the course of his experiments on this subject. They prove that the photographic properties of coloured media do not conform to their colorific character; the laws of their absorptive action as exerted on the chemical, being different and independent of those on the luminous rays: instances are given of the absence of any darkening effect in green and other rays of the more refrangible kind, which yet produce considerable illumination on the paper that receives them.

13. The exalting and depressing power exercised by certain media, under peculiar circumstances of solar light, on the intensity of its che-

mical action. This branch of the inquiry was suggested by the fact, noticed by the author in his former communication, that the darkening power of the solar rays was considerably increased by the interposition of a plate of glass in close contact with the photographic paper. The influence of various other media, superposed on prepared paper, was ascertained by experiment, and the results are recorded in a tabular form.

14. The paper concludes with the description of an *Actinograph*, or self-registering Photometer for meteorological purposes: its objects being to obtain a permanent and self-comparable register and measure, first, of the momentary amount of general illumination in the visible hemisphere, which constitutes day-light; and secondly, of the intensity, duration, and interruption of actual sunshine, or, when the sun is not visible, of that point in the clouded sky behind which the sun is situated.

In a postscript, dated March 3rd, 1840, the author states that he has discovered a process by which the calorific rays in the solar spectrum are made to affect a surface properly prepared for that purpose, so as to form what may be called a *thermograph* of the spectrum; in which the intensity of the thermic ray of any given refrangibility is indicated by the degree of whiteness produced on a black ground, by the action of the ray at the points where it is received at that surface, the most remarkable result of which is the insulation of *heat-spots* or thermic images of the sun quite apart from the great body of the thermic spectrum. Thus the whole extent over which prismatic dispersion scatters the sun's rays, including the calorific effect of the least, and the chemical agency of the most refrangible, is considerably more than twice as great as the Newtonian coloured spectrum.

In a second note, communicated March 12, 1840, the author describes his process for rendering visible the thermic spectrum, which consists in smoking one side of very thin white paper till it is completely blackened, exposing the white surface to the spectrum and washing it over with alcohol. The thermic rays, by drying the points on which they impinge more rapidly than the rest of the surface, trace out their extent and the law of their distribution by a whiteness so induced on the general blackness which the whole surface acquires by the absorption of the liquid into the pores of the paper. He also explains a method by which the impression thus made, and which is only transient, can be rendered permanent.

This method of observation is then applied to the further examination of various points connected with the distribution of the thermic rays, the transescence of particular media, the polarization of radiant heat (which is easily rendered sensible by this method), &c. The reality of more or less insulated spots of heat distributed at very nearly equal intervals along the axis of the spectrum (and of which the origin is *probably* to be sought in the flint glass prism used—but *possibly* in atmospheric absorption) is established. Of these spots, two of an oval form, are situated, the one nearly at, and the other some distance beyond the extreme red end

of the spectrum, and are less distinctly insulated; two, perfectly round and well-insulated, at greater distances in the same direction; and one, very feeble and less satisfactorily made out, at no less a distance beyond the extreme red than 422 parts of a scale in which the whole extent of the Newtonian coloured spectrum occupies 539.

March 19.—“Researches in Electricity, Seventeenth Series: on the source of power in the Voltaic Pile.” By Michael Faraday, Esq., D.C.L., F.R.S., &c.

In this series, the author continues his experimental investigation of the origin of electric force in the voltaic pile. Having found abundant reason, in the experiments already described, to believe that the electricity of the pile has its origin in the chemical force of the acting bodies, he proceeds to examine how the circumstances which can affect the affinity of substances for each other, influence their power of producing electric currents. First, with relation to *heat*:—circuits were made of a single metal and a single fluid, and these were examined with a view to ascertain whether, by applying heat at one of the junctions, only thermo-currents can be produced. Some peculiar effects of heat are noticed and explained; and several very necessary precautions in conducting these experiments are pointed out; and it is found, when these are guarded against, that heat has a decided and distinct effect over the chemical affinities of the parts of a circuit subjected to its power, and a corresponding influence on the electric current produced. This proceeds to such an extent, that, in some cases, either of two metals can be made positive or negative with respect to the other in the same fluid, solely by virtue of this power of heat.

The effect of *dilution* is then examined. For this purpose, only one metal and one fluid are used in a circuit; but the fluid is rendered more dilute at one point of contact than at the other. First, it was ascertained that such dilution produces little or no effect with metals which are not acted on by the electrolyte employed; and the precautions requisite as to other points are then stated. But when these are observed, still dilution is found to have a most powerful influence on the results; and, as the author believes, solely on account of its influence on the active chemical affinity. Thus copper in dilute nitric acid is positive with respect to copper in strong nitric acid; and the same is the case with lead, silver, and other metals. It is not that the piece in the weakest acid is always positive with respect to that in the stronger acid; for, in the first place, some very curious cases are given, in which a piece of metal in acid of a certain strength is *positive* with respect to a piece of the same metal in acid, either stronger or weaker; and, in the next place, other cases are stated in which the piece in the medium acid is *negative* with respect to the other piece in either stronger or weaker acid. The effect of dilution in nitric acid is such, that when certain different metals are compared together, one can, at pleasure be made positive or negative with respect to the other; thus, of the five metals, silver, copper, iron, lead, and tin, any one of them can be made either positive or negative with respect to any other; with

the sole exception of silver positive with respect to copper. The inconsistency of these results with any theory of contact electromotive force is then strongly insisted on by the author.

The next division of the paper treats of the order of the metallic elements of voltaic circuits when different electrolytes are used. It is usual to say, that metals are positive or negative with respect to each other in a certain order; but Davy, and afterwards De la Rive, showed that, in certain cases, this order must be inverted. The author, by using ten metals and seven different exciting electrolytic solutions, shows that in no two solutions is the order the same; but that changes of the most extreme kind occur in exact conformity with the changes in chemical action, which the use of the different solutions occasions.

The next division of the paper considers the very numerous cases in which voltaic circuits, often such as are able to effect decomposition, are produced without any metallic contact, and by virtue of chemical action alone; contrasting them with the numerous cases given in the previous series, where contact *without* chemical action, whether it be the contact of metal with metal, or with chemically inactive electrolytes, can produce no voltaic current.

There then follows a consideration of the sufficiency of chemical action to account for all the phenomena of the pile. It is shown that chemical action does actually evolve electricity; that according as chemical action diminishes or ceases, so the electrical current diminishes or ceases also; that where the chemical action changes from side to side, the direction of the current likewise changes with it; that where no chemical action occurs, no current is produced, but that a current will occur the moment chemical action commences; and that when the chemical action which has, or could have produced a current is, as it were, reversed or undone, the current is reversed or undone likewise; that is, it occurs in the opposite direction, in exact correspondence with the direction taken by the transferred anions and cathions. The accordance of the chemical theory of excitation with these phenomena, is considered by the author as of the strictest kind.

The phenomena of thermo-electricity are considered by some philosophers as affording proofs of the efficacy of mere metallic contact in exciting an electric current. The author proceeds, therefore, to examine these phenomena in relation to such an action, and arrives at the conclusion, that they in fact disprove the existence of such a power. In thermo-electricity the metals have an order which is so different from that belonging to them in any electrolyte, that it appears impossible to consider their succession, in any case, as due to any mutual effect of the metals on each other common to both modes of excitation. Thus, in the thermo-circuit, the electric current is, at the hot place, from silver to antimony, and from bismuth to silver; but in a voltaic series, including dilute sulphuric or nitric acids, or strong nitric acid, or solution of potash, the electric current is from silver to both antimony and bismuth; whilst if the yellow sulphuret of potash be used, it is from both antimony and

bismuth to silver; or if the hydro-sulphuret of potash be used, it is from bismuth to silver, and from silver to antimony; and, finally, if strong muriatic acid be used, it is precisely the reverse, or from antimony to silver, and from silver to bismuth. The inconsistency of these results with the contact theory is then insisted on and further developed.

The last section of this series is on the improbability of there existing any such force as the assumed contact force. The author contends that it is against all natural analogy and probability that two particles which, being placed in contact, have by their mutual action acquired opposite electrical states, should be able to discharge these states one to the other, and yet remain in the state they were in at the first, i. e., entirely unchanged in every point by what has previously taken place; or, that the force which has enabled two particles by their mutual action to attain a certain state, should not be sufficient to make them keep that state. To admit such effects would be, he thinks, to deny that action and reaction are equal. The contact theory, according to him, assumes that a force which is able to overcome powerful resistance, both chemical and mechanical, can arise out of nothing. That without any change in the acting matter, or the consumption of any other force, an electric current can be produced which shall go on for ever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped in its own course;—this, the author thinks, would be a creation of power, such as there is no example of in nature; and, as there is no difficulty in converting electrical into mechanical force through the agency of magnetism, would, *if true*, supply us at once with a perpetual motion. Such a conclusion he considers as a strong and sufficient proof that the theory of contact is founded in error.

FRIDAY-EVENING MEETINGS AT THE ROYAL INSTITUTION.

January 24, 1840.—Mr. Faraday on voltaic precipitations.

January 31.—Dr. Grant on the structure and growth of corals.

February 7.—Mr. Faraday on a particular relation (Dove's) of condensable gases and steam.

February 14.—Mr. Catlin's account of his residence and adventures among the native tribes of North America, with notices of their social condition, customs, mysteries, mode of warfare, tortures, &c.

February 21.—Mr. Nasmyth on the origin of alphabetic characters, and on the pneumatic mirror.

February 28.—Mr. Brayley on the application of science to the choice of building stones, with reference to the selection of stone for the New Houses of Parliament.

March 6.—The Rev. Mr. Hincks on the monstrosities of plants.

March 13.—Mr. Grove on voltaic reaction, or the phænomena usually called polarization.

Mr. Grove detailed the first experiments of Volta, Erman, Ritter, and Davy, the more recent ones of De la Rive, the explanation of these by Becquerel, and the confirmation of this latter philosopher's

opinion by the experiments of Dr. Schœnbein, Mr. Matteucci, and Mr. Grove himself; all which, as well as the experiments of Mr. Grove on the inactivity of amalgamated zinc, which he proved to be due to the same order of causes, have been already given in full in various numbers of the *Philosophical Magazine*. All the effects which have generally been included under the term polarization were proved by Mr. Grove to be traceable to one principle, viz. the electrolytic transfer of elements having for each other a chemical affinity, and the reaction caused by this affinity when the decomposing and transferring power, i. e. the initial voltaic current, is arrested. What we are most anxious to call the attention of our readers to, are the astonishing effects exhibited by Mr. Grove at the conclusion of his lecture. Two batteries, little differing in construction from that described by him in the *Lond. and Edinb. Phil. Mag.*, were charged some time previously to the lecture, and up to the period of its conclusion remained in perfect inactivity until the circuit was completed. One of these was arranged as a series of five plates, and contained altogether about four square feet of platina foil; with this the mixed gases were liberated from water at the surprising rate of one hundred and ten cubic inches per minute. A sheet of platinum, one inch wide by twelve long, was heated in the open air through its whole extent, and the usual class of effects produced in corresponding proportion. With the other arrangement, consisting of fifty plates of two inches by four, arranged in single series, a voluminous flame of one inch and a quarter long was exhibited by charcoal points, which showed beautifully the magnetic properties of the voltaic arc, as Dr. Faraday held a piece of iron near it, being attracted and repelled by different portions of the iron: bars of different metals were instantly run into globules and dissipated in oxide. It should be borne in mind that all these effects were produced by a battery which did not cover a space of sixteen inches square, and was only four inches high, and which had been charged for some hours.

Mr. Grove adverted to the letter of Prof. Jacobi to Dr. Faraday published in *Lond. and Edinb. Phil. Mag.*, vol. xv. p. 161, and stated that Mr. Pattison, who navigated the Neva with Prof. Jacobi in October last, had observed that the batteries employed were on Mr. Grove's construction, which the Professor without hesitation admitted.

March 20.—Mr. Schomburgk on the aborigines of Guiana.

March 27.—Dr. Gregory on the statistics of disease and mortality in London.

LVI. Intelligence and Miscellaneous Articles.

THEORY OF SUBSTITUTIONS. POND GAS.

M. PERSOZ sent a letter to the Academy of Sciences, relating to the conversion of acetic acid into pond gas and to the theory of substitutions. The author appears to have written, lest some expressions made use of by M. Dumas, in the sitting of the 13th of January, should create a belief that he had entertained the same views as M. Dumas in arriving at the discovery of the production of pond

gas from the acetates. He endeavours to show that if they have both arrived at the same conclusion, the means by which they have done so are peculiar to each and have nothing in common. On this subject M. Persoz recites several passages from a memoir which he sent in January 1838, under the title of *On the necessity of distinguishing in chemical actions the phenomenon of displacement from those of alteration*; this memoir is still unpublished, and M. Dumas has declared that he was unacquainted with it; it contains the details of experiments, from which M. Persoz states it clearly results—

1st. That acetone contains two volumes of oxide of carbon.

2nd. That protocarburetted hydrogen gas is derived from acetone, and not from acetic acid, and consequently that the protocarburetted hydrogen which arises from the decomposition of the acetates, cannot be as M. Dumas supposes the immediate product of the decomposition of acetic acid by hydrate of potash; but, on the contrary, a consecutive secondary product resulting—

a. From the action which heat exerts on acetic acid, whether in a free state or combined with bases.

b. From the action which water exerts on acetone, one of the immediate products of the action of heat on acetic acid (MM. Liebig and Pelouze).

3rd. That this pond gas is formed by the disappearance of two volumes of oxide of carbon, and the assimilation of two volumes of hydrogen, derived from 1 eq. *decomposed water*.

From the preceding statements, M. Persoz adds, it is evident that he has not adopted the views of M. Dumas in explaining the formation of pond gas; and that the fact of its formation, so far from offering proof in favour of the theory of substitutions, justifies the statement which he has made respecting it, in the following passage of his "*Introduction to the Study of Molecular Chemistry*," p. 853.

"When M. Dumas maintains that chlorine is isomorphous with hydrogen, he lays it down as a principle, that bodies may be totally changed in their elementary condition, without varying in their molecular composition; we think this theory ought to be rejected as being contrary to experience: it is dangerous in its application, for to a certain extent it dispenses with the consideration, of the action which the first products, formed during a reaction, exert upon those which have not yet been altered."

In applying then the theory of substitutions to the formation of pond gas, obtained by the decomposition of acetic acid by means of an alkali, M. Dumas has not excluded the action which heat exerts upon acetic acid, and has thus neglected the compounds which result from it. He has, moreover, considered the formation of pond gas as the product of a simple action, whereas it is really the result of a complex one. Lastly, M. Dumas has completely neglected the action which water may exert, which in the formation of pond gas, acts according to M. Persoz a most important part.

M. Dumas has lately advanced, as a new argument in favour of the theory of substitutions, the fact of the identity which he has

stated to exist between the final product of the action of chlorine on protocarburetted hydrogen, and the final product of the action of chlorine on chloroform: a final product which is represented by $C^2 Cl^8$, and obtained in the first case by the loss of eight volumes of hydrogen, replaced by eight volumes of chlorine, and in the second case by the loss of two volumes of hydrogen, replaced by Cl^2 .

It appears to M. Persoz that in this case M. Dumas has confounded a phænomenon of alteration with a phænomenon of displacement. Will it be said, that because on burning four volumes of protocarburetted hydrogen with an excess of oxygen, there are obtained four volumes of carbonic acid containing four volumes of oxygen equivalent to eight volumes of hydrogen, there occurs in this fact an additional proof in favour of the theory of substitutions, and that eight volumes of hydrogen being taken from $C^2 H^8$ they ought to be replaced by four volumes of oxygen? Will carbonic acid be ever confounded with protocarburetted hydrogen in the same *chemical* type? certainly not; for all chemists agree, and M. Dumas especially, in admitting that in such a combustion the quantity of oxygen fixed with the carbon depends on the number of the atoms of the latter body; so that, in an organic compound, two atoms of carbon being combined with eight to twenty or any number of atoms of hydrogen, this compound being decomposed by excess of oxygen, there will be only four volumes of oxygen combined with the carbon. Will not M. Dumas admit, that in destroying, as he has done, with excess of chlorine, *chloroform* and *protocarburetted hydrogen*, compounds which both contain two atoms of carbon, he could in fact obtain only chloride of carbon, corresponding to carbonic acid, that is to say $2 C Cl^4 = C^2 Cl^8$, in the same way as by destroying protocarburetted hydrogen by excess of oxygen, there are obtained $2 C O^2 = C^2 O^4$, without resorting to his theory of substitutions?

M. Persoz adds, that it appears to him important to refer to the fact that the formation of pond gas served him as a means of discovering the mysterious agency of water in the reactions of organic bodies. By this decomposition of water, he explains the conversion of starch into sugar, that of sugar into alcohol, and that of certain immediate principles into essential oils; and he also conceives that he can reduce to the same order of phænomena (that of oxidization) the action of nitric acid and hydrate of potash on sugar, which, as it is well known, is converted by both of these agents into oxalic acid.—*L'Institut*, No. 323.

ON ARSENIC CONTAINED NATURALLY IN THE HUMAN BODY.

M. Orfila has read a memoir on the above subject before the Royal Academy of Medicine; the experiments detailed were made with M. Couerbe, and their object was to solve the following questions:

1st, Does arsenic exist originally in the human body? 2ndly, Do the viscera contain any? 3rdly, Can its existence in the muscles be proved? 4thly, Is it possible to determine that the arsenic obtained from a corpse is not that which originally existed among the elements composing the tissues, but was introduced into the digestive organs, applied to the exterior, &c.?

I. Arsenic exists in human bones; if the bones of an adult be calcined, taking care not to raise the temperature too high, and to avoid contact with the fuel, these bones, when reduced to powder and treated with purified sulphuric acid, and then tried in Marsh's apparatus, will yield brown, brilliant and thick arsenical spots. This result was obtained both from the bones of corpses of adults who had been dead some days, or buried for some months.

When the calcination is effected at a white heat, no arsenic is obtained, nor is any procured, from the bones of commerce reduced to a soft paste; but if they be subjected to heat and the processes indicated (nitric acid, potash and sulphuric acid), a certain quantity of arsenic is obtained.

From this first series of experiments, which amount to fourteen, I conclude, says M. Orfila, 1st, That the bones of the human adult, of the horse, ox, and sheep contain minute portions of arsenic, which it is possible to discover by treating the bones with potash purified by alcohol and pure sulphuric acid.

2ndly, This quantity of arsenic is not increased by long burial.

3rdly, Vittrification removes a portion of it, which is undoubtedly occasioned by the volatilization which it occasions.

4thly, Among the conditions favourable to the discovery of arsenic, must be especially reckoned that of not calcining the bones too strongly, and secondly to avoid carefully the contact of fuel.

5thly, When bones are treated with pure water and ebullition, no arsenic is discoverable.

6thly, If in operating in this mode, any arsenic be detected, it has certainly been in some mode introduced into the œconomy.

II. No arsenic is found in the viscera unless it has been absorbed. The organs of a dog which was hung, treated by the usual processes, did not yield any. The blood, brain, the liver, spleen, kidneys, intestines, stomach, &c. gave no traces of it. Carbonized with nitric acid, and afterwards tried in Marsh's apparatus, white opaque spots only were obtained, and these were also produced without the presence of these organic matters.

The liver of an adult gave none; nor did the decoctions made with various organs yield any.

From these facts we may conclude, observes M. Orfila, but not positively, that the viscera do not originally contain arsenic; or to state the fact more accurately and not to prejudge the case, it may be asserted, that they do not yield any when treated with boiling water, sulphuretted hydrogen, or when carbonized by concentrated nitric acid, &c. It may so happen that the quantity is too small to be detected by sulphuretted hydrogen, or that it is lost by carbonization; but by acting on a large quantity of brain or other organs, it may be detected. At any rate, it is sufficient at present to have ascertained, that the viscera yield no arsenic by the reactions described, unless it has been introduced by poisoning.

III. It is not proved that muscular flesh contains arsenic: twelve pounds of it taken from the corpse of an adult, carbonized by nitric acid and tested by Marsh's apparatus, gave white opaque spots; some were brilliant, with a bluish tint; others were yellow, and had an

arsenical appearance ; dissolved in boiling nitric acid, they gave no alliaceous smell when put on red-hot charcoal ; in fact, they possessed none of the characteristics of arsenic. These spots were, however, very numerous ; submitted for nearly twenty days to a current of sulphuretted hydrogen gas, they gave no indication of arsenic. It is possible that they were a mixture of arsenic and animal matter, and that the muscular flesh of two or three bodies might yield some by analysis ; lastly, other processes may discover it in the same quantities as those employed, by occasioning less loss ; therefore, adds M. Orfila, I will not conclude, positively, that arsenic does not exist in muscular flesh.

IV. It is possible to ascertain that the arsenic which may be discovered does not come from the organic substance itself, but that it has been combined with it by absorption. For if it be found in the bones, it will not be removed by long boiling in water, unless it had been introduced ; and the same holds good with respect to the blood and the organs which have been examined.

Lastly, if the muscles yield spots, some of which resemble arsenic at first sight, the distinctive characters which have been stated must be remembered ; and if the subject had taken arsenical remedies, this circumstance ought to be particularly attended to.—*Journal de Chim. Med.*, Dec. 1839.

METEOROLOGICAL OBSERVATIONS FOR FEB., 1840.

Chiswick.—Feb. 1. Cloudy : rain. 2. Very fine. 3. Rain. 4. Boisterous with rain. 5. Rain : clear. 6. Rain : cloudy. 7. Rain. 8. Heavy showers. 9. Fine. 10. Heavy showers : clear and very fine at night. 11. Fine. 12. Rain. 13. Very fine. 14. Foggy. 15. Frosty : rain. 16. Hazy and mild. 17. Dense fog. 18. Dry cold haze. 19. Clear and cold. 20. Cloudy, with some snow-flakes falling. 21. Bleak and cold. 22. Overcast. 23. Cold and dry. 24. Fine but cold. 25. Frosty haze. 26. Cold haze. 27. Cloudy, cold and dry. 28, 29. Fine but cold.

Boston.—Feb. 1. Cloudy : rain P.M. 2. Fine : rain early A.M. 3. Stormy : rain early A.M. 4. Stormy : rain early A.M. : rain P.M. 5. Cloudy : rain P.M. 6. Cloudy. 7. Cloudy : rain early A.M. : rain P.M. 8. Fine : rain and snow P.M. 9. Fine. 10. Rain. 11. Fine. 12—15. Fine : rain P.M. 16, 17. Cloudy. 18. Fine. 19. Cloudy : snow A.M. and P.M. 20. Cloudy : snow A.M. 21. Cloudy : snow melted. 22. Cloudy. 23—27. Fine. 28. Cloudy. 29. Fine.

Applegarth Manse, Dumfries-shire.—Feb. 1. Frequent showers. 2. Frequent showers : snow gone. 3. Frequent showers. 4, 5. Shower A.M. : fair rest of the day. 6. Rain very early : fine day. 7. Heavy rain A.M. : stormy P.M. 8. Occasional showers of rain and hail. 9, 10. Occasional showers of rain and hail with high wind P.M. 11. Fine day : a few drops of rain. 12. Storm of wind and rain P.M. 13. Fine day : no rain. 14. Fine day, but cloudy. 15. Wet morning : cleared up P.M. 16. Calm, cloudy, and mild. 17, 18. Fine A.M. : grew cloudy and sharp. 19. Cold easterly wind, but fair. 20. Cold easterly wind with slight frost and snow showers. 21. Cold easterly wind : frost : threatening snow. 22—24. Cold easterly wind : still frosty : sprinkling snow. 25. Cold easterly wind. 26. Beautiful sunny day, but still frosty. 27. Beautiful sunny day : frost very keen. 28. Cloudy all day : but still freezing. 29. Fine frosty day.

Sun shone out 25 days. Rain fell 13 days. Snow 2 days. Frost 10 days.

Wind north-easterly 6 days. Easterly $3\frac{1}{2}$ days. South-easterly 7 days. Southerly $3\frac{1}{2}$ days. South-westerly $8\frac{1}{2}$ days. West 1 day.

Calm $14\frac{1}{2}$ days. Moderate 8 days. Brisk $1\frac{1}{2}$ day. Strong breeze 2 days. Boisterous 3 days.

Days of Month. 1840.	Barometer.				Thermometer.				Wind.				Rain.			Dew point. Land: Roy. Soc. 9 a.m.				
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	Fah. 9 a.m.	London: Roy. Soc. Self-register.		Chiswick. Max.	Chiswick. Min.	Dumfries-shire. Max. Min.	London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.
		Max.	Min.				Max.	Min.												
Feb.																				
1.	29.338	29.363	29.270	29.07	29.29	29.14	43.7	44.6	40.4	47	48	41	38½	32	SE.	S.	SE.	NE.	41	41
2.	29.354	29.416	29.349	28.86	29.04	28.95	41.2	41.8	39.9	50	40	41	41	35	W.	SW.	W.	S.	40	40
3.	29.174	29.191	28.988	28.80	28.89	28.86	43.8	44.4	41.2	49	44	42½	40	39	S.	SW.	S.	ESSW.	41	41
4.	28.648	28.802	28.597	28.40	28.73	28.79	44.8	45.6	40.2	47	40	41	40	33	S. var.	S.	SE.	NE.	40	40
5.	29.104	29.656	29.116	28.60	29.10	29.44	42.3	45.7	41.5	46	35	42	40½	38	W.	NW.	calm	NNE.	40	40
6.	29.754	29.857	29.759	29.25	29.40	29.52	39.8	45.7	38.4	50	34	39	40	33½	SW.	W.	calm	W.	40	40
7.	29.596	29.616	29.449	29.18	29.07	28.74	46.9	48.0	39.0	53	40	43	44½	37	S.	SW.	SW.	SW.	37	37
8.	29.532	29.682	29.559	29.05	29.06	29.25	43.8	51.0	41.0	47	32	41	38	35	W.	SW.	W.	WSW.	41	41
9.	29.724	29.759	29.722	29.27	29.32	29.10	38.0	38.7	35.5	47	43	34	45	32	S.	NW.	calm	S.	40	40
10.	29.580	29.796	29.588	29.10	29.05	29.34	47.4	48.4	37.2	51	37	38	46	40	S. var.	SW.	SW.	SW.	37	37
11.	29.950	29.975	29.945	29.50	29.57	29.52	42.2	42.8	40.5	52	45	38	47	37	S.	SW.	W.	SW.	42	42
12.	29.724	29.781	29.715	29.26	29.29	29.18	47.4	48.3	41.9	50	35	46	47	42½	S.	SW.	SW.	S.	40	40
13.	29.838	29.927	29.826	29.32	29.49	29.69	43.0	43.5	41.6	53	28	41	44	37½	S.	SW.	SW.	SW.	42	42
14.	29.956	29.966	29.957	29.55	29.78	29.72	35.2	50.2	35.3	47	26	35½	44	32	S.	SW.	calm	SW.	41	41
15.	29.848	29.880	29.733	29.43	29.45	29.41	37.7	39.5	33.8	46	40	39	46	35	S.	SW.	calm	SW.	38	38
16.	29.772	29.954	29.796	29.30	29.55	29.80	46.7	47.3	37.0	51	44	47	47½	44	SW.	SW.	calm	SW.	35	35
17.	30.004	30.162	30.008	29.48	29.99	30.10	48.2	50.8	46.0	48	32	45	46	40	SW.	SE.	E.	E by S.	40	40
18.	30.244	30.306	30.240	29.90	30.15	30.22	37.7	49.0	36.0	42	32	38	40½	32½	NE.	E.	E.	E by S.	41	41
19.	30.324	30.437	30.335	30.11	30.45	30.38	35.2	41.2	35.2	36	30	36	36½	33	N.	E.	calm	E by S.	35	35
20.	30.450	30.515	30.475	30.16	30.44	30.45	33.5	36.6	32.8	33	30	35	36	30	N.	NE.	calm	E by S.	29	29
21.	30.452	30.480	30.417	30.10	30.30	30.38	32.3	33.4	32.0	34	28	32½	34½	30½	NE.	E.	calm	SE.	28	28
22.	30.324	30.362	30.286	30.10	30.30	30.28	32.7	33.8	31.0	34	24	33	36½	29	NE.	NE.	E.	E.	26	26
23.	30.224	30.287	30.265	30.10	30.28	30.28	29.5	34.6	28.0	36	24	35	39	29	NE.	NE.	E.	E.	24	24
24.	30.376	30.547	30.407	30.15	30.35	30.46	30.7	35.0	27.8	41	26	33½	38½	30½	NE.	E.	E.	E.	25	25
25.	30.596	30.661	30.609	30.27	30.50	30.50	35.3	37.4	30.7	44	25	35	34½	29	NE.	E.	E.	E by S.	23	23
26.	30.596	30.637	30.563	30.33	30.53	30.48	35.2	35.6	32.8	40	32	35	34	27½	NE.	E.	E.	E by S.	30	30
27.	30.468	30.549	30.438	30.15	30.44	30.36	34.7	35.3	33.6	40	32	33	30	22	ENE.	NE.	E.	E by S.	27	27
28.	30.316	30.359	30.271	30.10	30.30	30.31	34.6	35.2	33.6	45	32	34	32½	25	N.	NE.	N.	ENE.	2	2
29.	30.356	30.414	30.369	30.05	30.35	30.40	36.0	36.7	33.6	45	28	38	34	26	NE.	E.	E.	ENE.	31	31
Mean.	29.918	30.011	29.898	29.53	29.740	29.795	39.3	42.4	36.5	44.96	34.00	38.2	40.3	33.3		Sum. 1.224	1.25	1.28	2.62	Mean. 34.9

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

M A Y 1840.

LVII. *On the Boulder Formation, or drift and associated Freshwater Deposits composing the Mud-cliffs of Eastern Norfolk.* By CHARLES LYELL, Esq., V.P.G.S., F.R.S., &c.*

THE cliffs extending from Happisburgh or Hasborough light-house to near Weybourne, north-west of Cromer, in Norfolk, comprising a distance of about 20 miles, are designated in some maps as "the mud-cliffs." They are for the most part composed of deposits of two kinds, first, stratified and unstratified drift, called by some "diluvium;" secondly, freshwater strata. Both of these rest on chalk, which is usually concealed below the level of the sea. Occasionally between the chalk and drift or the chalk and freshwater beds, a thin layer is found of marine crag, agreeing in its fossils with that of Norwich, but occurring only in patches of small extent, except near Weybourne, where it is more continuous.

The drift, which sometimes attains a thickness of more than 300 feet, consists principally of clay, loam, and sand, in some places stratified, in others wholly devoid of stratification. Pebbles, and in some places large boulders of granite, porphyry, greenstone, lias, chalk, and other transported rocks are interspersed, especially in the unstratified portion. Pure and unmixed white chalk rubble, and even huge fragments of solid chalk, are also associated in some localities. No fossils have been detected in this drift which can positively be referred to the æra of its accumulation; but besides the organic remains derived from secondary strata, it contains almost everywhere broken fragments of shells which agree in species with those of the Norwich crag, from which there is good reason to believe them to have been washed out.

* Communicated by the Author.

The freshwater strata associated with the boulder formation above mentioned occur for the most part in patches at the bottom of the drift, and immediately above the subjacent chalk and crag, where the latter is present, as may be seen at a variety of places between Happisburgh and Runton near Cromer. The two spots where it is most largely developed are at Mundesley and Runton. At the latter place it underlies the drift and rests immediately on chalk, with the occasional intervention of marine crag; while at Mundesley it occupies for a certain space the whole cliff, taking the place as it were of the drift, and appearing in part to be of contemporaneous origin, and in part of subsequent date and superimposed. Everywhere it contains the same species of shells, and as these are almost without exception identical with well-known British species, it is evident that the entire formation of the mud-cliffs, whether freshwater or drift, belongs to the latest part of the tertiary period, the only doubt being whether it should not rather be considered as post-tertiary or referable to a class of deposits which contain exclusively shells of recent species.

I mention at once this conclusion, because the recent origin of the drift adds a peculiar interest to the great derangement and change of position which it has undergone since its deposition. In no other parts of our island, or perhaps of Europe, are there evidences of local disturbance on so great a scale and of an equally modern date, for there are proofs of the movement both downward and upward of strata several hundred feet thick for an extent of many miles; together with most complicated bendings and foldings of the beds and the intercalation of huge masses of chalk, and what is no less perplexing and difficult of explanation, the superposition of contorted upon horizontal and undisturbed strata.

The line of coast in which the formations above alluded to are displayed was well described in Mr. R. C. Taylor's *Geology of East Norfolk*, published in 1827, and afterwards in Mr. Woodward's *Outline of the Geology of Norfolk*, 1833. Both these papers are the result of a careful survey of the coast, and contain original observations of great merit. In both are given coloured sections of the cliffs, from which a good general idea of their structure and composition may be derived. A memoir was also read to the Geological Society in January 1837, by the Rev. W. B. Clarke, in which among other remarks he insists on the necessity of separating the diluvium of Norfolk from the crag*.

My own observations on the coast of East Norfolk were made first in the year 1829, and afterwards in 1839; and as

* Geological Transactions, 2nd series, vol. v. part ii. p. 363.

the sea has been continually encroaching on the cliffs, I found after an interval of ten years that a different section of the same beds was exhibited, and some difficulties cleared up which I had been unable to explain on my first visit. Nevertheless a high beach precluded me at both periods from obtaining a view of the lowest beds, which are sometimes exposed in winter at low water, and after storms. During my last visit in particular (1839) the prevalence of easterly gales prevented my seeing in some places no less than 12 feet in vertical height of the section which was visible in the summer of 1829.

The principal deposit which constitutes the mud cliffs of Eastern Norfolk is strictly analogous in character to that which has been called the "boulder formation" in Denmark and Sweden, and which, from the numerous erratics included in it, forms so remarkable a feature in the superficial geology of Scandinavia, and all the countries surrounding the Baltic, as well as northern Russia. It may be said to extend uninterruptedly from Sweden through the Danish islands, Holstein, and the countries of Hamburgh, Bremen, and Osnabruck, to the borders of Holland, and then to appear again with the same characters in Norfolk and Suffolk. Throughout this tract, however, the average number and dimensions of the included erratic blocks, especially those of granite, porphyry, gneiss, and other crystalline rocks, diminishes sensibly on proceeding from north to south.

As I am of opinion that the boulder formation in all these countries has been accumulated almost exclusively on ground permanently submerged beneath the waters, and that it does not consist of materials transported either by one or many transient rushes of water over land which had previously emerged, I shall dispense as far as possible with the term "diluvium," substituting that of "drift" for such portions of the deposit which cannot be proved to be fresh-water. Part of this drift consists of clay and loam wholly devoid of stratification, to which the name of "till" may be applied, a provincial term widely used in Scotland for similar masses of unstratified matter, which there also contain most commonly included boulders. The entire want of a stratified arrangement in the *till*, whether in Scandinavia, Scotland, or Norfolk, implies some peculiarity in its mode of origin; yet in all these countries some of the till has accumulated contemporaneously, and apparently in the same body of water, as much of the accompanying stratified gravel, sand, and clay. Moreover the stratified drifts are often identical in composition with the *till*, the distinction consisting merely in the mode of arrangement.

I have seen no kind of deposit now in progress precisely similar in character to the till, except one, namely, the terminal moraines of glaciers. These, as Charpentier has justly remarked, are entirely devoid of stratification, because the accumulation has taken place without the influence of any currents of water by which the materials would be sorted and arranged according to their relative weight and size. Year after year the ice, as it melts at the extremity of a glacier, adds fresh mud, together with fine and coarse sand, gravel, and huge blocks, to the moraine, all being carried to the same distance*, without the least reference to the volume or specific gravity of the component particles or masses.

There can be no doubt that similar accumulations must take place in those parts of every sea, where drift ice, into which mud, sand, and blocks have been frozen, melts in still water, and allows the denser matter to fall tranquilly to the bottom. The occasional intercalation of a layer of stratified matter in the till, or the superposition or juxtaposition of the same, may be explained by the existence or non-existence of currents, during the melting of the ice, whether successively in the same place or simultaneously in different places.

It is, I believe, a common error of those who are not unwilling to admit the agency of ice in reference to the larger fragments of transported rock, to forget that what carries heavier masses from place to place must unavoidably convey a much larger volume of lighter and finer materials.

Having offered these preliminary remarks, I shall proceed to describe in detail some of the appearances which present themselves to one who travels along the coast from Hasborough to Weybourne. The section of the mud cliffs begins at the more southern of the two lighthouses about a mile and a half south of Hasborough. The cliffs here, which are between sixteen and twenty feet in height, are composed generally of a mass of blue clay covered with yellow sand, the clay and sand being both stratified in some places with great regularity, but in others the clay or mud is quite unstratified. Included in this *till* I found pieces of unrounded white chalk, angular chalk flints, fragments of argillaceous limestone (lias?), blocks of dark greenstone, and other rocks. There are also interspersed pieces of shells, apparently belonging to *Cyprina*, *Cardium*, *Macra*, and *Tellina*, such as might have been derived from the denudation of the Norwich crag. At some points where stratified clay reposes on the till, the surface of the latter is very uneven, and was evidently so when the superior deposit was thrown down upon it. Examples of intercalation of

* *Ann. des Mines*, tom. viii.

till between laminated beds of clay and loam are not unfrequent. In 1839, I saw at a spot between the two lighthouses, till resting on stratified clay and covered by stratified gravel and white chalk rubble, which latter formed the top of the cliff.

Owing to the continual dilapidation of the cliffs the details of the sections seen by me in 1829 and 1839 were very different. During my last visit the beach at Hasborough was too high to allow me to see the fundamental bed of lignite which exists there, which in June 1829 was exposed at low water, the descending section being then as follows: 1st, sand and loam, 13 feet; 2ndly, unstratified mud or till varying from 8 to 16 feet; 3rdly, laminated sand and clay, one foot and a half, part of the clay being bituminous and inclosing compressed branches and leaves of trees. The clays, which were blackish, greenish or brown, contained occasional layers of small pebbles, rounded and angular, mostly of chalk flint. The entire height of the cliff was about 35 feet.

This locality has been mentioned by various authors as the principal site of the submarine forest of East Norfolk, which has been described as occurring about the level of low water; and Mr. R. C. Taylor observes of this deposit generally,—

“That it consists of forest peat, containing fir cones and fragments of bones; in others of woody clay; and elsewhere of large stools of trees standing thickly together, the stems appearing to have been broken off about 18 inches from their base. They are evidently rooted in the clay or sandy bed in which they originally grew, and their stems, branches, and leaves lie around them, flattened by the pressure of from 30 to 300 feet of diluvial deposits. It is not possible to say how far inland this subterranean forest extends; but that it is not a mere external belt is obvious from the constant exposure and removal of new portions, at the base of the cliffs*.”

A letter of the Rev. James Layton is thus cited by Mr. Fairholme:—

“One remarkable feature in this compact blue clay is a stratum of wood, exhibiting the appearance of a wood overthrown or crushed *in situ*. At Paling the stumps of trees seem now to be really standing, the roots are strong, spread abroad, and intermingling with each other; were a torrent to sweep away the mould from the surface of a thick wood, leaving the roots bare in the ground, the appearances would be exactly the same. This phenomenon occurs again at Hasborough: the line of crushed wood, leaves, grass, &c., frequently forming a bed of peat, extends just above low-water mark. About this stratum are found numerous remains of mammalia, the horns and bones of at least four kinds of deer, the ox, the horse, hippopotamus, rhinoceros, and elephant. These fossil remains are found at Hasborough and its neighbourhood on the denuded clay shore: at Mundesley they are found in the cliff. The great mine, however, is in the sea, some miles from land, where there is an oyster bed on a stratum of gravel about six fathoms deep. How far this bed of fossils extends, I cannot pretend to say, but in 1826 some fishermen while dredging for soles

* Geology of E. Norfolk, p. 21.

on 'the knowl,' a bank 20 miles off shore, brought up an entire tusk of an elephant nine feet six inches long. The elephants must have been abundant; I have at least 70 grinders, and the oyster dredgers reported that they had fished up immense quantities and thrown them into deep water, as they greatly obstructed their nets*."

Mr. Woodward had previously spoken of the same oyster bed, which was discovered off Hasborough in the year 1820, and says that during the first twelve months, many hundred specimens of the molar teeth of the elephant were dredged up by the fishermen, and that the remains of upwards of 500 animals must have been found there†.

I was not so fortunate either here or elsewhere on this coast as to see the stools of trees erect in this stratum, but so many independent eye-witnesses have lately described them to me with such minuteness as to leave in my mind no doubt of the fact. Besides the accounts of several fishermen, Mr. Simeon Simons of Cromer states, that at Cromer he saw ten or more trees in the space of half an acre exposed below the cliffs eastward of that town, the stumps being a few inches, or all less than a foot in vertical height, some of them no less than 9 or 10 feet in girth, the roots spreading from them on all sides throughout a space twenty feet in diameter. Many others were seen by him laid open on the beach opposite Sidestrand, about three miles further to the eastward, evidently belonging also to a submerged forest. All these roots were in a laminated blue clay, with associated blue sand, the whole, six or seven feet thick, resting on chalk. In one place a thin layer of Norwich crag intervenes between the chalk and the bed of blue clay with lignite. Shells had been found immediately below the roots, but I have been unable to obtain them.

I ascertained that at Woolcot Gap, between Hasborough and Bacton, the bed of lignite, containing the bones of elephants, pieces of wood, and the roots of trees *in situ*, had been exposed at the base of the cliff in the preceding winter of 1838-39. A mass of incumbent drift about 30 feet thick must have been removed by the waves and currents, in order to lay open this lignite on the spot alluded to, and the great extent of the submerged forest is proved not only by the numerous points between Paling and Runton, which are about 18 miles apart, reckoning by the sea-coast, and nearly as far in a direct line, but also by the proofs afforded of its extension inland in proportion as the overlying beds are swept away by denudation.

It follows then from the facts above stated that the chalk in this region had been overspread with layers of sand and

* Rev. James Layton, cited in Fairholme's *Geology*, p. 281.

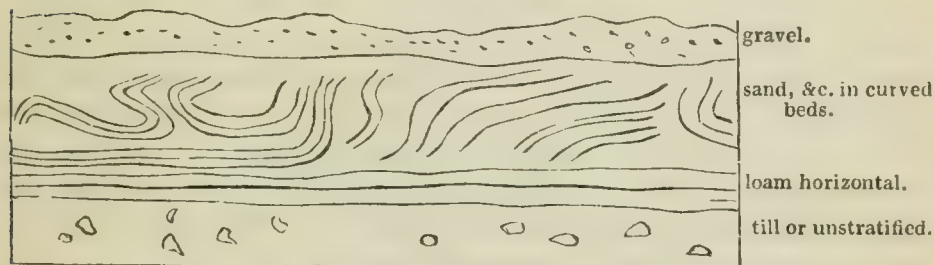
† *Geol. of Norfolk*, pp. 7 and 23.

clay, and converted into dry land, on which forest trees lived; these were afterwards submerged, broken off short near their roots, and buried with their branches and leaves. The subsidence implied by the submergence of the forest continued afterwards, so as to allow of the superposition of a considerable thickness of stratified and unstratified drift.

The general character of the cliffs between Hasborough and Bacton Gap, a distance of about three miles, may be thus described: first, at the bottom, the lignite or forest bed, a few feet thick; secondly, above this, blue argillaceous till, containing boulders of granite and quartz and small pieces of shells from the Norwich crag; thirdly, laminated blue clay resting on the till; fourthly, stratified yellow sand, the entire height of the cliffs being between 30 and 40 feet.

The cliffs between Bacton Gap and Mundesley, a distance of about three miles, are higher, but consist in like manner of drift containing a great variety of boulders, this being usually the lowest bed which is visible. Here we first meet with fine exemplifications of strata which have undergone great derangement since their original deposition, and which present that most perplexing phænomenon the superposition of bent and folded beds upon others which appear to have undergone no dislocation. Thus for instance the annexed section (fig. 1.) represents a cliff about 50 feet high, at the bottom of

Fig. 1.



Cliff 50 feet high between Bacton Gap and Mundesley.

which is *till* containing boulders, having an even horizontal surface on which repose beds of laminated clay and sand about 5 feet thick, which in their turn are succeeded by vertical, bent, and contorted beds of sand and loam 20 feet thick, the whole being covered by flint gravel. Now the curves of the various coloured beds of loose sand, loam, and gravel are so complicated, that not only may we sometimes find portions of them which maintain their verticality to a height of 10 or 15 feet, but the replication is often such that a continuous seam of fine loose sand between two layers of gravel or loam might be pierced three times in one perpendicular boring. As it is clear that some of the underlying

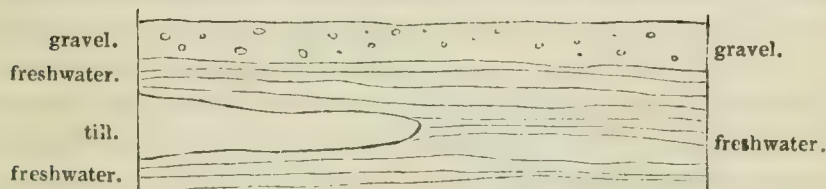
horizontal beds, and apparently the till also, of which the surface is so even, have not participated in these movements even in the smallest degree, we are compelled to suppose that some lateral force has been exerted against the upper masses of drift which has not been applied to the lower ones. Yielding beds having a thickness of at least 15 or 20 feet must in some cases have been subjected to this sideway pressure and moved bodily; and it is impossible to conceive that any original irregularity in the mode of deposition, nor any shrinking or settling of the materials, nor anything in short but mechanical violence, could have produced such complicated folds. Commonly we are in the habit of attributing such movements to a subterranean force acting from below, but it is difficult to imagine how such agency could have disturbed the overlying beds without affecting the subjacent. I shall defer to the sequel the consideration of the various hypotheses which may be suggested to account for such appearances, first describing other irregularities and apparent anomalies which present themselves in this same line of cliffs.

At one spot between Bacton and Mundesley, where the cliff is 50 feet high, I observed at a depth of 30 feet from the top, a small pit or furrow as it were cut into strata of blue clay, and filled with fragmentary white chalk and chalk flint, regular strata of sand and loam being superimposed. This indentation was four feet deep and six wide, and precisely resembles those irregularities which we see in superficial gravel; and they may all be explained if we suppose, that during the subsidence which is indicated by the buried forest, the drift of the mud cliffs was formed in very shallow water, so as to be exposed to the denudation of small streams or currents, by which narrow grooves and hollows were excavated, then filled with drift, and then, after the sinking of the whole, overspread with regular strata.

Freshwater Strata at Mundesley. — Both to the north and south of Mundesley, the cliffs, varying in general from 40 to 70 feet in height, consist in their lower part of blue clay or till, covered with stratified yellow sand and loam; but at the town of Mundesley itself the cliff lowers to a height of between 20 and 30 feet, and for several hundred yards is occupied by a freshwater deposit, covered with about 10 feet of flint gravel. The freshwater beds consist of brown, black, and grey sand and loam, mixed with vegetable matter, sometimes almost passing into a kind of peaty earth containing much pyrites. A few layers also of gravel occur composed of rounded flint pebbles. These beds are often irregular and rarely continuous for great distances. The bottom of the de-

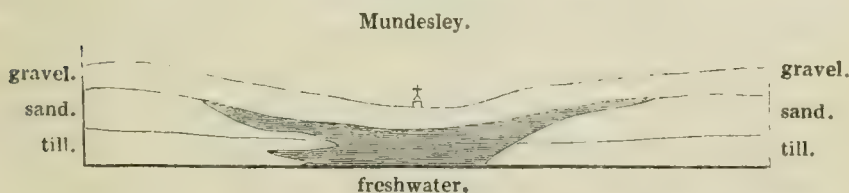
posit is unseen, but is probably not much below the level of the sea, as Mr. Simons of Cromer tells me that he has seen chalk *in situ* at Mundesley at low water. In 1829 I observed a mass of the till prolonged in such a way into the freshwater formation at the southern junction of the latter with the drift as to imply the contemporaneous origin of the lower part at least of both formations. (See diagram, fig. 2.)

Fig. 2.

*Interstratification of drift and freshwater at Mundesley.*

Yet I inclined then to the conclusion that the Mundesley formation, which I traced for nearly 300 yards along the coast, might, as a whole, be considered as the deposit of a lake or hollow excavated in the drift. In 1839, when a new section had been laid open by the sea, it appeared to me rather that these strata of Mundesley were simply a large development of those observable at the base of the cliff at Hasborough and other places, and that their position relatively to the drift might be represented by the diagram, fig. 3.

Fig. 3.

*Position of the freshwater beds at Mundesley.*

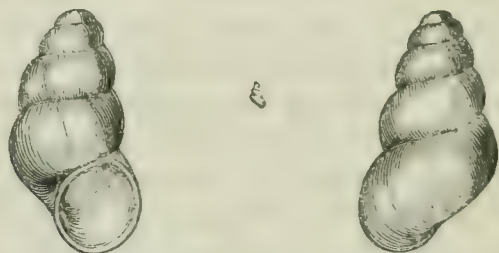
We may imagine that while the coast was sinking gradually, a small river may have entered here bringing down drift wood, freshwater shells, mud, and sand, and the flow of the stream may have partially counteracted those causes by the influence of which the boulder formation was accumulating in the spaces immediately contiguous. The following is a list of the shells which I obtained from the Mundesley beds, including some which Mr. Fitch of Norwich, and others which Mr. J. B. Wigham have kindly communicated to me: 1. *Paludina impura*; 2. *P. minuta*, Strickland, (see fig. 4); 3. *Valvata cristata*; 4. *V. piscinalis*; 5. *Limnea glutinosa*; 6. *L. peregra*; 7. *Planorbis albus*, var. (less flat, and aperture less

oblique than the common form); 8. *P. vortex*; 9. *P. lævis*, Alder, Newcastle Trans.; 10. *Cyclas pusilla*; 11. *C. cornea*.

Specimens of *Unio* or *Anodon* occur, but too imperfect to be determined; one however resembles *A. cygneus*.

Of the eleven shells above enumerated, the only one which is unknown as a living species is the *Paludina minuta*, found by Mr. Strickland in a freshwater deposit at Crophorn, in Worcestershire, and also by Mr. Wood at Stutton on the Stour in Suffolk. Mr. George Sowerby, who has examined this species for me, finds that among recent species it agrees most nearly with the *Turbo thermalis*, Lin., as its volutions are exactly *four*: its apex is more obtuse and its volutions are more ventricose than in other recent species, and it is constantly smaller. (See fig. 4.)

Fig. 4.



Paludina minuta, from the freshwater beds at Mundesley; the middle figure is of the natural size.

Insects.—The elytra of beetles are not uncommon in the clay of Mundesley, especially those of the genus *Donacia*, a tribe which frequents marshy grounds. The beautiful green and gold colours of these wing cases are almost as bright when first the clay is removed as in the living insect, but they soon lose a great part of their lustre on exposure to the light. Mr. Curtis, to whom I am indebted for an examination of these fossils, says that there appear to be two species of *Donacia*, (one of them *D. linearis*?) both probably identical with recent British insects; and among the other remains he found the thorax of an *Elater*, and the elytron of one of the *Harpalidæ* (*H. ophonus* or *H. argutor*). He also refers with confidence another elytron to *Copris lunaris*, a British beetle.

Fish.—I found many scales of fish, together with one large tooth, at Mundesley, and Mr. J. B. Wigham sent me similar remains, together with a smaller tooth, and some vertebræ and ribs of fish. These I submitted to the Rev. Leonard Jenyns and Mr. Yarrell, who referred them to the genera Perch, Carp, Pike, and Trout. The pike appears both by the teeth and scales to be the common *Esox lucius*. Of the salmo there were several small and one large scale, in which the concentric

striæ of growth were extremely minute; the species was not determined. The scales of the perch were very numerous, but they did not agree in a satisfactory manner with those of the common British *Perca fluviatilis* which we were able to procure. The cilia of the free edges were proportionally smaller and blunter in the fossil, and the divisions in the fan at the basal extremity were longer and more numerous. But a more extensive comparison might perhaps have enabled us to identify these fossil scales with those of the living European perch. Most of the vertebræ and ribs may probably belong to this same fish.

Mammalia.—I was informed at Mundesley, that many years ago, when a zigzag road was cut down to the beach, the horns of the Irish elk were found in the cliffs, but I know not where they are preserved and by what naturalist they were seen, nor whether they were found in the freshwater deposit, as is most probable, or the overlying gravel.

Plants.—Among the vegetable fossils the most common and best preserved are the seed-vessels of an aquatic plant which Mr. R. Brown refers to *Ceratophyllum demersum*, English Botany, 947; (see fig. 5.) and I learn from Mr. J. B. Wigham, that his father considers the remains of the accompanying trees and shrubs to be those of the oak, alder, fir, and bramble; but more specimens will be required before a perfect reliance can be placed on these last determinations.

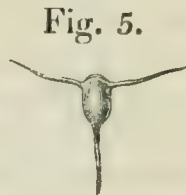


Fig. 5.
Seed vessel of *Ceratophyllum demersum*; Mundesley.

Between Mundesley and Trimmingham the cliffs are composed as usual of drift, the upper part being stratified and more sandy, the lower part consisting of till or blue clay with pieces of white chalk. Whether there intervenes everywhere between this drift and the fundamental chalk a bed of lignite like that of Hasborough or Mundesley, I was prevented from ascertaining by the height of the beach, but I found a substratum of this kind with numerous flattened leaves and branches at the base of a cliff 70 feet high about a mile north-west of Mundesley.

Protuberances of chalk near Trimmingham.—We have now followed the mud cliffs for a distance of about eight miles without finding any chalk *in situ* above the mean level of the sea, but near Trimmingham are three remarkable protuberances of chalk which rise up and form a part of lofty cliffs, the remainder of which consists entirely of drift. These detached masses or outliers of chalk were noticed in Mr. Greenough's map of England, and are described by Mr.

R. C. Taylor, who had opportunities of observing them at low tide as being continuous with the solid bed of chalk extending under the sea for nearly a mile from Trimmingham to Sidestrand, constituting everywhere under water a level platform. He also says that the chalk of this platform contains throughout parallel strata of flint, is harder than that of Cromer or Norwich, is characterized by several peculiar fossils, and occupies, he thinks, a higher place in the series than the chalk at Norwich*.

Now the platform here alluded to is evidently what the sea has left after sweeping away by gradual denudation all that once rose above low water, and it is therefore impossible for us now to conjecture to what height the chalk thus removed may once have risen. The most southern of the three protuberances before mentioned occurs near the Beacon hill, about half way between Mundesley and Trimmingham, and it is in contact with stratified drift the beds of which are highly inclined. The mass of chalk is about 20 feet in height, its extent along the beach about 100 feet, and its thickness from the beach inland a few yards only. It stands up like a narrow wall, which will ultimately be destroyed, and then the whole face of the cliff will consist of clay sand and gravel.

The surface of this wall of chalk, where in contact with the drift, dips inland at an angle of about 45° , and the beds of the newer deposit conform to this slope. As the chalk offers more resistance to the waves than the drift, a small promontory is produced at this point, which projects about 40 feet beyond the general coast line, and by aid of this promontory we are able to see the junction of the chalk and newer beds, both on the north and south side, so that the relative position of the two formations is very clearly ascertained (see fig. 6.)

When I visited this spot in 1829, I found the cliff nearly in the same state as it remained in 1839, and the description which I gave of it in the *Principles of Geology* would still be appropriate†.

But when last there I was able to examine the entire structure of this cliff more thoroughly, and I was more fully confirmed in my opinion that both the chalk and incumbent formation, for the thickness of several hundred feet, must have been subject to some common movement, whether sudden or gradual, by which the strata of both have been tilted.

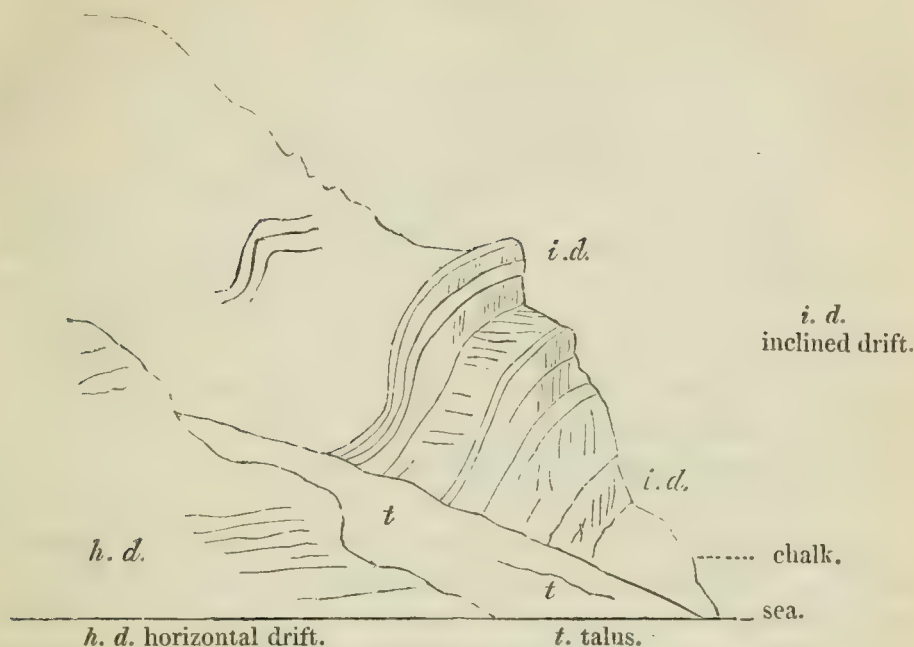
The annexed view of the promontory (fig. 6.) was taken from a point on the sloping cliff a few hundred yards to the south, where the beds have already recovered their hori-

* *Geol. Trans.*, vol. i. 2nd series, p. 376.

† Vol. iii. 1st edit. p. 179, or 5th edit. vol. iv. p. 85.

zontality, although they seem to correspond to the beds of

Fig. 6.



Southernmost protuberance of chalk, Trimmingham.

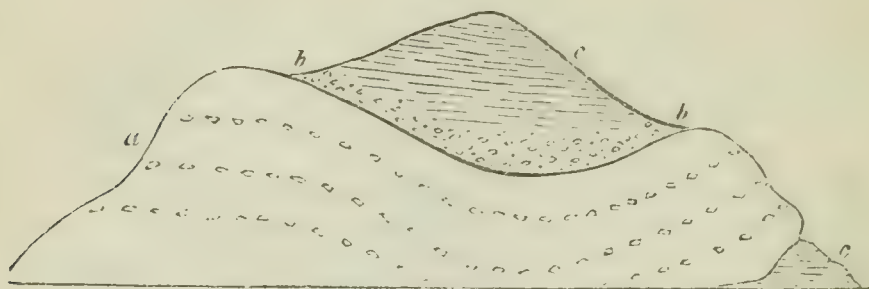
sand and clay which are so highly inclined near their point of contact with the chalk. In the diagram it will be seen that from the projecting point of chalk the cliff retires in a series of ledges and small precipices in which inclined beds of drift, *i, d*, are exposed for an aggregate thickness of several hundred feet. At the top of the cliff, which I conjecture to be about 400 feet above the sea, the beds of sand seemed to be horizontal, but these it should be observed are not immediately over the inclined beds. Respecting the tilted beds which are in contact with the chalk, it will be sufficient to say that they consist of gravel, sand, clay and loam like the stratified drift before described, that the clays are occasionally finely laminated, and that broken fragments of Norwich crag shells are dispersed through some of the strata; but there are no signs here of the freshwater or lignite beds.

The second or middle protuberance of chalk is near that last described: its front along the shore measured in 1839, 65 yards. Its height was between 15 and 20 feet.

The third and most considerable mass extends along the beach for a distance of 106 yards, (see fig. 7.) and its position deserves particular notice, for it forms like the southernmost mass a projecting promontory about thirty yards beyond the general line of cliff. On both sides of this promontory it is seen that the beds of gravel, clay and sand which abut against

the wall of chalk are vertical, (see diagram, fig. 8.) yet the beds of the same formation have but a moderate inclination

Fig. 7.



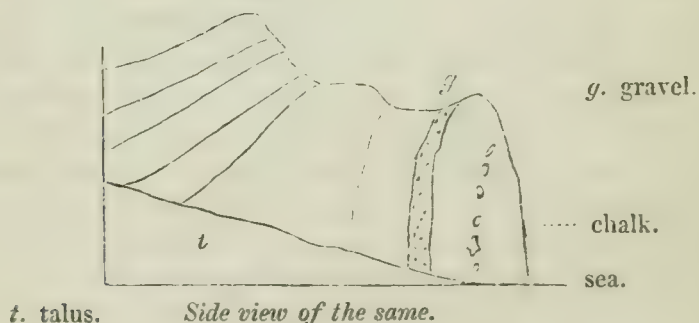
Northern protuberance of chalk, Trimmingham.

- a.* Chalk with flints.
- b.* Gravel of broken and half-rounded flints.
- c.* Laminated blue clay.

in the lofty cliff behind. A layer of chalk flints *in situ* shows that the stratification of the chalk itself is nearly vertical at least in one place, although the beds seen in a large cave facing the sea show a slight curvature only. Where the chalk joins the drift on the southern or Mundesley side of the promontory, I observed in 1839, at the junction, 1st, a portion of the chalk itself decomposed, then a vertical bed of gravel, (*g*, fig. 8) 30 feet high and several feet thick, then dark blue clay with white chalk pebbles, then sandy, and then other beds of ordinary drift. Some of these disturbed beds contain fragments

Fig. 8.

drift.



Side view of the same.

of marine crag shells, as *Cyprina*, *Cardium*, *Tellina*, &c. I have stated in the Principles* that this mass of chalk at its northern edge, or towards Trimmingham, actually overlies some beds of blue clay or drift as at the right hand extremity of fig. 7. Now this remarkable superposition was still evident in June 1839, notwithstanding the unusual height of the sea

* Vol. iii. p. 180, 1st edit., and vol. iv. p. 86, 5th edit.

beach, the clay, containing broken chalk flints, being traceable for seven feet under the chalk. It is known to have extended formerly much further in a seaward direction. It appeared to me impossible that any landslips or movements of the present cliffs could have given rise to this inverted position of the chalk and newer formation. Some persons employed in the Preventive Service assured me that the cliffs immediately above and behind this chalk are upwards of 400 feet high, but they appeared to me less elevated. They also said that in digging a well at Trimmingham at the top of the cliff they reached chalk at a depth of 120 feet from the surface. Without insisting on the precise accuracy of their measurements, I think it by no means improbable that the three protuberances of chalk may belong to a much larger mass, which still forms the nucleus of, the hill called Trimmingham Beacon, and I have no doubt, that as the sea encroaches, the chalk will eventually occupy more of the cliffs between Trimmingham and Cromer.

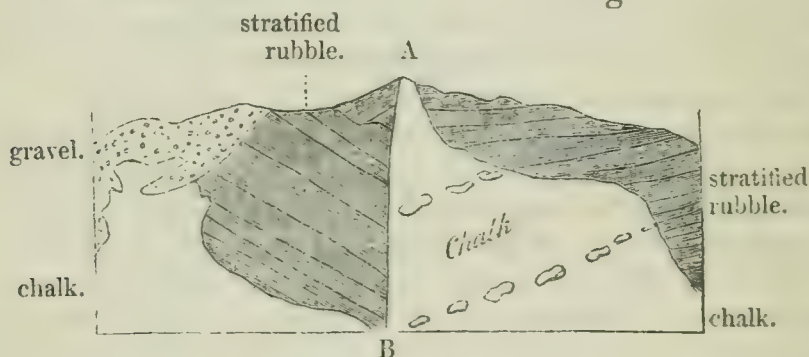
In like manner it may be observed that in other localities further to the north these masses of chalk are included in drift, or where strata of white chalk rubble enter largely into the composition of the cliffs we always find the chalk cropping out in the interior at a short distance from the shore.

In speculating on the time when, and the manner in which, the protuberances of chalk near Trimmingham have been brought into their present position, we may safely assume that the event happened after the deposition of the greater part of the drift, which has been subjected to precisely the same movements, and abuts in some places in vertical beds against the wall of displaced chalk. As the submerged forest before mentioned occurs both to the north and south of Trimmingham at about the level of low water, we must suppose that the Trimmingham cliffs have participated in the subsidence of 300 or 400 feet, and in the subsequent upheaval to an equal amount which the buried forest has undergone. If we imagine the drift to have accumulated gradually while the first or downward movement was going on, we must conclude that the disturbance of the beds did not take place till nearly the whole of this movement was completed; for had it occurred sooner, the upper beds in the Trimmingham cliffs would have been unconformable to the lower ones, whereas they are seen to be conformable throughout a thickness of at least two or three hundred feet of the beds above the chalk. I conceive therefore that the deranged position of the chalk and newer formation was more probably effected during or after the upheaval of the mass, and must in that case have been a very modern

event. Although there is an obvious connexion between the amount of derangement of the newer strata and their proximity to the outliers of chalk, I saw nevertheless no signs of the masses of solid chalk having pierced the newer beds, as if forced through them; on the contrary, it appeared to me in every case, that the lowest bed of the drift, whether inclined at a high angle or vertical, conformed everywhere to the surface of the chalk, as if the same bed might have been originally in contact with it when horizontal. The chalk itself appears to have been in a flexible state, and therefore its beds of flint are variously bent.

Proceeding northwards from Trimmingham, we find the cliffs near Overstrand, about a mile S.E. of Cromer, entirely composed of clay and sand; but this drift does not continue far inland, and if the sea should advance for a few hundred yards, we might expect to see the whole cliff composed of chalk; for at the surface at Overstrand, a chalk pit is worked in which the very disturbed and shattered state of the chalk deserves notice.

Fig. 9.

*Disturbed chalk in a pit at Overstrand, near Cromer.*

In one part of the quarry we find what appears to be a fault, the line A B (fig. 9.) representing 18 feet in vertical height, where the solid chalk with flints, inclined at about an angle of 40° , comes abruptly in contact with alternating beds of white chalk rubble and gravel having an opposite dip, also at an angle of about 40° . After removing part of the chalk rubble I ascertained that the plane of the fault was continuous inwards at right angles to the line of section represented in the annexed diagram. The inclined chalk is covered by beds of stratified rubble resembling those before mentioned.

I stated that there were no signs of the submerged forest or freshwater deposit at the junction of the drift and chalk at Trimmingham, but this forest has been seen by Mr. Simons, about a mile and a half north-west of Trimmingham, at a

place called Sidestrand, where the cliff, composed of drift, is 120 feet high. When I was there the base of the cliff was concealed by a high beach; but when this is removed, beds of laminated blue clay and sand, 6 or 7 feet thick, make their appearance, in which are some trunks of trees 3 feet in diameter, broken off to within a few inches of the roots, which spread for a distance of several feet on all sides. At one point near the bottom of this cliff a stratum of clay has been seen, in which freshwater shells of the genus *Unio*, apparently *U. ovalis*, abound.

At the town of Cromer itself, Mr. Simons has observed beneath the drift, several feet below low-water mark, a bed of lignite, in which were found the seeds of plants, and the wing-case of a beetle.

Norwich crag at Cromer.—At a still lower level than the freshwater beds last mentioned, and only exposed at very low water, is a thin bed of Norwich crag *in situ*, about one foot thick, resting immediately on the chalk. It was barely visible at low tide on the west side of the jetty when I visited Cromer, but with the assistance of Mr. Simons, I obtained many fragments in which pebbles, sand, and shells were aggregated together by a ferruginous cement. The most abundant shells were the *Purpura crispata*, Min. Con., *Tellina solidula*, and *Littorina littorea*, both the common form and the variety called *L. squalida*; I found also a *Fusus contrarius* and *F. striatus*, and *Cyprina islandica*, but I could detect no small or delicate shells, and the deposit had the appearance of having been formed in a shallow sea, and not in still water.

Although the deposit at Cromer varies slightly at each new spot where we examine it, it appears from repeated observations of Mr. Simons that the following section would give a fair representation of the whole: first, chalk, with horizontal surface; 2ndly, Norwich crag, with marine shells, from 1 to 2 feet thick; 3rdly, laminated blue clay, with pyrites, and the bones of mammalia, 8 feet. The upper part of this clay is at about high-water mark, and it forms the beach; 4thly, above high-water mark, layers of pure sand alternating with blue clay, with occasionally patches of gravel. In these beds the bones of mammalia occur and lignite abounds, thickness 10 feet. To these horizontal strata succeed the curved beds of drift, partly argillaceous and partly white and yellow sand, with imbedded masses of chalk and chalk rubble, the whole 60 feet thick.

Among the mammalian remains found on the beach and chiefly *in situ* in the blue clay, No. 3, Mr. Owen has recognized the following: 1. Teeth of *Elephas primigenius*; 2.

tooth of rhinoceros; 3. teeth of horse, the largest which Mr. Owen has ever seen fossil; its longest transverse diameter is 1 inch 4-10ths, which, however, does not exceed that of large living individuals; 4. bones of the ox; 5. horns and bones of a deer of the size of the red deer, and the base of a shed horn of the same; 6. a smaller species of deer; 7. lower jaw left ramus of the beaver, a species larger than the living one and apparently distinct. Among other characters the anterior molar of the lower jaw has a much greater proportional breadth.

The wood collected from the lignite bed, No. 4, is coniferous, and a cone which Mr. Simons procured from the same bed is certainly not the Scotch fir. Mr. R. Brown, who has examined it, has little doubt that it belongs to *Pinus abies*, or the spruce fir, a northern species not indigenous to Britain.

Cromer is the most south-eastern point on this coast at which I observed yellow ferruginous crag; but a blue sand containing the same marine shells has been traced for more than a mile further in that direction by Mr. Simons; and I have lately learnt from Mr. J. B. Wigham, that at Bacton Gap before mentioned, about $8\frac{1}{2}$ miles distant in a straight line from Cromer, the hard ferruginous crag has been found immediately on the chalk. At that place, besides some of the usual shells, teeth of a small rodent (*arvicola*?) have been found, as at Norwich. About a mile *westward* of Cromer the crag re-appears, and again at Runton, as will be presently mentioned.

Freshwater strata of Runton between Cromer and Weybourne.

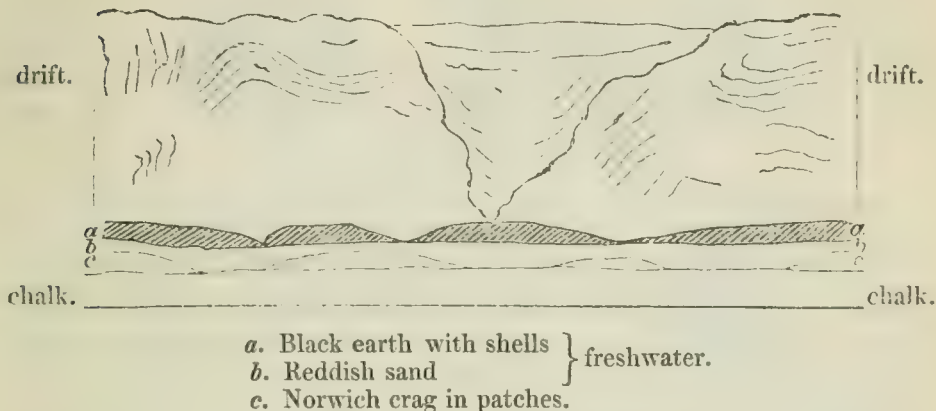
—I shall mention here the only locality in which the freshwater deposit has been seen beyond Cromer, namely, at about $2\frac{1}{2}$ miles N.E. of that town, on both sides of West Runton gap. Here it contains many shells as at Mundesley, and its position is unequivocally at the bottom of the drift, and immediately over the fundamental chalk, which is covered with patches of crag as at Cromer.

This section seen here on both sides of the gap consists, first, of drift, having its usual characters and irregularly curved stratification, and including small dispersed fragments of crag shells, its thickness being 60 feet and upwards. At the bottom of this the freshwater deposit occurs in patches of black earth from 3 to 5 feet thick, under which is a bed of reddish sand about 3 feet thick with freshwater shells in its upper part, and below this the crag in a discontinuous stratum less than a foot in thickness. The fundamental chalk contains large flints or paramoudræ. The lower part of the section

beneath the black earth was covered up in June last, but exposed to view in March, and examined by Mr. Simons.

Fig. 10.

Runton Gap.



I shall now describe, first the freshwater beds and their fossils, and then the fossils of the subjacent layer of crag. The black earth is heavy and turns greenish when dried. It is sometimes divisible into layers, on the surface of which shells are seen in a compressed state; but this is not always the case, the shells being often uninjured and irregularly dispersed. Although the colour of this earth is doubtless due to vegetable matter, I have not found seeds in it, but occasionally small pieces of wood. The most shelly portions which I have seen were sent to me before my last visit to Norfolk, through the kindness of Robert Fitch, Esq., of Norwich. The red sand below resembles the crag in colour and contains the same shells, of which the following is a list, all of which have been examined by Mr. G. Sowerby: 1. *Paludina vivipara*. 2. *P. impura*. 3. *Valvata piscinalis*. 4. *Limnea palustris*. 5. *L. stagnalis*. 6. *Planorbis imbricatus*. 7. *P. albus*. 8. *P. marginatus*. 9. *Ancylus lacustris*. 10. *Cyclas cornea*. 11. *C. appendiculata*. 12. *C. amnica*, var.? Besides these is a small shell allied to *Turbo ulvæ*, but apparently different, of which I only procured one individual; also fragments of *Anodon*. Among these twelve species the only one which could not be identified with well-known British living species is the *Cyclas*, resembling *C. amnica*. It belongs to the sub-genus *Pisidium*, and is remarkable, says Mr. G. Sowerby, "for its great proportional altitude, in which respect it differs not only from the recent *P. amnicum*, but also from the fossil variety of *P. amnicum*, found at Grays in Essex. The concentric ridges on the outside of each valve are much more prominent than in the recent *P. amnicum*, particularly near the beaks, and in this circumstance they resemble the Grays fossil

var. of *amnicum*. The shell appears to be rather thicker than the recent *P. amnicum*, and the teeth stronger: see fig. 11.

Fig. 11.



Cyclas (Pisidium) amnica, var.?

From the freshwater beds at Runton. The two middle figures are of the natural size.

Neither here nor at Mundesley was I able to find *Cyrena trigonula*, which however we might have expected to discover in these beds, as it accompanies a similar assemblage of shells from various localities in Suffolk and Essex.

I found no remains of insects in the black earth, but the Hon. and Rev. R. Wilson, of Ashwell Thorpe, showed me in his collection, in 1838, the elytra of beetles of the genus *Donacia*, preserving their colours, which he had found several years before at Runton. I observed the scales of perch and of other fish resembling those of Mundesley in the black earth. Mr. Simons has also found fragments of the scapula and horns of a deer in the black earth.

In general it is most difficult to speak with certainty respecting the position of fossil bones of quadrupeds derived from the mud cliffs, because they have been picked up at the base of the cliff after portions of it had been washed away by the sea. It is the opinion, however, of collectors that they are chiefly derived from strata, in which the lignite and submerged trees occur. The remains are those of the elephant, rhinoceros, hippopotamus, horse, ox, pig, beaver, deer, &c. At Cromer and Weybourne some mammalian bones occur in the crag, but they are commonly more rolled and worn than those derived from the lignite deposits. Unfortunately no freshwater shells have yet been obtained from precisely the same bed as that in which the bones of the elephant and other extinct quadrupeds are met with, nor from the stratum in which the stools of buried trees are enveloped. The freshwater shells of Mundesley and Runton, although they may probably belong to the same formation, are not yet proved to be strictly coeval with the extinct quadrupeds. The present state, therefore, of our knowledge would not enable us to enter into minute details in regard to the order of superposition of the beds between the chalk and drift in the mud cliffs, but it would appear that the principal site of the bones of extinct mammalia as well as of the buried forest and lignite is be-

tween the marine crag and those beds from which freshwater shells have been procured.

Crag at Runton.—In the patches of marine crag below the freshwater at Runton, the following shells have been found and presented to me by Mr. Simons: 1. *Fusus striatus*. 2. *Scalaria grænlandica*. 3. *Littorina littorea*. 4. *Natica helicoides*, Johnston, (see fig. 12.). 5. *Tellina obliqua*. 6. *T. solidula*. 7. *Cardium edule*, and a fragment of a *Helix*.

The shell which I have called *N. helicoides* is identical with No. 58. in my list of Norwich crag shells published in the Mag. Nat. Hist., vol. iii. new series, 1839, p. 313. I have given it there as a new and extinct species, stating, that it resembled in shape *Paludina solida*, Say. I afterwards learnt from Mr. Edward Forbes that it had been found recent on our east coast in

Berwick Bay, and published by Dr. Johnston in the Berwick Transactions, 1835, under the name of *N. helicoides*. That gentleman has since sent me the recent shell, which is quite identical with the fossil figured above. The species is remarkable for departing from the normal form of the genus *Natica*. It seems to have been much more abundant in the sea of the Norwich crag than in our own sea at present.

Cliffs between Cromer and Sherringham.—The drift near Cromer and to the north of it includes a much larger quantity of chalk rubble than to the southward, and huge fragments of chalk itself are sometimes intercalated in a manner which is very difficult of explanation. It is often no easy matter to decide whether the largest of the chalky masses associated with drift have been regenerated or not, in other words whether they have been brought piecemeal or in mass into their present position; but there are some clear and unequivocal exemplifications of both of these modes of transport. Some of the enormous fragments of chalk which are interstratified with drift have not only layers of undisturbed flints, but also sandpipes in the middle of them, or cylindrical cavities filled with sand and gravel, such as are found penetrating the chalk at various depths from the surface in the interior of Norfolk. These pipes seem to me to imply that such masses of chalk were once at or near the surface of emerged land, but a hasty observer seeing such patches of sand or pebbles in the middle of the chalk might suppose the whole mass to have been broken up and then redeposited, whereas

Fig. 12.



Natica helicoides, Johnston;
from the crag at Runton, near
Cromer.

in fact it has been brought bodily into its present position. The intercalated masses of unregenerated chalk are sometimes horizontal, sometimes vertical. Of the former I observed an example near West or Upper Runton, where a mass of chalk marl 15 feet thick, which I could not distinguish from undisturbed chalk, reposed on stratified blue clay 20 feet thick, and was again covered by stratified loam 30 feet thick.

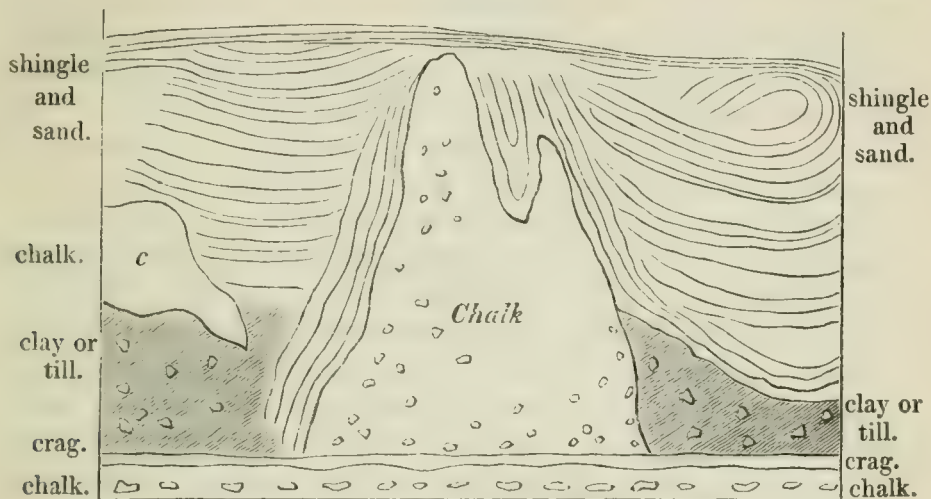
The most remarkable example which I saw of a mass of chalk protruding in the midst of the drift adjoins to Old Hythe Gap about three quarters of a mile west of Sherringham: it is represented on a small scale by Mr. R. C. Taylor in his coast section, though nowhere described as far as I am aware. I found the shape of this mass considerably altered between the years 1829 and 1839, and by a comparison of its appearance at these two periods, I was able to form a more correct idea of its relative position to the chalk and drift than I could possibly have done during a single visit. In order to understand the peculiar position of this great outlier, the reader must be informed, that the fundamental chalk, which at Cromer does not rise above low water, begins, immediately west of Sherringham, to rise and form a ledge a few feet above high-water mark, being usually covered by a hard breccia of crag, commonly called *the pan*, nearly 1 foot thick. The waves at high tides and during storms wash over this ledge, and sweep away the more destructible clay, sand, and gravel of the overlying drift, which is thus made to recede four or five feet inward from the beach or seaward termination of the ledge of chalk. The chalk thus clearly exposed is seen by its horizontal layers of flint to be undisturbed.

The drift sometimes reposes in horizontal and sometimes in curved beds on the pan or ferruginous breccia of crag. At Old Hythe point above mentioned, the beds of drift suddenly become vertical for a height of nearly 70 feet, and flank an enormous pinnacle of chalk between 70 and 80 feet in height, (see fig. 13), which is enveloped in drift.

In this figure the fundamental chalk is seen at the bottom with its horizontal flints, and immediately upon the chalk the pan or layer of consolidated crag, continuous in this spot and varying in thickness from 6 to 12 inches. It contains large chalk flints and fragments of shells cemented by oxide of iron. The broken shells are abundant at some spots. Among them were observed *Cyprina islandica*, *Tellina solidula*, *Mya arenaria*? *Cardium* —, *Littorina littorea*, *Fusus striatus*, *Balanus* —. Next above the crag is the huge pinnacle or needle of chalk, distinctly separated from the fundamental chalk by “the pan.” Chalk flints are scattered somewhat

irregularly through the outlier of chalk, which is distinctly forked in its upper extremity. It will be seen that the pinna-

Fig. 13.



Included pinnacle of chalk at Old Hythe point, west of Sherringham.*

cle is flanked on both sides by drift: that on the east, or Sherringham side (left of the diagram), consists of alternate layers of loam, clay, and white chalk rubble several feet thick, which must have been deposited horizontally although now vertical. These are traceable from within a few yards of the pan to near the summit of the chalk, for a height of 60 feet or more. Between the two prongs of the fork, near the top of the cliff, are curved beds of drift. On the western or right side of the pinnacle the beds of drift are not the same as those on the left. They consist first, and nearest to the chalk, of strata of flinty gravel; secondly, layers of sand with round flint pebbles; thirdly, loose yellow sand, alternating with loam. These join on to curved beds of drift, which are represented near the top of the cliff on the right of the diagram—innumerable layers of sand and shingle, some of them bent round upon themselves and containing seams of carbonaceous matter, or in other places small white pieces of broken shells. Near the bottom of the section argillaceous till rests immediately on the crag, and on one side comes in contact with the chalk pinnacle near its base. Through this till small pieces of chalk and flint are interspersed. Another included fragment of chalk (*c*) occurs nearly half-way up the cliff, enveloped in drift to the westward of the pinnacle.

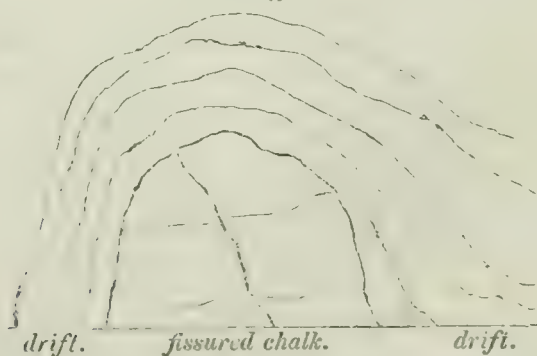
* This sketch is taken principally from my own drawing, but corrected from a view by Mr. Simons taken in March 1840, when the waves during a storm had reached about 8 feet above the level of the pan or crag, removing the talus which previously masked the junction.

The most singular and important circumstance connected with the great outlier of chalk at Old Hythe is the fact of its being perfectly disunited from the subjacent horizontal chalk. I could not myself positively determine this point either in 1829 or 1839, because there was a talus at the base of the vertical cliff resting on the projecting ledge of chalk and concealing the junction; but when the whole was cleared away by the waves in March 1840, after a storm, Mr. Simons visited the spot, and ascertained the continuity and infra-position of the crag which I had before inferred. My inference, previously announced to the Geological Society, was drawn from a comparison of the state of the cliff in 1839, with my sketches and memoranda made ten years before. At both periods I was able to trace the horizontal crag to within 5 feet of the base of the precipice, composed of vertical beds of drift enveloping the chalk; and as the sea had advanced greatly in the interval of ten years, the pan, had it not been continuous, must have been entirely removed before my last visit, in which case nothing could have been visible but chalk on the ledge immediately opposite the pinnacle.

From the summit of Old Hythe point the land slopes down to Old Hythe gap with a rapid descent. It also slopes, though at a less angle, directly inland, so that as the sea advances the cliff at this point will become less elevated. In 1829 the two masses of chalk appeared much more equal in size, and wrapped round as it were both on their sides and at the top with strata of shingle and drift.

Another included mass of pure chalk was also observable in 1839 between Cromer and Lower Runton near the bottom of the cliff. It was traversed by several rents, and alternating beds of laminated clay and sand were bent round it, as in the annexed diagram (fig. 14), which represents a perpendicular section 25 feet in height.

Fig. 14.



Section 25 feet high, west of Cromer.

This mass, although on a smaller scale, may be compared to

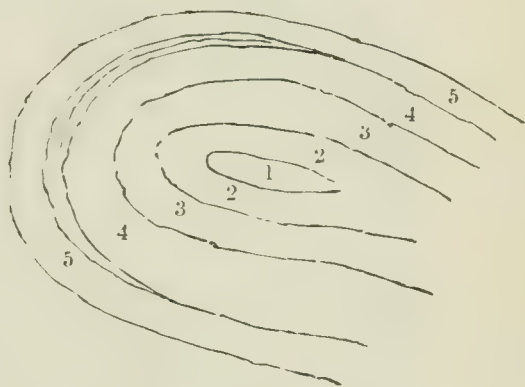
that of Old Hythe point (fig. 13.). It will sometimes happen, however, that the enveloping beds of drift appear to be folded completely round a nucleus of chalk or sand, or any other material found in the mud cliffs as in the annexed cut (fig. 15.) or in fig. 16, which represents a perpendicular cliff 20 feet high,

Fig. 15.



*Folding of the strata between
East and West Runton.*

Fig. 16.



Section of concentric beds west of Cromer.

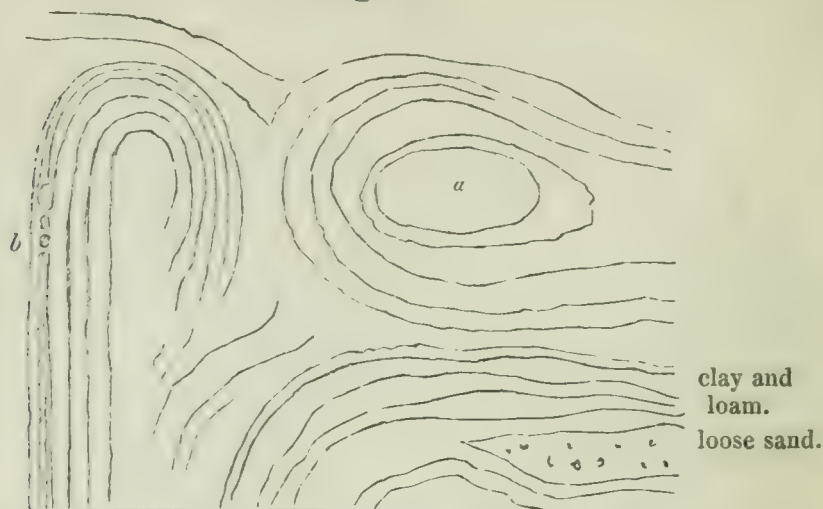
in which the beds are: 1. blue clay; 2. white sand in thin layers; 3. yellow sand; 4. striped loam and clay; 5. laminated blue clay; and I saw curves not far from this place which extended for a vertical height of 50 feet, in which 30 distinct strata, without counting the subordinate laminæ, in all 24 feet thick, presented the same concentric arrangement. The beds consisted alternately of blue clay and white sand, the bed of sand exposed in the centre being blackened by bituminous matter.

I have mentioned some of these cases of the apparent folding of the beds round a central nucleus in the *Principles of Geology*, especially one which occurs in the cliffs east of Sherringham, where a heap of partially rounded flints about five feet in diameter appears nearly enveloped by finely laminated strata of sand and loam, in the midst of which again is a nucleus of loam. After a more scrupulous examination of many of these cases, I have now ascertained that they are all, without exception, examples of the intersection of a series of strata which have been bent into a convex form, the apparent nucleus being in fact the innermost bed of the series, which has become partially visible by the entire removal of the protuberant part of the outer layers.

I observed a portion of a cliff 8 feet in vertical height between Beaston Hill and East Runton, in which a nucleus of very loose sand 18 inches in diameter (*a*) was surrounded by layers of clay and loam as represented in fig. 17. The vertical beds on the left side of the cut consisted of similar incoherent materials, some of the seams of sand being charac-

terized by broken crag shells, and in one place some flint pebbles, *b*, occupying the space of several layers of loam.

Fig. 17.



Section 8 feet high of vertical and curved drift in the cliff near Runton.

Between the Runtons and Sherringham, and at a short distance from the latter place, are seen strata of vertical drift, on the one side of which are horizontal, and on the other curved and folded beds. The change in these cases from the horizontal set to the vertical is very abrupt.

Crag near Weybourne.—It is not until we arrive within less than two miles of Weybourne, that the Norwich crag appears in considerable force *in situ* above the level of the sea, in a cliff about 30 feet high, between Old Hythe Gap and Weybourne. At two different points I observed the chalk in contact with several feet of shelly sand and clay containing pebbles and the fossils of the Norwich crag without any intervening breccia or “pan.” This crag was covered with clay and loam without shells.

About half a mile from Cliffend, Weybourne, the following section appeared, in a vertical cliff about 40 feet high, where I saw the greatest thickness of crag abounding in shells: 1st, horizontal chalk with flints, 8 feet; 2ndly, sand and flint pebbles with crag shells, 1 foot; 3rdly, fine sand with perfect crag shells, 10 feet; 4thly, sand and pebbles without shells, 3 feet; 5thly, unstratified clay or till with flints, 10 feet.

The following is a list of the shells obtained from this crag: *Fusus striatus*, *Littorina littorea*, *L. squalida* (var. of preceding?), *Purpura crispata*, perhaps var. of *P. lapillus*, *Cyprina islandica*, *Cardium edule*, *Cardium echinatum*? *Tellina obliqua*, *T. solidula*, *Nucula Cobboldia*, *Mya arenaria*? *Macra*, *Astarte*.

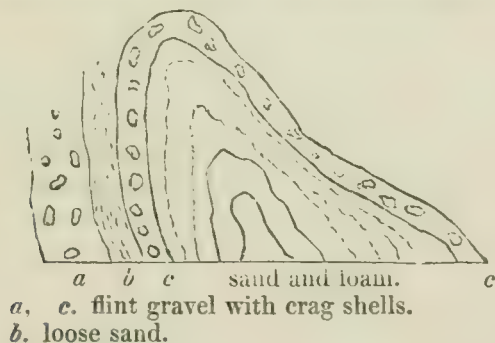
Among the above, the *Fusus striatus* and *Nucula Cobboldiæ* were very rare.

I have remarked, that westward of Sherringham, where the fundamental chalk rises a few feet above high-water mark, its surface, whether covered by the ferruginous breccia or not, is for the most part very level, a singular fact when the contortions of the overlying strata are considered. A slight exception occurs at one place near Cliffend, Weybourne, where the surface of the chalk undulates; so that in the distance of a few paces the chalk sometimes rises 12 feet above the level of the sea, then sinks to 1 foot, and then rises again to 8 feet above that level, being covered everywhere with a similarly undulating breccia made up of slightly rolled chalk flints and crag shells more or less broken.

Finally, near Weybourne, at the extreme end of the cliff, where it is 10 feet in height, the section given in the annexed diagram (fig. 18.) is seen. We here see the shelly crag subjected to the same violent movement so common elsewhere in the drift. The vertical gravel beds *a c* are separated by loose sand. Other loose sand occurs in the arch at *c c*. The crag shells in the gravel, consisting chiefly of *Cardium* and *Cyprina*, are in fragments, and the denudation of such beds may well have supplied those smaller and worn pieces of these shells which are so widely dispersed through the mud cliffs of Eastern Norfolk.

Fig. 18.

Arched beds of shelly crag at Cliffend, Weybourne; height of section 10 feet.



THEORETICAL CONSIDERATIONS.

Age of the deposits composing the mud cliffs.—It has been shown in the above account of the cliffs between Hasborough and Weybourne, that the chalk is everywhere the fundamental rock, lying southward of Cromer at about the level of low water, and rising on the north of that town to the height of a few yards above that level. Its surface between Cromer and Weybourne is covered with occasional patches of

Norwich crag, which is rarely more than one or two feet thick, except near Weybourne. Upon the crag, and where this is wanting immediately upon the chalk, rests here and there a lignite and freshwater formation, which varies in thickness from five to ten feet and upwards. It is seen at intervals throughout the whole line of cliff from Hasborough to Runton. In some places it resembles a bed of lignite, in others a black earth like that found in connexion with peat, while occasionally it consists of gravel, sand, clay, and marl, such as may be met with in any lacustrine deposit. In certain localities it contains the stools of trees, which remain in the position in which they originally grew, and which could only have been buried under the strata now incumbent on them by the submergence of what was once dry land. At Mundesley the freshwater formation is about 40 feet thick and occupies the whole cliff.

As both the crag and freshwater formations are extremely discontinuous in the mud cliffs, we sometimes find the one and sometimes the other in immediate contact with the chalk, while in many places both are wanting, and then the chalk is covered exclusively by drift, of which the great mass of the mud cliffs is composed. A cursory observer, indeed, might see nothing but drift from Hasborough to Cromer, except at Trimmingham, where the protuberances of chalk occur; and the section north of Cromer would seem to present little more than the same drift, with a slight exposure of chalk on the sea beach. The thin stratum of freshwater origin and the subjacent marine crag are most commonly hidden by the beach, or by the sea, except at low water.

Age of the crag.—As to the age of the crag, it agrees with that of Norwich in the species of marine shells which it contains, and the occasional presence of land shells and the rolled bones of mammalia. From the various localities above enumerated, I obtained the following eleven species of shells: *Purpura crispata*, *Fusus striatus* and *contrarius*, *Littorina littorea* and *squalida*, *Scalaria grænlantica*, *Natica helicoides*, *Nucula Cobboldiæ*, *Curdium edule*, *Cyprina islandica*, *Tellina obliqua*, *T. solidula*, and *Mya arenaria*? All of these are known as recent except three, *Fusus striatus*, *Tellina obliqua*, and *Nucula Cobboldiæ*. It would be rash however to pretend to determine the percentage of recent species from so small a number, and the late discovery of *Natica helicoides*, one of the eleven, in a living state, should make us careful not to assume, when reasoning on these more modern deposits, that we have acquired a perfect acquaintance with the present Fauna of our seas.

Age of the freshwater deposit.—Next, as to the age of the freshwater beds, we know as yet too little of the species of mammalia, fish, insects, and plants, which are imbedded in them in considerable abundance, to entitle us to lay much stress on their evidence alone. But we have from Mundesley and Runton, at least nineteen species of shells in an excellent state of preservation, namely, *Paludina vivipara*, *P. impura*, *P. minuta*, *Valvata piscinalis*, *V. cristata*, *Limnea palustris*, *L. stagnalis*, *L. glutinosa*, *L. peregra*, *Planorbis vortex*, *P. imbricatus*, *P. albus*, *P. marginatus*, *P. lævis*, Alder, *Ancylus lacustris*, *Cyclas cornea*, *C. appendiculata*, *C. amnica*, var.? and *C. pusilla*.

Of these all but two are certainly identical with species now living in Great Britain. One of these two, *Cyclas*, fig. 11, p. 364, may possibly be a variety of our living *C. amnica*, while the other, *Paludina minuta*, fig. 4, p. 354, is unknown. I have not included in the list the shell allied to *Turbo ulvæ*, because it would be unsafe to decide on a species from a single individual; nor have I enumerated among the recent species *Anodon cygneus* and *Unio ovalis*, although there is little doubt that the freshwater mussels of Mundesley and Sidestrand belong to these species.

Upon the whole we may conclude that this freshwater deposit must agree very nearly in age with those of Stutton in Suffolk, Grays in Essex, Cropthorn in Worcestershire, and others, which contain nearly the same species, with fossil bones of extinct quadrupeds. It is still a question in all these cases, whether all the species are not living, although some few may not be British shells, or whether there is really a very slight percentage of lost species, to which opinion I incline. It will be seen that the freshwater stratum in the mud cliffs everywhere overlies the crag when in contact. Many, however, of the same species of fluviatile or lacustrine shells are found intermixed with the marine crag itself near Norwich, in which latter the same *Cyclas* figured above (p. 364) is met with.

Age and origin of the drift.—As to the age of the drift, it is proved by direct superposition to be newer not only than the Norwich crag, but also than the freshwater beds at Runton and Sidestrand. At the same time the section at Mundesley (fig. 2, p. 353) seems to prove, that in some places the deposition of the drift was going on contemporaneously with the accumulation of freshwater beds. To frame a satisfactory theory respecting the origin of the drift is difficult. The fluvio-marine contents of the Norwich crag imply the former existence of an estuary on the present

site of parts of Norfolk and Suffolk, including the eastern coast of Norfolk. Into this estuary or bay one or many rivers entered, and in the strata then formed were imbedded the remains of animals and shells of the land, river, and sea. Certain parts of this area seem at length to have been changed from sea into low marshy land, either because the sea was filled up with sediment, or because its bottom was upheaved, or by the influence of both these causes. Two consequences followed: first, trees grew on some spaces gained from the sea; secondly, in other spots freshwater deposits were formed in ponds or lakes, and in the channels of sluggish rivers, or grounds occasionally overflowed by streams. Next succeeded a period of gradual subsidence, by which some of the lands supporting the forests were submerged, the trees broken down, and their roots and stumps buried under new strata. At the same time, the freshwater beds, whether resting on crag or immediately on chalk, became covered with drift, except in certain places, such as Mundesley, where for a small space the accumulation of drift seems to have been entirely prevented, perhaps by the continued flow of a small body of freshwater.

I have met with no fossils so imbedded in the drift as to entitle me to form any positive opinion whether it be of freshwater or marine origin. The regularly stratified arrangement of a large part of it, and the different materials of the alternating strata, clearly demonstrate that it was formed gradually, and not by any single or sudden flood. The boulders which it contains, some of large size, seem to imply, that while a great proportion of the mass may have been derived from neighbouring regions, part at least has come from a great distance. Mr. R. C. Taylor observes, that the shore to the west of Cromer exhibits a singular accumulation of travelled fragments of rocks, whence it would not be difficult to collect a tolerably illustrative series. They consist chiefly of rounded blocks of granite, basalt, porphyry, trap, micaceous schist, sandstones of various kinds, chert, breccia, besides limestone and claystone; also fragments derived from the chalk, plastic clay, London clay, green sand, Kelloway's rock, the oolites, lias, and marlstone; in fact almost every formation above the coal-measures. These, he says, are of all intermediate magnitudes up to four tons weight, large bouldered masses appearing in the sea at low water, lying mixed with flints upon the chalk. One block of granite is stated to be near six feet in diameter, and another mass, standing six or eight feet high, has for some years been known to the fishermen under the

name of Black Meg. This collection extends about two miles, chiefly opposite to Beeston Hill*.

The author just cited truly remarks, that this singular assemblage of boulders must have been dislodged from the wasting cliffs, of which the softer and finer materials have been removed by currents, for similar boulders are occasionally observed in the midst of the clay or till of the cliffs.

In different parts of the interior of Norfolk, boulders weighing several tons have been found in blue clay or till†.

I stated in the first edition of my *Principles of Geology* that I was unable in 1829 to draw a line of demarcation between the crag and the drift or diluvium. The Rev. W. B. Clarke afterwards insisted on the distinctness of the two formations‡, in which opinion I now concur, although I am still unable, in many spots, as, for example, near Weybourne, and between Southwold and Yarmouth, to say where the crag ends and the stratified drift begins. But this difficulty arises from the absence of fossils in the crag as well as the drift, and from the fact that the strata in the latter are often as regular and continuous for considerable distances as those of the crag.

Professor Sedgwick informs me, that in the unstratified brown clay or till of certain parts of Cambridgeshire, large angular blocks of lower green sand and chalk, with fossils of the Oxford clay and lias, occur. The till alluded to attains at some points a thickness of 300 feet: it resembles that in the Norfolk mud cliffs, and has been traced over many of the adjoining counties. Its extent therefore in area and depth render its history of high importance in the geology of the east of England.

I mentioned in the beginning of this paper, that I recognized the strongest resemblance between the boulder formation which I have seen in Sweden, Denmark, Holstein, and other countries, and the drift of Norfolk; and as I believe coast-ice and icebergs to have been instrumental in transporting much of the large and small detritus in Scandinavia, so I presume that at the same period the effects of the same agency was extended to the British seas, although on a smaller scale. But while some of the Norfolk erratics may be of northern origin, other portions of the associated drift may have been brought from neighbouring regions, and perhaps in an opposite direction, just as we now observe that

* *Geology of East Norfolk*, p. 24, 1827.

† C. B. Rose, *Geology of West Norfolk*, Lond. and Edin. *Phil. Mag.*, January 1836, p. 195.

‡ *Geol. Trans.*, 2nd Series, vol. v., part 2, p. 363.

some granitic boulders are floated in ice from the distant shores of Labrador into the Gulf of St. Lawrence, while other large fragments of rock, together with much gravel and sand, are firmly frozen into ice and carried down every winter by various rivers into the same gulf. As the part of Canada where this drift is now forming corresponds in latitude to that of Norfolk, the adoption of this theory of ice-drift does not of necessity require us to assume the former existence of a colder climate than that now prevailing in North America.

Dr. Mitchell, in a paper on the Drift of Norfolk, Suffolk, &c., (Geol. Proc., vol. iii., p. 5) has suggested that the materials have been in great part derived from the destruction of strata which once occupied the site of the German Ocean. This conjecture is, I think, by no means improbable, and we are often too prone, when speculating on the original site of travelled boulders, to refer them exclusively to the places where similar substances happen now to be exposed above water, whereas they may often have come from a neighbouring region now submerged. The island of Heligoland for example, about forty miles off the mouth of the Elbe, has been wasting away for centuries, and in time will probably disappear. Its cliffs, from 100 to near 200 feet high, composed of marl and marlstone of the new red sandstone formation, might supply stony fragments and red mud, which if stranded by ice or other agency on the adjacent coasts of Holstein, Bremen, or Friesland, would differ entirely from the rocks occurring there *in situ*, or from any rocks met with nearer than parts of Hanover, situated 100 or 150 miles in an opposite or eastern direction. We ought always, therefore, to bear in mind, that fragments of chalk, green sand, oolite, and lias, imbedded in the drift of Norfolk and other counties, may not have come from the westward where those formations now crop out, but possibly from the N.N.E., like the erratic blocks, if some of these be really of Scandinavian origin.

The association of stratified drift with unstratified materials or till, a general character of this formation in Sweden and Scotland, as in Norfolk, has been already attributed to the possible cooperation of ice and currents of water (see p. 348).

DISTURBED POSITION OF THE STRATA.

The chalk and overlying formations seen in the cliffs between Hasborough and Weybourne may have been brought into their present contorted and dislocated position by three distinct kinds of mechanical movement; first, by ordinary upheaval and subsidence, to which geologists are accustomed

to attribute the bendings, inclination, and dislocation of strata; secondly, by landslips or the sliding down of sea-cliffs, or the falling in of undermined banks of rivers or of submarine sand banks; thirdly, by the stranding of islands and bergs of ice. It is possible that all these three causes of disturbance may have co-operated to produce the complicated movements which we now behold in the cliffs under consideration.

By ordinary subterranean movement.—First, in regard to ordinary subterranean movements, a general subsidence must, I conceive, have taken place over a considerable area, in order to explain the submergence and burial of the trees of which the stools are found *in situ*; and this forest bed could not have been brought up again, together with the incumbent drift, to the level of low water, without a subsequent upheaval nearly equal in amount to the previous subsidence. But such a depression and re-elevation of a large tract may have taken place slowly and insensibly, and without any derangement of the stratification. A question would still remain, whether such protuberances of chalk as those at Trimmingham (p. 357), and the inclination or verticality of the associated drift, should be attributed to a local and violent movement from below, fracturing the chalk and thrusting up portions of it above the ordinary level of that formation. It is scarcely profitable to speculate on a subject which could only be set at rest if the section were prolonged downwards into the subjacent chalk. I have described in the *Geol. Trans.*, vol. v., part 1, p. 243, masses of drift entangled in chalk at the top of the cliffs of Möen in Denmark; but in those lofty cliffs the section extends downwards for a depth of more than 400 feet into the underlying chalk with flints. The verticality of some of the layers of flint, the curvature of others, and numerous faults, bear testimony to such repeated convulsions, that I did not hesitate to refer the entanglement of the upper chalk and incumbent drift of Möen to subterranean movements. During those convulsions, fissures and chasms may have opened in the chalk, and masses of the superimposed boulder formation may have been engulfed.

There are many sections, such as that represented in fig. 14, p. 368, where the first hypothesis which suggests itself is the protrusion upwards of a boss of chalk, which has forced the yielding and incumbent beds to fold round it, so that the beds become perfectly vertical on the flanks of the protuberant chalk. But it frequently happens that these masses repose on chalk and crag so horizontal and undisturbed, that we are entirely precluded from the supposition of a movement from below upwards.

By landslips and slides.—The last remark leads naturally to the consideration of every combination of causes which can give rise to great disturbance in the overlying beds, while the stratification of those below remains even and unchanged. For striking examples of this phænomenon the reader is referred to figures 1 and 13, in which the superposition of vertical to horizontal drift, and of huge fragments and needles of chalk to horizontal chalk and crag, are clearly exhibited. In order to explain these sections, we may imagine that banks of mud and sand existed beneath the sea in which channels were occasionally excavated by currents. In banks of this kind off Great Yarmouth, a broad channel sixty-five feet deep was found in 1836, where there had been only a depth of four feet in 1822*. If the cliffs of loam or sand bounding this new channel give way, large masses may descend bodily and assume a vertical or curved position. They may easily escape subsequent denudation, because the direction of the currents are constantly shifting. Thus strata which have assumed a vertical position may be forced laterally against the opposite sides of the channels, where the beds have remained horizontal. Both the juxtaposition of vertical and horizontal beds, and the superposition of disturbed to undisturbed strata, may be caused in this manner. The constant descent of strips of land into river beds in the deltas of the Indus, Ganges, and Mississippi, on the subsiding of the annual inundations, are well known, and may give rise to analogous effects.

During the late landslide near Axmouth on the 24th of December 1839, a lateral movement took place, by which masses of chalk and green sand, which had been undermined, were forced more than forty feet in a seaward direction, and thrown into great confusion, while the subjacent lias was not disturbed†. The pressure moreover of the descending rocks urged the neighbouring strata extending beneath the shingle of the shore, by their state of unnatural condensation, to burst upwards in a line parallel to the coast, by which means an elevated ridge more than a mile in length, and rising more than forty feet, has been made to form an extended reef in front of the present range of cliffs. This ridge when it first rose was covered by a confused assemblage of broken strata and immense blocks of rock, invested with sea-weed and corallines, and scattered over with shells, star-fish, and other productions of the deep.

* See Elements of Geol. p. 307.

† I have been indebted to the kindness of the Rev. W.D. Conybeare for a description and section of this landslide, which I have published in the 6th edition of the Princ. of Geol. vol. ii. p. 78.

We may imagine in like manner masses of chalk and overlying drift to have fallen from cliffs, and to have been forced sideways over a floor of horizontal chalk; but it appears to me impossible, even if we adopt this hypothesis, to explain how the Old Hythe pinnacle of chalk (see p. 367) became enveloped by drift, and this drift in great part vertical and resting on horizontal crag and chalk. It seems necessary first to suppose that a needle of chalk was thrown down on horizontal drift, and then that the whole was forced by lateral pressure into a vertical position, the fundamental rocks remaining unmoved.

It cannot be objected to explanations of this kind that ancient cliffs and adjoining needles of chalk are no longer visible, because they may have existed when the country was subsiding, and they may have been removed by denudation, when brought down within the action of the waves.

By pressure of drift ice.—There is still another cause, hitherto, I believe, overlooked, by which great foldings and contortions may be produced in the upper portions of banks of sand and gravel, while the lower remain undisturbed; I mean the stranding of icebergs and large masses of packed ice. In different parts of Scotland, Sweden, Norway, and probably everywhere in Europe where drift is found containing erratic blocks, between the latitudes 50° and 70° north, coiled and folded beds of loam, gravel, and sand are frequent, and I have often seen them in Scotland resting on and covered by strata which remain horizontal.

In the account given by Messrs. Dease and Simpson of their recent arctic discoveries, we learn that in lat. about 71° N. long. 156° W. they found “a long low spit named Point Barrow, composed of gravel and coarse sand, in some parts more than a quarter of a mile broad, which the pressure of the ice had forced up into numerous mounds, that viewed from a distance assumed the appearance of huge boulder rocks*.” So many facts indeed have come to my knowledge of the manner in which masses of ice, even of moderate size, in the Baltic, and still more in the Gulf of St. Lawrence, push before them large heaps of boulders, that I can scarcely doubt that lateral pressure, exerted under favourable circumstances by drift ice on banks of stratified and incoherent sand, gravel, and mud, is an adequate cause for producing considerable flexure and dislocation. The banks on which icebergs run aground occasionally between Baffin’s Bay and Newfoundland are many hundred feet under water, and the force

* Journ. of Roy. Geograph. Soc., vol. viii. p. 221.

with which they are struck will depend not so much on the velocity as the momentum of the large floating islands. The same berg is often carried away by a change of the wind and then driven back again upon the same bank, or in other cases it is made to rise and fall by the waves of the ocean, and may thus alternately strike the bottom with its whole weight, and then be lifted up again until it has deranged the superficial beds over a wide area. On these beds new and undisturbed strata may be afterwards thrown down. In other cases, when banks of mud and sand forming the top of a shoal have been made to assume various shapes by the lateral pressure of icebergs, the bed of the sea may subside, and then the disturbed beds may be overspread by horizontal strata, which may never afterwards be deranged by similar mechanical violence.

LVIII. *On the Method of performing the simple Experiment of Interferences with two Mirrors slightly inclined, so as to afford an experimentum crucis as to the nature of Light.*
By R. POTTER, Esq.*

FRESNEL'S genius devised the experiment which is the most direct test of the reality of the interference of light, and which proves that property in the most unequivocal manner. This experiment is performed by causing the light diverging from a luminous point to be reflected by two plane mirrors, placed side by side, whose surfaces are *nearly* in the same plane, but which contain an angle a little less than 180° , and then examining the light by means of an eye-lens. Each mirror gives an image of the luminous point, and we have the reflected light proceeding as if it diverged from these two images and also from its having originally constituted only one pencil, the two reflected pencils are in the same state, so that they interfere where they cross each other's direction, producing in ordinary light coloured bands parallel to the line of intersection of the planes of the two mirrors, with dark intervals between them. These bands are seen in the air in the focus of the eye-lens when looking towards the images of the luminous point.

Without examining the experiment more minutely than just to ascertain that both the pencils are necessary to the production of the bands, it must be admitted that it is conclusive in establishing the theory of interferences.

The theory of interferences was brought forward by Dr. Young as a consequence of the undulatory or wave theory of

* Communicated by the Author.

light; and it must be confessed that the latter theory was greatly advanced in probability by the demonstration of the former. Certain circumstances, however, such as the colour and arrangement of the bands, are required to be examined before we can consider the primary theory to be confirmed in the same extent as its subordinate theory. For instance, according to the fundamental property of wave interferences the central band must be white; but if it should be found in the experiment that the central band is black, this discrepancy, whilst it would not weaken the demonstration of the theory of interference, would yet be a fatal objection to the theory of undulations.

Now if two equal series of circular or spherical waves which have the same direction, or nearly so, arrive at any points in the transmitting fluid, in such a manner that the like parts of the waves arrive at the same instant, then their conjoint effect will be to produce a resultant wave stronger than either of the component waves. If, however, the two series are in a state of complete discordance, so that one series would produce an effect equal and opposite to that which would be produced by the other series, they would counteract each other, that is, no resultant wave would be produced. Two series of waves, which are respectively of equal diameters, and similar, at every instant may be denominated simultaneous; and if we draw a line bisecting perpendicularly that joining their origins, the waves of each series will meet in the same state everywhere along this line, since every point in it is equally distant from the two origins. When these waves are very distant from the origins compared with the distance of the origins from each other, they will have very nearly the same directions, and we shall have resultant waves of greater strength than the component waves. This is a conclusion depending only on the fundamental properties of waves, and does not involve any review of the various hypotheses which are held, as to the nature of the vibrations of the particles of the fluid through which the waves are propagated.

The experiment with the two mirrors, before mentioned, produces the case we have just discussed; for the single pencil diverging from the original luminous point, is made into two pencils after reflection by the mirrors; and these two, if light consist of waves, are composed of waves both equal and simultaneous. We have here then an experiment on which to test the wave theory of light, and the experiment itself shows us which is the band corresponding to the line bisecting perpendicularly that which joins the luminous images of the point. For if the luminous point be formed of white light,

the various colours having different intervals for their luminiferous surfaces or different lengths of waves in the undulatory theory of light, the bands will be of different breadths for different colours, but will have the one on the bisecting line above named bright for every colour, and which will be consequently white, when the luminous point is formed with white light; but on each side of it, the superposition of bands of different breadths corresponding to different colours, will cause the compound bands to be coloured, at first on their edges only; afterwards the colours will become more and more spread, and the bands at the same time more confused, as the distance from the central band becomes greater, until they are at length gradually lost in a light uniform to the eye. The bands on each side and near to the central one will have their inner edges violet and their outer edges red, so that the arrangement of colours will be symmetrical on each side of the central white band. The central band is thus pointed out in the experiment by the arrangement of the colours.

In my early trial of this experiment, I happened to have a clear sky and unclouded sun, which afforded a result causing me to hesitate before I accepted the undulatory theory as true. I have seen the experiment on days in which there were thin clouds in the atmosphere, and once when the sun was near the horizon, such that it would have led me to a different conclusion.

In my first experiments I was surprised to find the colours symmetrical on each side of a dark band, and not a bright one. Every precaution was used, such as keeping the bands clear of the diffracted fringes formed by the edges of the mirrors, making the distance between the luminous images so small that the bands were very large, and therefore that any prismatic effect produced by slight error of looking centrally through the eye-lens did not produce a sensible effect in the arrangement of the colours; also care was taken that the direct rays (as the term direct is used in optics in contradistinction to oblique) passing through the lens forming the luminous point were those which fell on the two mirrors. Still the central band was a dark one and not a bright one. I obtained the assistance of friends accustomed to accurate observation, to examine the appearances, and they came to the same conclusion. I was thus at a loss to conceive how the advocates of the undulatory theory could state that *the central band was always a white one*. Some time afterwards, however, I obtained a different result; for experimenting one fine evening when the sun was near the horizon, I saw the middle band clearly white, and the colours accurately symme-

trical on each side of it, although the whole appeared misty and without that darkness in the intervals which I had found in the previous experiments. This continued whatever pains I took to keep every part of the apparatus in adjustment. The dullness and mistiness of the latter phænomenon led me to conclude that there could be no doubt but the former was the normal result. I however tried the experiment again with every care in the former circumstances, and found the same result as formerly.

In the Number of *Phil. Mag.* for April, 1833, p. 279, I stated, "The result of considerable experience with me is, that it may be seen both black and white, though with me it has much oftener been the former, especially when the bands have been well defined," &c. &c.

In the November Number for 1833, p. 342, I said, "I shall consider it extremely important to determine whether, when an achromatic lens of short focus is used to form the luminous point, the central band of direct interference given by two mirrors is black, as it has appeared to me, and to several friends to whom I have shown it, when adequately tried with a common lens," &c., &c.

I have in the papers from which the above extracts are taken, publicly stated my difficulties without reserve, and my ideas of the requirements for a decisive mode of experimenting.

A mode of experimenting has been adopted, (where or with whom the discovery originated I do not know,) which is stated to give the central band white. It is this: the image of the sun formed in the focus of a lens is made to fall on a small aperture in a thin plate of metal; the light passing through the aperture falls on the two mirrors, and the aperture thus illuminated is called the luminous origin. I am informed, that when the rays falling on the two mirrors are those which pass obliquely through the aperture, then the central band is most distinctly a white one; and it has been argued that this must be the normal way of trying the experiment, inasmuch as the rays of all colours will have accurately a common origin on the edge of the aperture.

It has been with me a subject of frequent study to find out a method of trying the experiment which must give a result not to be disputed; and I feel confident the apparatus described in this paper fulfils every desideratum. The points kept in view have been to employ only such parts as are essential in the simplest form of the experiment; and thus in dispensing with the mirror which is

usually employed to throw the sun's light through the window-shutter into the darkened room, I have employed an equatorial method of mounting, which keeps all the apparatus in the adjustment it is first placed in, and at the same time enables us to follow readily the sun's daily motion, keeping at the same time the room quite dark.

To obviate all objections which attach to the luminous point being formed by a common lens, which has different foci for differently coloured rays, I have employed a spherical mirror, as better even than any achromatic lens; and again, as it was suggested to me that an objection might be raised if the rays crossed in a real focus, I have used a convex mirror, so that they diverge from a virtual focus without having crossed: also to enable me to use the luminous point the smaller, I have generally used the two mirrors slightly inclined, of polished speculum metal, which reflects many times the quantity of light which glass reflectors do in the position which my apparatus requires. With these precautions, I find, when the sun is perfectly unclouded, and near the meridian, *high above the horizon*, that the central band is black. When there are clouds before the sun's disc, however slight, the central band is more difficult to fix upon, and generally either white or doubtful. The discrepancies which have been to me a puzzle for so many years, I am now able to solve, and I announce the following new principle of interferences:—

When light in a state of interference is made to interfere again, the result is of an opposite character to what it would have been if the light had been in the first instance in the ordinary state.

This proposition, which is in itself reasonable, might have been anticipated, and solves all the anomalies. The light falling on the small hole in the thin plate of metal, formerly mentioned, as used to form the luminous point, is thrown into a state of interference by diffraction at the edge of the hole, and hence the central band is seen white. Again, the sun's light passing through thin clouds, or through the vapours of the atmosphere when near the horizon, is thrown more or less into a state of interference by diffraction at the edges of the particles of the vapours, and gives results which are either doubtful or with a white centre: for where part of the light is in its original state and part in a state of interference, the bright and dark centred bands will be superposed, the central band of the one set over the central band of the other, and thus produce indistinct phænomena in which the intensity of the one or the other species may prevail.

From the full investigation, I thus maintain that the normal result of this fundamental experiment in interferences, is, that the central band is black, and at variance with the consequences of all wave theories of light, and therefore the undulatory theory of light is not the physical theory.

This conclusion is also borne out by the phænomena of the rainbow, (see *Lon. and Edin. Phil. Mag.* for July 1838, vol. xiii. p. 9.) the actual primary and secondary bows being much more distant than they should be from all calculations according to the undulatory theory of light, the position calculated by that theory to be bright being actually found to be dark. In the artificial methods of producing the analogous effect with a minute stream of water, there is no doubt but that the effect calculated from the undulatory theory might be obtained, by using a *line of light* in a previous state of interference.

The apparatus is represented in figs. 1 and 2, in which the

Fig. 1.

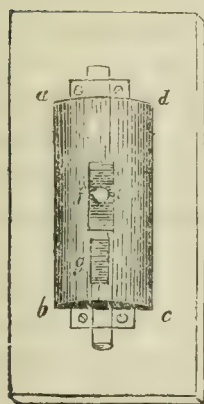
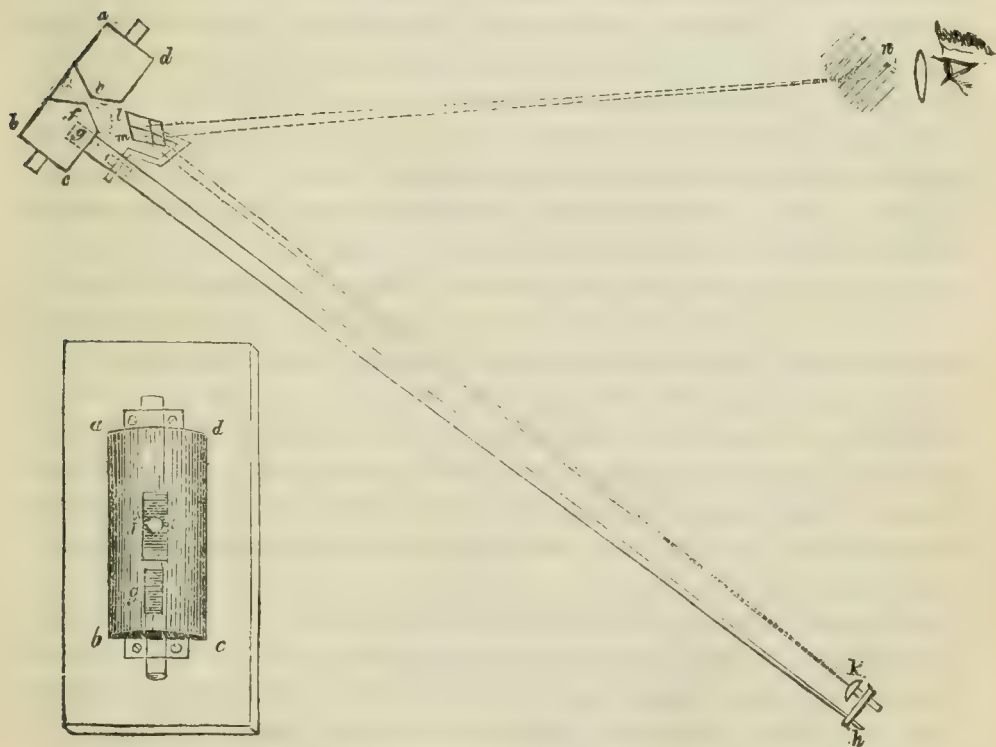


Fig. 2.

same letters refer to the same parts: *a, b, c, d* is a cylinder of wood about three inches in diameter, and seven inches in length from *a* to *b*, with pivots as in the figures. In fig. 2 this cylinder is seen placed in a rectangular aperture cut in a thick piece of board in which it exactly fits, and is retained in

its place by steps screwed over the pivots. An aperture is cut straight through the centre of the cylinder, which in the axis at *ef* is circular, of a little more than half-inch diameter, but towards the surface it is extended longitudinally: through it the sun's light falls on the small convex mirror *k*. This mirror is attached, as in the figure, to the arm *g h*, made of light wood, about four feet long, which fits tightly in a lateral direction in a groove *g*, fig. 2, but can be moved in a plane passing through it and the axis of the cylinder, so as to place the mirror in the sun's light shining through the hole. The axis of the cylinder is fixed parallel to the earth's axis, when the apparatus is finally secured in the window-shutter of a room with a south aspect. The form of the aperture allows for the sun's declination at different times of the year, by which means, and the motion of the arm, the sun's light can be made to fall on the mirror *k* at all seasons; and to follow the sun's daily motion we have only to push the arm so as to turn the cylinder about its pivots. Every part is made to fit just so tight that the apparatus remains in any position in which it is placed. The two mirrors are placed as at *l m*, so that the sun's light just passes their edges when it falls on the mirror *k*; by this arrangement we make it certain that the light received by the two mirrors is that which is reflected nearly directly by the mirror *k*, and therefore with very little aberration. The two mirrors are attached to a piece of wood, which, having a hole in the line of contact of the mirrors, is moveable about a thick wire which fits tightly into this hole. This piece of thick wire is bent at right angles in three places, as seen in fig. 1, and forms a very convenient universal joint, which allows the mirrors to be turned about in any direction required: at its lower part it is pushed through a hole in a piece of wood fixed to the side of the arm *g h*. The dotted lines *n l m k*, are intended to show the course of two rays from the mirror to their interference at *n* in the focus of the eye-lens.

All the parts of the apparatus should be well blackened, and a piece of black velvet (as the most perfect black) placed behind and about the mirror. It is also desirable to have a tube of blackened paper placed on the arm near *k*, through which the sun's light may shine, but which will prevent the stray light from the mirror *k* injuring the darkness of the room.

Those who understand analytical geometry will find the readiest way of fixing the cylinder parallel to the earth's axis, to be that of calculating the lines of intersection of the board in fig. 2 with the plane of their window-shutter: great accu-

racy is not needed in fixing the cylinder, as its use is merely to afford the means of following the sun's daily motion and still to leave the room dark.

As the apparatus is here drawn, it will be advisable to have the mirrors *m l* of speculum metal, for the angle of incidence of the light is too small for glass mirrors to give very bright phænomena; although I have seen them of the same character as with the mirrors of speculum metal. The mirror *k* I have used of one-fourth of an inch focus.

All the mirrors should be of metal which is not porous, and of the highest polish, so that their surface is not visible when they are placed a short distance from the flame of a candle; for it is probable that numerous small pores in the metal, or the fine lines which constitute a second-rate polish, would produce diffraction sufficient to invalidate the experiment. As such mirrors are not everywhere to be had, I should, in such cases, recommend, as a substitute, that a good lens, of short focus, well centred, be placed in the aperture *e f*, and that a pair of glass mirrors blackened at their second surface, be placed on the arm at the other end *h*: the incidence would then be sufficiently oblique to afford an intense reflexion. Ordinary plate glass, or good pieces of window-glass, will show the bands very well; but those who wish to test the colour of the central band, should have their mirrors, as well as their lens, of good workmanship. The obtuse-angled prisms which may be obtained in the shops, are very convenient for observing the bands popularly, but are not to be depended upon as test apparatus.

Those who use the two mirrors for the first time will find some care necessary in fixing them with their contiguous edges in contact, for if the edge of one be raised any appreciable distance above that of the other, the bands are not produced. With glass mirrors, blackened at their second surface by sealing-wax melted over them, it is convenient to cement them together with the same material along their line of contact. If they are placed in position whilst the sealing-wax is soft, and then retained in their position until cool, they will remain ready adjusted to be used at any time.

I have been the more minute and popular in explaining the theoretical bearings, as well as the experimental details, from the consideration that this interesting and important experiment has very singularly been but little noticed in our more descriptive optical treatises; and also from the hope that I may by this essay induce others to try it, who have a sincere wish to form a correct opinion on the nature of light.

LIX. *On the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c.* By THOMAS WEAVER, Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.

[Continued from page 297.]

AS a conclusion to this paper, and as connected with my subject, I am tempted to advert to the disposition so prevalent among geologists of placing in parallel British with foreign formations, without always maturely considering all their respective peculiar relations. As a case in point, none comes more readily to my hand than the late attempt of M. Dumont to assimilate the transition tracts adjacent to the Rhine, extending from the north of France through Belgium into Germany*, to the Cambrian and Silurian systems of Professor Sedgwick and Mr. Murchison†. From this author we had previously derived much valuable information concerning that region‡; but the parallel since drawn by him between the Belgian and British formations is the more remarkable, as it is confessed that the two countries do not correspond in the development of the mineral masses, nor yet in the distribution of organic remains. Nay, so far as the latter have hitherto been ascertained, there appears to be but a slight analogy between them, and which I now propose briefly to show, first introducing M. Dumont's table of equivalents, for the sake of reference.

	Belgium.	England.	
Terrain Anthracifère, 'Anthraciferous tract.'	Terrain Houiller 'Coal Tract'	{ Coal measures. Millstone grit.	Carboniferous system.
	Upper calcareous system. { Limestone. Dolomite. Limestone.	{ Carboniferous limestone.	
	Upper quartzo-schistous system. { Sandstone. Limestone subordinate. Schist. Limestone subordinate.	Old red sandstone.	Silurian system.
	Lower calcareous system. { Limestone. Dolomite. Limestone.	{ Upper Ludlow rock. Aymestry limestone. Lower Ludlow rock.	
	Lower quartzo-schistous system. { Grey fossiliferous schist. Schist and red sandstone. Conglomerate. Sandstone, quartz-rock, schist.	{ Wenlock limestone. Wenlock shale.	
	Terrain Ardoisier, 'Slate Tract.'	{ Caradoc and Llandeilo formation.	
	{ Upper. Medial. Lower.		{ Cambrian system.

* For a connected view of these tracts, it will be useful to refer to the "Geognostic Map of Germany and the adjoining States," published by Schropp and Co., Berlin, 1826.

† *Bulletin de l'Académie Royale des Sciences à Bruxelles*, Novembre 1838; see also a translation of this Memoir in the Lond. and Edinb. Philosophical Magazine for August 1839.

‡ A. H. Dumont, *Sur la Constitution Géologique de la Province de Liège*,

In the Ardennes, the clayslate tract (*terrain ardoisier*), referred to the Cambrian system, is divided into three stages: the lower destitute of fossils; the medial, in which roofing slate abounds, but in which few fossils have hitherto been found; and the upper, more quartzose than slaty, in which the vestiges of life begin to acquire greater extension, and in which the first indications of limestone appear, the latter being generally slaty, containing numerous crinoidal remains, and forming beds about a yard in thickness, but never occurring in mass like the limestone of the anthraciferous tract. In this upper stage M. Dumont notices *Orthoceratites*, *Crinoidea*, *Polyparia*, *Trilobites*, *Strophomena*, &c.; but according to M. d'Omalius d'Halloy, the most common fossils which it displays are *Spiriferæ* greatly expanded in the direction of their breadth, besides species referable to the genera *Calymene*, *Asaphus*, *Orthoceras*, *Hamites*, *Leptaena*, *Strophomena*, and remains of *Crinoidea* and *Polyparia**. M. d'Halloy admits that the fossils of this clayslate tract have not been determined with sufficient exactness. Yet as far as developed by himself and M. Dumont, can it be safely maintained that a precise analogy has been established between them and those found in the Cambrian system?

The clayslate tract, thus referred to the Cambrian system, passes in its upper stage by insensible gradations into the anthraciferous tract†, both on the north-western and south-eastern sides of the Ardennes; in the former direction in Belgium, and in the latter in the Eifel.

In the anthraciferous tract, the lower quartzo-schistous system, referred partly to the Llandeilo and Caradoc formation and partly to the Wenlock shale, is divided by M. Dumont into three stages: the lower, which contains no limestone nor fossils, or at least the latter, are extremely rare; the medial, in which the remains of organized bodies are also very seldom exhibited; and the upper, distinguished by the abundance and variety of its Shells and *Polyparia* from all other portions of the anthraciferous tract. In the upper stage, nodules or beds of fossiliferous limestone are often con-

Bruxelles, 1832, a work respecting which I agree with M. Beyrich, that it does not appear to have drawn as much attention as it deserves. See the same author also on the Ardennes, Belgium, &c., in the *Bulletin de l'Académie Royale des Sciences à Bruxelles*, November 1836 and November 1837.

* *Elémens de Géologie*, troisième édition, 1839, pp. 476, 477.

† *Terrain anthraxifère*—this term, introduced in 1808, by M. d'Halloy, is admitted by the author not to be good, since anthracite is found in other groups, and it does not occur in all the systems which compose the tract designated as anthraciferous by himself and M. Dumont.—*Elémens de Géologie*, third edition, p. 438.

tained in the schistose rocks. But the only fossils enumerated hitherto are referred to the genera *Producta*, *Spirifera*, *Strophomena*, and the remains of *Crinoidea* and *Polyparia*. Where, then, it may be asked, are the peculiar organic remains of the Llandeilo and Caradoc formations and of the Wenlock shale, to which this system is referred?

In the lower calcareous system, referred to the Wenlock limestone, ten species of coral are enumerated, six of which occur in the Wenlock limestone, and eight in the Eifel limestone*. Two of them are also met with in the carboniferous limestone of Liège and Namur, namely, *Cyathophyllum cæspitosum*, *Cyathophyllum pentagonum*; and one in the lower part of the coal formation of Liège, namely, *Cyathophyllum quadrigeminum*. Of *Terebratula* we have two species common to the Wenlock limestone, namely *T. prisca* and *T. aspera*, besides two or three other species not found in the latter. If to these we add *Strophomena*, *Solarium*, *Nerita*, and *Crinoidea*, we have the whole of the organic remains enumerated as belonging to this calcareous system in his work on the province of Liège: but at a later period the author remarks†, that in this lower calcareous system are found also *Terebratula concentrica*, some *Spiriferæ* and *Euomphali*, besides *Productæ* and other shells which are commonly met with in the upper calcareous system (namely, the carboniferous limestone). Such being the case, it may be asked, wherein does the analogy consist between this lower calcareous system and the Wenlock limestone? Where are the numerous characteristic fossils of the latter? On the other hand, the occurrence in this transition limestone of fossils which are common also in the carboniferous limestone, deserves the attention of geologists, since it forms a parallel to similar phænomena in Devon and Cornwall, and the south of Ireland.

The upper quartzo-schistous system is referred by M. Dumont to the Ludlow formation. But where do we find the various fossils of that formation as enumerated by Mr. Murchison? The only corresponding species noticed are *Spirifera radiata*, *Terebratula aspera*, and *T. Wilsonii*. The rest of the fossils given appear more nearly allied to the upper calcareous system (the carboniferous) than to the lower calcareous (the transition). As bearing on this question, it may also be noticed that in the upper part of this system a bed of coal has been found.

Of the upper calcareous system there is no question among

* These will appear in the tables given further on.

† See Lond. and Edinb. Phil. Mag. for August 1839.

geologists as to it being referable to the carboniferous limestone. I may here remark also, that it contains among its upper beds one or two seams of coal. It exhibits many fossils that are common to the carboniferous limestone of the British isles; but in it are found also some species which occur in the Silurian system and other transition regions, namely, *Calymene macrophthalma*, *Orthoceras striatum*, *Terebratulula lacunosa*, *Cyathophyllum turbinatum*, *Cyathophyllum cæspitosum*; besides one fossil, which is a native of the transition districts of Normandy, Brittany, and Anjou, namely, the *Calymene Tristani*. The only Goniatite noticed in this carboniferous limestone is *Gon. sphaericus*; but in the lower part of the succeeding coal formation there occur also *Gon. Listeri*, *Gon. Diadema*, and *Gon. atratus**.

Even this brief review may be sufficient to show that there is but little analogy between the three lower systems of the anthraciferous tract in Belgium and the formations of the Silurian system, to which they have been assimilated; that is judging by the organic remains hitherto elicited from the former as compared with the latter. What other fossils may be discovered by the extended researches of M. Dumont and others remains to be seen. At present the author appears to be fully justified in his remark, that "though the English divisions established by Mr. Murchison are very good for England, as being founded on the existence of fossils which appear to be different in each of them; yet these divisions must present palæontological differences more or less remarkable in other countries, and that this is in fact what takes place in Belgium: and he therefore proposes to draw the attention of the Royal Academy of Brussels to this subject at a future period†." With such an admission, would not the term *Belgian* have been more appropriate than that of *Silurian*, as applied to these formations? Premature approximations tend rather to retard than promote the advance of science. It has been well observed by a profound judge, that, "as we must be careful not to apply our domestic types without modification to other regions, so we must take care not to despair of modifying our scheme, so that it shall be more extensively applicable than it at first appeared to be‡." It will be well for geologists to bear this reflection in mind. Much, I apprehend, remains yet to be accomplished, before anything like a definite order

* See the works of MM. Dumont and Beyrich already quoted.

† *Bulletin de l'Académie Royale des Sciences à Bruxelles*, November 1838, and *Lond. and Edinb. Phil. Mag.*, August 1839.

‡ *Proceedings of Geol. Soc.*, Address of the Rev. W. Whewell, as President, February 1839. [*Lond. and Edinb. Phil. Mag.* vol. xiv., p. 450.]

can be established in the sequence of the transition formations.

M. Beyrich considers the lower calcareous system of M. Dumont as identical with the Eifel limestone. And M. Dumont also places the Eifel limestone in parallel with the lower limestone of Belgium, finding below them both the same lower quartzo-schistous system reposing on the clayslate tract of the Ardennes, and tracing all these formations to the banks of the Rhine. In this view, I believe, he is correct.

The evidence as far as produced leads to the conclusion, that the north-western and south-eastern flanks of the clayslate tract of the Ardennes, together with the succeeding lower quartzo-schistous and lower calcareous systems on each side, in Belgium and the Eifel, are justly referred to the transition epoch. From the same evidence it would also appear that the upper quartzo-schistous system is more nearly allied to the carboniferous than to the transition series; and I conceive it may be composed in part of alternations of old red sandstone with beds of carboniferous limestone*, while the succeeding mass of the carboniferous limestone and the coal measures follow in their regular order.

That the whole of these series, extending from the north-western flank of the Ardennes to the Belgian coal formation inclusive, should have been formerly referred by Continental geologists to the transition æra in general is not very surprising, since they are commonly represented as forming an uninterrupted succession (yet with some exceptions), passing one into the other, and as having been subjected to the same disturbing forces; while they exhibit also some fossils that are common both to the lower and higher members of the series. But the *old red sandstone* of British geologists has been usually thought to be wanting in the succession, both here and in the corresponding tracts extending hence beyond the Rhine into Germany. M. Dumont originally conceived the *lower* quartzo-schistous system to represent the old red sandstone†, an idea afterwards abandoned; and which has been since also repudiated by M. Beyrich, who justly observes, that were we to adopt such a view, we should have scarcely anything in the whole Rhenish slate mountains but old red sandstone and carboniferous limestone, the latter of

* This view appears supported by the pertinent observations of M. Von Dechen, at p. 484 of his modified translation of M. de la Beche's Manual, in which he refers to the beds of red conglomerate which occur on the Meuse near Lustin and Profondeville, at Hayoux, south of Huy; on the Vesdre near Pepinster, and on the Vichbach at Binsfelser Hammer.

† In his Memoir on the province of Liège, p. 67.

which would fall into two great divisions, of which the lower would be the Eifel limestone: but such an arrangement, he observes, would be quite inconsistent with nature, as proved by the evidence of organic remains*. That the old red sandstone, however, occurs in considerable force in various parts of Germany, I showed in the year 1821, when giving abstracts of the observations of M. Freiesleben on the Forest of Thuringia, on Mansfeld, Thuringia, and the Circle of the Saale, and of those of MM. Von Buch and Von Raumer on Lower Silesia, the county of Glatz, and part of Bohemia and Upper Lusatia. And at that time, this formation was also conceived to exist in partial distribution in the Netherlands, then forming the immediate support of the carboniferous limestone†. This view has been more recently revived by MM. Rozet and Constant Prevost, who contend that the old red sandstone of Britain is not wanting in Belgium, stating as an example, that it is well developed between Dinant and Namur; that is, between the lower Belgian limestone which is in force at Givet, and the carboniferous limestone which prevails at Namur; and hence that M. Dumont was in error when he excluded that formation altogether from the series‡. This appears to accord with the prior observations of M. Von Dechen, to which I have referred above.

But perhaps no evidence is more conclusive of the want of similarity between the Belgian and the Silurian formations than that which is to be derived from a consideration of the fossils of the Eifel limestone (which, as already shown, is considered as identical with the lower limestone of Belgium), placed in parallel with those of the Wenlock limestone; the fossils of the Eifel having been determined to a much greater extent than those of the other formations, by the labours of Professors Goldfuss and Bronn, and MM. Von Dechen, Steininger, Dumont, and Beyrich; while the fossils of the Wenlock limestone have been well developed in Mr. Murchison's highly valuable work. To institute this comparison, I have drawn up the following tables, founded upon a comparison of the works of those authors, which may serve to place the subject in a clear light:—

* *Beiträge*, p. 4, General observations on the fossiliferous strata of the Rhenish transition slate mountains.

† *Annals of Philosophy*, October 1821; *Ibid.*, the same subject continued in August 1822, May 1823, and July 1824.

‡ *Bulletin de la Société Géologique de France*, tome ix., Séance de 18 Decembre, 1837.

I. General Table of classes and orders in both the Wenlock and Eifel limestones, with the number of species distinct and common in each.

Wenlock limestone.	Total Species.	Distinct Species.	Species common to both limestones.	Distinct Species.	Total Species.	Eifel limestone.
Crustacea	15	14	1	9	10	
Annelida	1	1	—	3	3	
Mollusca.						
Heteropoda...	2	2	—	3	3	
Cephalopoda..	10	9	1	16	17	
Gasteropoda..	8	7	1	22	23	
Conchifera	27	18	9	69	78	
Crinoidea	14	12	2	19	21	
Polyparia.....	58	30	28	24	52	
Sedis incertæ....	2	2	—	2	2	
	137	95	42	167	209	

II. Table of genera and species common to the Wenlock and Eifel limestones.

CRUSTACEA, Trilobite	1	<i>Calymene macrophthalma.</i>
MOLLUSCA.		
— Cephalopoda.....	1	<i>Orthoceras annulatum.</i>
— Gasteropoda	1	<i>Euomphalus carinatus.</i>
CONCHIFERA	9	<i>Terebratula Wilsonii</i> (<i>Terebratula lacunosa, Dalm.</i>)
		<i>Spirifera trapezoidalis</i> ; <i>S. crispa, Dalm.</i> , (<i>S. octoplicata, Sow.</i>); <i>S. radiata.</i>
		<i>Atrypa reticularis</i> (<i>Terebratula prisca, Schlot.</i>).
		<i>A. aspera</i> (<i>Terebratula aspera, Schlot.</i> , including <i>T. explanata, Schlot.</i>).
		<i>A. galeata.</i>
		<i>Leptæna euglypha</i> , <i>Leptæna depressa.</i>
CRINOIDEA	2	<i>Cyathocrinites rugosus.</i>
		<i>Actinocrinites moniliformis.</i>
POLYPARIA	28	<i>Aulopora serpens</i> , <i>A. tubæformis.</i>
		<i>Glaucanome disticha.</i>
		<i>Fenestella antiqua, Lons.</i> (<i>Gorgonia antiqua, Goldf.</i>).
		<i>Fenestella prisca, Lons.</i> (<i>Retepora prisca, Goldf.</i>).
		<i>Discopora?</i> <i>favosa, Lons.</i> (<i>Cellepora favosa, Goldf.</i>).
		<i>Discopora antiqua, Lons.</i> (<i>Cellepora antiqua, Goldf.</i>).
		<i>Ceriopora granulosa.</i>
		— <i>affinis.</i>

POLYPARIA (continued).

Ceripora punctata.

——— oculata.

The three last species are given on the authority of Goldfuss, as occurring in the Dudley limestone.

Stromatopora concentrica.

Favosites alveolaris, F. gothlandica, F. polymorpha, F. spongites, F. fibrosa.

Catenipora escharoides.

Porites pyriformis, Lons. (Astrea porosa, Goldf.).

Astrea ananas de Blainville (Cyathophyllum ananas, Goldf.).

Caryophyllia flexuosa.

Cyathophyllum dianthus, C. turbinatum, C. caespitosum.

Cystiphyllum Siluriense, Lons. (Cyathophyllum vesiculosum, Goldf.).

Strombodes plicatum, Ehr. (Cyathophyllum plicatum, Goldf.).

Limaria clathrata, L. fruticosa.

42 Species.

III. Table of genera common to both limestones, but with distinct species in each.

In Wenlock limestone.

CRUSTACEA—11.

Calymene Blumenbachii, C. ?
Downingiæ, C. tuberculata, C. variolaris, C. ? punctata.

Asaphus caudatus, A. tuberculato-caudatus, A. flabellifer, A. Stokesii.

Paradoxides 2-mucronatus.

——— 4-mucronatus.

MOLLUSCA.

Heteropoda 2. *Bellerophon* dilatatus, B. Wenlockiensis.

Cephalopoda 6. *Orthoceras* Brighii, O. eccentricum, O. fimbriatum, O. canaliculatum, O. trochlearis.

Conularia quadrisulcata.

Gasteropoda 5. *Euomphalus* sculptus, E. discors, E. rugosus, E. funatus.

Patella? implicata.

CONCHIFERA 18.

Pentamerus Knightii.

Terebratulæ borealis, T. nucula, T. crispata, T. imbricata, T. cuneata, T. bidentata, T. deflexa, T. Stricklandii.

In Eifel limestone.

CRUSTACEA—9.

Calymene lævigata, C. arachnoides, C. Schlotheimii, C. latifrons.

Asaphus Hausmanni, A. Bucephalus, A. armatus.

Paradoxides macrocephalus.

——— flabellifer.

MOLLUSCA.

Heteropoda 3. *Bellerophon* undulatus, B. apertus, B. striatus.

Cephalopoda 4. *Orthoceras* giganteum, O. nodulosum, O. inflatum.

Conularia teres.

Gasteropoda 8. *Euomphalus* nodosus, E. radiatus, E. striatus, E. articulatus, E. depressus, E. delphinuloides, E. trigonalis.

Patella Neptuni.

CONCHIFERA 46. *Pentamerus* Aylesfordii.

Terebratulæ concentrica, T. heterotypa, T. triloba, T. lateralis, T. crumena, T. canaliculata, T. quinquelatera, T. dichotoma,

In Wenlock limestone.

CONCHIFERA (continued).

Orthis rustica, *O. hybrida*, *O. filosa*, *O. canalis*.

Spirifera? *sinuata*.

Atrypa didyma, *A. obovata*, *A. tenuistriata*, *A. compressa*.

Leptaena 0.

CRINOIDEA 8.

Cyathocrinites tuberculatus, *C. goniodactylus*, *C. capillaris*, *C. pyriformis*.

Actinocrinites simplex, *A.*? *arthriticus*, *A.*? *expansus*, *A.*? *retarius*.

POLYPARIA 13.

Aulopora conglomerata, *A. con-*
similis.

Fenestella *Milleri*, *F. reticulata*.

Discopora squamata.

Retepora infundibulum.

Gorgonia assimilis.

Stromatopora nummulitisimilis.

Favosites multipora.

Porites tubulata, *P. expatista*, *P. discoidea*.

Cyathophyllum 0.

Custiphyllum cylindricum.

SEDIS INCERTÆ 1.

Tentaculites ornatus.

64 Species.

In Eifel limestone.

CONCHIFERA (continued).

T. pentagona, *T. Wahlenbergii*,
T. acuminata, *T. diodonta*, *T.*
subglobosa, *T. bifida*, *T. cla-*
vata, *T. amygdala*.

Orthis Pecten, *O. testudinaria*, *O.*
radiata, *O. fasciculata*, *O. no-*
dosa.

Spirifera cuspidata, *S. cyrtæna*, *S.*
pinguis, *S. microptera*, *S. hete-*
roclyta, *S. macroptera*, *S. cep-*
toptera, *S. minima*, *S. attenuata*,
S. curvata, *S. striatula*, *S. glabra*
et obtusa, *S. oblata*, *S. cana-*
lifera (*Terebratula aperturata*,
Schlot.).

Atrypa cassidea.

Leptaena convoluta, *L. furcata*,
L. capillata, *L. pectinata*, *L.*
minuta, *L. scabricula*, *L. ru-*
gosa, *L. Scotica*, *L. lepis*.

CRINOIDEA 6.

Cyathocrinites geometricus.

Actinocrinites triacontadactylus,
A. cingulatus, *A. muricatus*, *A.*
nodulosus, *A. lævis*.

POLYPARIA 18.

Aulopora spicata, *A. sarmentacea*.

Fenestella 0.

Discopora 0.

Retepora antiqua.

Gorgonia infundibuliformis.

Stromatopora polymorpha.

Favosites infundibuliformis.

Porites 0.

Cyathophyllum radicans, *C. hypo-*
crateriforme, *C. ceratites*, *C.*
flexuosum, *C. vermiculare*, *C.*
secundum, *C. lamellosum*, *C.*
placentiforme, *C. quadrigemi-*
num, *C. hexagonum*, *C. helian-*
thoides, *C. pentagonum*.

Cystiphyllum 0.

SEDIS INCERTÆ 2.

Tentaculites scalaris, *T. annulatus*.

96 Species.

IV. Table of genera and species distinct in both limestones.

In Wenlock limestone.

CRUSTACEA 3.

Homalonotus delphinocephalus.

Acidaspis Brightii.

Bumastus Barriensis.

ANNELIDA 1. *Spirorbis tenuis*.

MOLLUSCA.

Cephalopoda 3. *Nautilus undosus*
(*Clymenia*? Münster).

Lituites giganteus, L.? Biddulphii.

Gasteropoda 2. *Nerita spirata*, N.? *Haliotis*.

CONCHIFERA 0.

In Eifel limestone.

CRUSTACEA 0.

ANNELIDA 3. *Serpula ammonia*, *S. omphalodes*, *S. socialis*.

MOLLUSCA.

Cephalopoda 12. *Cyrtoceras depressum*, *C. compressum*, *C. annulatum*, *C. lineatum*.

Spirula nodosa, *S. costata*, *S. annulata*, *S. carinata*, *S. dorsata*.

Goniatites subnautilus, *G. multiseptatus*, *G. orbiculus*.

Gasteropoda 14. *Turbo armatus*, *T. nodosus*, *T. cælatus*.

Trochus exaltatus.

Pileopsis prisca, *P. compressa*.

Sigaretus? *rugosus*.

Rotella helicinaeformis.

Phasianella ventricosa, *P. buccinoides*, *P. fusiformis*.

Turritella bilineata, *T. striata*, *T. obsoleta*.

CONCHIFERA 23. *Gypidia gryphoides*. (*Terebratula gryphus*, Schlot.)

Strigocephalus Burtini (*Terebratula rostrata*, Schlot.), *S. striatus*.

Calceola sandalina.

Aptychus lævigatus.

Pecten Neptuni.

Pterinea radiata, *P. elegans**.

Modiola Goldfussii.

Cardium alæforme, *C. elongatum*.

Lucina Proavia, *L. lineata*, *L. rugosa*.

Cyprina minuta.

Sanguinolaria concentrica, *S. lamellosa*, *S. dorsata*, *S. truncata*, *S. phaseolina*.

* M. Beyrich remarks that *Pterineæ* are numerous, and in a manner characteristic of the greywacke and slate rocks which support the Eifel limestone. Besides the two species noticed above, Professor Goldfuss enumerates the following as occurring in the greywacke and slate countries adjacent to the Rhine, namely near Ems, *Pterinea ventricosa*, *P. costata*, *P. lineata*, *P. plana*, *P. trigona*, *P. lævis*, *P. elongata*; in Siegen (and the Harz) *P. lamellosa*; near Iserlohn, *P. reticulata*; and at Paffendorf (Coblenz) *P. carinata*. This last species occurs also, with *P. bicarinata*, at Lindlar, in the Berg territory. Of these fourteen species thus found in transition tracts, two only have been observed in the carboniferous limestone, viz., *Pterinea elegans* in the Ratingen limestone, and *P. carinata* near Lewistown adjoining Niagara in North America.

In Wenlock limestone.

CONCHIFERA (continued).

CRINOIDEA 4.

- Marsupiocrinites cælatus.*
Hypanthocrinites decorus.
Dimerocrinites decadactylus, D.
icosidactylus.

POLYPARIA 17.

- Escharina?* *angularis.*
Philodictya lanceolata.
Hornera crassa.
Berenicea irregularis.
Eschara? *scalpellum.*
Blumenbachium globosum.
Millepora repens.
Syringopora reticulata, S. *bifur-*
cata, S. *filiformis?* S. *cæspi-*
tosa?
Monticularia conferta.
Acervularia Baltica.
Cladocora sulcata.
Turbinolopsis bina.
Verticillopora? *abnormis.*
Cnemidium tenue.

SEDIS INCERTÆ 1.

- Cornulites serpularius.*

31 Species.

In Eifel limestone.

CONCHIFERA (continued).

- Pholadomya radiata.*
Isocardia Humboldtii, I. *antiqua.*

CRINOIDEA 13.

- Eugeniocrinites mespiliformis.*
Pentacrinites priscus.
Platycrinites ventricosus.
Melocrinites gibbosus.
Rhodocrinites verus, R. *giratus*, R.
quinquepartitus, R. *canalicu-*
latus, R. *crenatus.*
Cupressocrinites crassus, C. *graci-*
lis, C. *tesseratus.*
Eucalyptocrinites rosaceus.

POLYPARIA 6.

- Anthophyllum bicostatum.*
Achilleum cariosum.
Manon cribrorum.
Scyphia cornu-copiæ.
Coscinopora placenta.
Lithodendron bicostatum.

SEDIS INCERTÆ 0.

71 Species.

From the preceding tables it results that we have

<i>In the Wenlock limestone.</i>		<i>In the Eifel limestone.</i>	
Common species	42	Common species	42
Common genera but distinct species	64	Common genera but distinct species	96
Distinct genera with distinct species	31	Distinct genera with distinct species	71
Distinct species.....	— 95	Distinct species	— 167
Wenlock—total species..		Eifel—total species.....	
137		209	

From the foregoing review, and the resulting numbers, it becomes clearly apparent, that M. Dumont was not justified in pronouncing the Wenlock and Eifel limestones as of the same formation. It is, however, also evident that they are akin in a certain degree, the species which are common to both limestones being to the total number in each, in the Wenlock limestone as 4 to 13 nearly, that is, forming about

one third part; and in the Eifel limestone as 4 to 20, or one fifth part. Again, in the Wenlock limestone, the distinct species are to the common species as 9 to 4, or $2\frac{1}{4}$ to 1; and in the Eifel limestone as 16 to 4, or 4 to 1.

M. Beyrich has made the remark, that the greywacke and slate country which supports the Eifel and Belgian lower limestone, contains for the greater part the same fossils as are found in this limestone; and he adds, that among them occur a considerable number of species which are common to the carboniferous limestone, and in this remark he is joined by M. Dumont, as already observed: that this view is correct, the following table which I have drawn up will sufficiently prove.

V. Table of fossils common to the Eifel limestone and the carboniferous limestone—47 species.

Localities in carboniferous limestone.

CRUSTACEA.

Calymene macrophthalma, *Al. Brong.* Richelle, Liège.

MOLLUSCA.

Heteropoda—

Bellerophon undulatus, *Goldf.* Chimay, Schwelm.
B. apertus, *Sow.* Richelle, Bristol, Fermanagh.
B. striatus, *Goldf.* Chimay, Ratingen.

Cephalopoda—

Orthoceras annulatum, *Sow.* King's County.
O. giganteum, *Sow.* Closeburn.

CONCHIFERA.

Terebratula pugnus and *lateralis*, *Sow.* Ratingen, Derbyshire, Dublin.
T. crumena, *Sow.* Derbyshire.
T. lacunosa (*T. Wilsonii*, *Sow.*)
Dalm. Liège.
T. Mantiæ, *Sow.* Ireland.
T. acuminata, *Sow.* Ratingen, Yorkshire, Derbysh.
T. diodonta, *Dalm.* Ratingen.
T. amygdala, *Goldf.* Visé.
T. sacculus, *Sow.* Derbyshire, Rutherglen.
*Orthis testudinaria**, *Dalm.* Cromford in Westphalia.
Spirifera cuspidata (*S. elevata*, *Dalm.*)
Sow. Ratingen, Bristol, Derbyshire.
S. octoplicata (*S. crispa*, *Dalm.*),
Sow. Derbyshire, Castlemaine in
Kerry.
S. oblata, *Sow.* Richelle, Visé, Dublin.
S. glabra et obtusa (*Terebratula*
lævigata, *Schlot.*), *Sow.* Richelle, Ratingen, Derbyshire.
S. pinguis (*S. lævicosta*, *Goldf.*),
Sow. Liège, Dublin.
S. attenuata, *Sow.* Liège, Dublin, Clonmell.
S. striatula, *Goldf.* Ratingen.
S. canalifera, *Goldf.* Ratingen, Visé, Dublin.

* See Professor Bronn in *Lethæa Geognostica*, p. 82. This is, I believe, the only instance on record of an *Orthis* having been found in the carboniferous limestone.

CONCHIFERA (*continued*).*Localities in carboniferous limestone.*

Leptæna (Producta) depressa (Strophomena marsupita, <i>DeFr.</i>), <i>Sow.</i>	Richelle, Fermanagh.
L. Scotica, <i>Sow.</i>	Richelle, Chokier, Castlemaine.
L. hemisphærica, <i>Sow.</i>	Richelle, Chokier, Ratingen, Buttevant.
L. longispina, <i>Sow.</i>	Richelle, Linlithgow.
L. sarcinulata, <i>Sow.</i>	Richelle, Visé.
L. scabricula, <i>Sow.</i>	Visé, Liège, Dublin.
L. rugosa (Strophomena, <i>Rafin.</i>), <i>Dalm.</i>	Chokier.
Pterinea elegans, <i>Goldf.</i>	Ratingen.
Cardium elongatum, <i>Sow.</i>	Ratingen, Derbyshire.
C. alæforme, <i>Sow.</i>	Ratingen, Queen's County.
Sanguinolaria concentrica, <i>Phillips.</i> ..	Tour Mountain, County Cork.
CRINOIDEA.	
Actinocrinites 30-dactylus, <i>Mill.</i>	Mendip, Yorkshire.
A. lævis, <i>Mill.</i>	Ratingen.
Rhodocrinites verus, <i>Mill.</i>	Mendip, Bristol.
POLYPARIA.	
Gorgonia infundibuliformis, <i>Goldf.</i> ...	Arnsberg.
G. antiqua (Fenestella antiqua, <i>Lons.</i>), <i>Goldf.</i>	Arnsberg.
Cyathophyllum flexuosum, <i>Goldf.</i>	Limerick.
C. pentagonum, <i>Goldf.</i>	Namur.
C. cæspitosum, <i>Goldf.</i>	Chokier, Seilles.
C. turbinatum, <i>Goldf.</i>	Richelle.
C. quadrigeminum, <i>Goldf.</i>	Berneau.
Astrea ananas, <i>de Blainville</i> (Cyathophyllum ananas, <i>Goldf.</i>)	Namur.
Favosites polymorpha, <i>Goldf.</i>	Namur, Elberfeld.
F. fibrosa, <i>Goldf.</i>	Buffalo on Niagara River.

It thus appearing that there are 47 species in the Eifel limestone which are common to the carboniferous limestone, it follows that they constitute about *two ninths* of the whole number found in the Eifel, the ratio being as 47 to 209.

On the other hand, we have seen that the Eifel limestone contains 42 species which are common to the Wenlock limestone, or *one fifth* of its whole number, the ratio being as 42 to 209.

Hence, if we take these numbers as a guide, it may be said that the Eifel limestone is in a slight degree more nearly allied to the carboniferous limestone than it is to the Wenlock, the difference lying between the ratios of 2 to 9, and 2 to 10.

From the numbers which have been given it also results, that the Wenlock limestone contains 95 species which are distinct from those of the Eifel; and the Eifel limestone 167 species distinct from those of the Wenlock limestone; of which 47 species being also common to the carboniferous limestone, it follows that 120 species may be said to be distinctive of the Eifel, in contradistinction to the Wenlock and carboniferous limestones.

This view leads naturally to the inference, that in the order of superposition the Eifel limestone occupies a middle rank, namely, one higher in the series than the Wenlock, but lower than the carboniferous limestone. This result would appear to correspond with the notions of M. Beyrich, who, in speculating on the relative age which should be assigned to the Rhenish slate mountains among the transition formations, is disposed to consider them as of a later origin than those strata in the north of Europe, namely in Scandinavia and Russia, in which the *Orthis* tribe are so abundant. These latter may doubtless be placed, to a certain extent, in parallel with the Silurian.

If the views which I have taken be correct, in which the formations of the Eifel, and the lower formations in Belgium are, in the order of succession, considered as antecedent to the old red sandstone of Belgium (meaning such as in Britain has been usually designated by that name), it becomes doubly desirable that the distinguished geologists who have taken Devon and Cornwall in hand should complete their investigations. I have been led to anticipate that a considerable degree of analogy subsists between the two regions, and, if it be true, it may yet be proved that the older stratified rocks of those counties are not only of later origin than the Silurian formations (which would so far correspond with the views of Mr. Lonsdale, Professor Sedgwick, and Mr. Murchison), but of a date also anterior to that of the old red sandstone formation, taken in the common acceptation of that term. Whether the latter may yet be detected in the southern parts of Devon in unconformed position, may still be a fit subject for inquiry.

It is remarkable that among the fossils by which the Devonshire and Cornish formations are distinguished, Mr. Austen should state a number to exist as common to the carboniferous limestone, so nearly agreeing with the number of a corresponding character in the Eifel, namely, as 40 and 47. An account of the 40 species indicated by Mr. Austen, it is to be hoped, may be published, and it will be interesting to compare them with the list that I have given from the Eifel. What ratio that number may bear to the total number of species in the older stratified rocks of Devon and Cornwall remains yet to be seen, as well as what number of species may be common to the Silurian, what number may be similar to the Eifel fossils, or to those of the older stratified rocks and their included limestone bands in the south of Ireland, and what number may be more peculiarly distinctive of Devon and Cornwall. These are questions which can only be an-

swered by extended researches; and until these be completed, it may be prudent to defer the assignment of the older stratified formations of Devon and Cornwall to a precise period in respect of relative age.

In the limestone bands of the south of Ireland I have shown that 50 species of fossils have been determined with precision, that is, including *Leptæna depressa* and *Leptæna lata*; but others also occur which have not been determined, e. g., *Avicula* and *Goniatites*. Of the fifty species, *six* are peculiar; *twenty-six* common to other transition tracts at home and abroad (of which *twelve* occur in the Eifel); while *forty-three* of the species are common to the carboniferous limestone also*; thus nearly agreeing with the numbers of a corresponding character found in Devon and Cornwall, and in the Eifel. But as in the south of Ireland, neither the bands of limestone, nor the strata with which they are directly associated, nor yet those situated more north which lie deeper in the series, have hitherto undergone that rigid examination which is requisite with respect to the organic remains which they may contain, we are not authorized as yet to pronounce upon the precise proportions in which those fossils may exist relatively to each other in the general series. Yet enough has already been elicited to prove that those limestone bands stand in uninterrupted connexion with strata in which fossils of a more decided transition character are by no means wanting; and no sufficient evidence has yet been produced to invalidate the conclusion that the whole constitute together one consecutive series, notwithstanding we may perceive some distinctions between the fossils of the schistose and greywacke strata and those of the included bands of limestone. Continued researches will doubtless throw further light on this subject.

A pertinent observation of Professor Sedgwick may here be aptly introduced: "There are two elements of classification applicable to stratified rocks of all ages, namely, physical structure and order of superposition; one giving the mineralogical unity of a group of rocks, the other their relative age. In addition to the two former, are classifications founded on the organic remains in the several groups.....As, however, the (so-called) laws respecting the distribution of organic

* See Memoir on the South of Ireland in Geological Transactions, vol. v., second series, and Lond. and Edin. Phil. Mag. for August 1839. When the former work was finished in 1835, I considered the *Isocardia oblonga* as peculiar to the Cork band of limestone; but Professor Phillips's Illustrations of Yorkshire, part ii., published in 1836, have shown that the *Isocardia oblonga* occurs also in carboniferous limestone in Yorkshire, and in the counties of Kildare and Dublin.

types are mere general results founded on actual observation, it is obvious that they can never upset conclusions drawn from the clear and unambiguous evidence of sections. The two methods may be used independently, and conspire to the same end, but in their nature cannot come into permanent collision*." In the present case it should be borne in mind that the consecutive series of the older stratified rocks of the south of Ireland is *unconformably* overlaid in the northern parts of Kerry, Cork, and Waterford, directly either by the *true* old red sandstone formation of British geologists, or by the carboniferous limestone, or the coal formation †.

I might now extend the comparison by entering into the countries adjacent to the right bank of the Rhine, or into the Fichtelgebirge; but in neither of them is the information hitherto obtained of so extensive and detailed a character as to admit of precise conclusions. To what was previously known respecting the former tracts, M. Beyrich has made considerable additions in his *Beiträge*; and from these contributions it may be collected, that the greater part of the Nassau limestones near Dillenburg, &c., as well as those of Bensberg, Refrath, Paffrath, &c., adjoining the Rhine, together with the greywacke and slaty rocks in which they are intercalated, or on which they simply rest, exhibit in general the same organic remains as the Eifel, although they possess also species and even genera not hitherto found in the latter; e. g., at Paffrath the genera *Nerita*, *Megalodon*, *Cardita*, *Monodonta*, *Buccinum*; and in Nassau in the Wissenbach clayslate *Parmophorus*; and in the limestone on the Lahn near Villemar an *Ostrea*, a genus not previously noticed in any transition country. Of the Nassau limestones it is remarked that they differ chiefly from the Eifel limestone by being for the greater part interstratified with the greywacke and slate rocks, while the limestones of the Eifel are merely superimposed upon the latter in troughs. In Belgium, however, the same, namely, the lower limestone of that country, forms an interstratified portion of the general series. Considering then the fossils which have been noticed in these districts, we have here again an exemplification of affinities and differences in the organic types of their respective strata. Should M. Beyrich complete the work which he proposed to himself in the year 1837, of drawing up an exact critical catalogue of

* Proc. of Geol. Soc., vol. ii., p. 675, May 1838, [or Lond. and Edinb. Phil. Mag. vol. xiii., p. 299.

† See my Geological Map of the South of Ireland, in Geol. Trans. vol. v., second series.

all the fossils which occur in the Rhenish slate mountains, such a work could not fail to prove of high interest to every geologist, and its appearance is much to be desired.

From what is already known of the transition tracts of the Fichtelgebirge and of Bohemia in the environs of Prague, it may be inferred that the same doctrine of affinities and differences applies there also. It is understood that the late Count Münster was engaged in a work from which great light might be expected to be thrown on the more ancient formations. As his valuable collections have passed into the possession of the University of Cambridge, we may well anticipate that his extensive labours will not be lost to science.

If the genus *Orthis* be considered, as it appears generally to be, indicative of the older transition formations, it must also be admitted that the genera *Terebratula*, *Spirifera*, and *Leptæna* are of equal antiquity.

In fine, even this brief exposition may serve to confirm the opinion which I have formerly expressed, namely, that in widely extended, or distantly separated, lands of transition or protozoic origin, the relations though akin may not be exactly alike; in other words, that resemblances may exist, but diversities prevail in the details of different tracts, both with respect to the composition and disposition of the mineral masses and the distribution of the remains of organized bodies. Whether the transition formations may ultimately be separated into definite consecutive groups, is a question which can only be satisfactorily determined by the results of extended comparative inquiries.

[To be continued.]

LX. *On the Course of the Electrical Discharge, and on the Effects of Lightning on certain Ships of the British Navy, &c. &c.* By Mr. SNOW HARRIS, Esq., F.R.S.

[Continued from p. 128.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

24. **I**N the instance I last quoted of damage to H.M.S. Rodney by lightning, it will be remembered that there was no regular metallic line through which the forces in action could become neutralized. The electrical agency had therefore to find for itself such a general course, as upon the whole opposed the least resistance to its progress; and it is

evident that in this case its path was determined on the general principles before laid down in sec. 17, p. 123.

25. I shall now proceed to state a few cases of damage to certain other ships of the navy, where metallic bodies happened to be so disposed about the rigging and hull, as to approximate in some measure to the conditions of experiment 2, sec. 17, and consequently to that perfect state of defence against the expansive force of the electrical discharge in which a ship would become placed, by perfecting the conducting power of the masts, and uniting them into one general continuous system with the metallic masses in the hull, and with the sea.

These cases are particularly interesting, and conclusive of the general question of the protection to be afforded by such a system.

No. 1.—In September 1833, H.M. ship *Hyacinth* had both the fore and main-top masts and top-gallant masts destroyed by lightning in the Indian Ocean. The electric fluid shivered these masts from the truck to the heel of the top-mast, as indicated by the waving black line *a b* in the annexed diagram, fig. 1, which represents the effects on the main mast; at the point *b*, it became assisted by the chain topsail sheet leading to the deck at *c*, and so did no further damage to the mast; at *d* it received further assistance from the copper pipe of Hearle's patent pump, leading to a small well at *e*, and thence by a second pipe through the ship's side under water, and by this passed safely into the sea*.

26. Now it is evident here that a heavy discharge of lightning which shivered completely a sloop of war's main-top mast and top-gallant mast varying from 11 inches to a foot in diameter through a length of at least 80 feet, was conducted without damage or fusion by an iron chain and a short copper pipe. It is therefore important to state the dimensions of these metallic bodies. Now the iron chain consisted of links $2\frac{1}{4}$ inches long, made of iron rod $\frac{1}{2}$ inch in diameter. It reached from the lower yard to the deck, a distance of about 50 feet.

The pump consisted of copper pipe 4 pounds to the square foot; it was 3 inches in diameter, and about the $\frac{1}{11}$ th of an inch thick, extending through a distance of about 10 feet.

The effects on the foremast were very similar, they are omitted therefore for the sake of brevity.

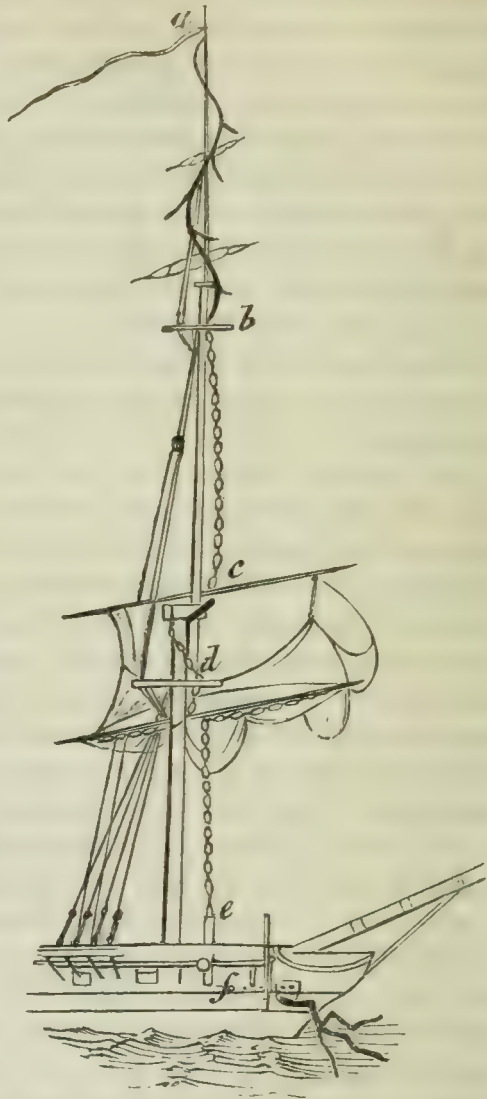
27. It is not a little remarkable, that five years after this, in 1838, this same ship was again struck by lightning, whilst at

* These circumstances are minutely detailed by Capt. Blackwood, who commanded the ship at the time, and may be seen in his interesting letter on the subject, in the *Nautical Magazine*, vol. viii., p. 116.

Fig. 1.

*Hyacinth.*

Fig. 2.

*Athol.*

anchor in Penang Bay, and again lost her main-top mast and top-gallant mast in a similar way, the lower mast being preserved by her chain topsail sheets.

28. No. 2. In 1830, the *Athol*, of 28 guns, was struck by lightning on her foremast, in the Bight of Biafra : at this time the topsails were lowered on the caps and the other sails furled, as showed in the annexed diagram, fig. 2. This ship had chains for hoisting the topsails which lay in the direction of the topmast as indicated by the dotted line *b c*. She had also a chain for topsail sheets, which led along the lower masts as indicated by the line *d e*. When the electrical explosion

fell on the truck it shivered the top-gallant mast in pieces so far as the commencement of the chain at *b*; here being assisted by the chain, it passed on *without* any damage to the topmast, which is extremely worthy of remark, because in the former case, where there was no chain, the top-mast was destroyed.

Having reached the point *c*, where the chain terminated, it passed *with* damage over the head of the mast, until again being assisted by the lower chain *d e*, it passed *without* damage to the deck; on reaching the deck at *e*, it passed by means of a bolt through a beam in the forecastle upon the chain cable, and thence into the sea*.

29. These effects are similar to the former, and show the protection afforded by the chains, and their power of conducting heavy discharges of lightning without any of the ill consequences insisted on by Mr. Sturgeon; since in both cases the chains were in the vicinity of large metallic masses, viz. the iron hoops, iron-bound blocks, &c. about the masts, and in both cases the lightning passed through the hull. Now as all the laws of nature are general, not partial, it is reasonable to infer, that if Mr. Sturgeon's view of a lateral explosion were true, it ought to apply in such palpable cases as these, more especially when he says he can produce a lateral explosion at 50 feet distance with a jar of only "a quart capacity."

30. No. 3. The effects of lightning on H.M.S. Snake, is another striking instance of the general laws we have been contending for. The phænomena are detailed with peculiar clearness by Capt. Milne in the March number of the Nautical Magazine. The electric fluid entered main truck, shivered royal mast, splintered top-gallant mast; then over *chain* main topsail tye *without* damage to within 8 feet of the deck so far as the topsail halliards.

Finding, as observed by Captain Milne, an obstruction here in the ropes, it again seized on the mast, and became divided at the saddle of main boom; one portion passed out of quarter-deck port to the sea, the other to lower deck and down the mast, and distributed itself over the hull, affecting persons below. The mast, on being examined at Halifax, was sprung about the partners two inches deep and 15 inches round, and was *perfectly burst asunder at the step*: hence the shock had extended to the heel, the electric matter, consequently, must have passed by the metallic bolts in the keelson to the sea.

* An interesting and authentic account of this circumstance will be found in the Nautical Magazine, vol. viii., p. 114.

It is further stated, and it is *a most important fact*, that a seaman aloft on the cross trees, at the time, did not experience any sensation whatever.

31. No. 4. The Buzzard brigantine was struck by lightning on the Coast of Africa in February 1838, and lost her top-gallant and topmast, under precisely the same circumstances as those of the Hyacinth, the lower mast being preserved by the chain topsail sheet*.

32. No. 5. The Fox revenue cutter was struck by lightning in March 1818. The mast was furrowed and otherwise damaged in every part *except where it was coppered*; as appears by a minute made at the time by the master mast-maker at the Plymouth dock-yard.

Now the copper usually placed about a cutter's mast is not the $\frac{1}{32}$ nd part of an inch in thickness. In this case it remained perfect.

33. No. 6. The spire of a church at Kingsbridge in Devonshire was struck by lightning in June 1828, and fearfully damaged. This case is particularly worthy of notice.

The lightning fell on an iron spill, *a*, *b*, supporting the weather-cock, about 7 feet in length and 1 inch in diameter. On this it produced no visible effect, nor did *any damage arise to the stone-work about the rod*. It was not until the rod ceased at the point *b* that the masonry was rent†.

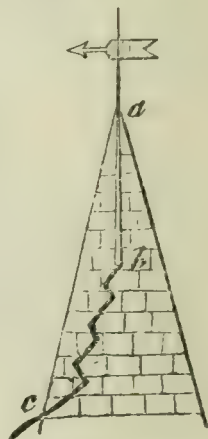
34. No. 7. Extract from a letter from Lieut. Sullivan, of H.M.S. "Beagle," addressed to the Editor of the Annals of Electricity, &c., &c., relative to the protection afforded by a continuous conductor attached to the mast of H.M.S. Beagle.

"Having considered your communication in the Annals of Electricity on marine lightning conductors, containing observations on the stroke of lightning which fell on the masts of H.M.S. Beagle, I think it fair, both to Mr. Harris and the naval service, to describe the phænomena I witnessed on that occasion; first stating, that at the time of my joining the Beagle in 1831, previously to her leaving England, I had no acquaintance with Mr. Harris, and certainly *no bias* in favour of the conductors with which the ship was fitted. I may therefore claim to be considered an impartial observer.

* This case was given me by the commander Lieut. Fox. I was myself on board the vessel on her arrival. The particulars are noted in her log.

† MS. letter with a drawing, dated July 11, 1828, from the Rev. G. F. Wise, late Vicar of Kingsbridge.

Fig. 3.



“ At the time alluded to, I was first Lieutenant of the *Beagle*, and was attending to the duty on deck. She was at anchor off Monte Video, in the Rio de la Plata, a part of the world very often visited by severe lightning storms. Having been on board H.M. ship *Thetis* at Rio Janeiro a few years before, when her *foremast was entirely destroyed by lightning*, my attention was always particularly directed to approaching electric storms, and especially on the occasion alluded to, as the storm was unusually severe. The flashes succeeded each other in rapid succession, and were gradually approaching; and I was watching aloft for them when the ship was apparently wrapt in a blaze of fire, accompanied by a *simultaneous* crash, which was equal if not superior to the shock I felt in the *Thetis*; one of the clouds by which we were enveloped had evidently burst upon the vessel, and as the mainmast appeared for the instant to be in a mass of fire, I felt certain that the lightning had passed down the conductor on that mast; the vessel was shaken by the shock, and an unusual tremulous motion could be distinctly felt. As soon as I had recovered from the surprise of the moment, I ran down below to state what I saw, and to see if the conductors below had been affected; and just as I entered the gun-room, the purser, Mr. Rowlett, ran out of his cabin, (along the beam of which a main branch of the conductor passed) and said that he was sure the lightning had passed down the conductor, for at the moment of the shock he heard a sound like rushing water passing along the beam. Not the slightest ill consequence was experienced; and I cannot refrain from expressing my conviction, that had it not been for the conductor the results would have been of very serious moment.

“ This was not the only instance where we consider that the vessel had been saved from being damaged by lightning by Mr. Harris’s conductors; and I believe that in saying I had the most perfect confidence in the protection which those conductors afforded us, I express the opinion of every officer and man in the ship.

“ Not being sufficiently acquainted with electrical experiments, I cannot remark upon those you have adduced in support of your opinions detrimental to Mr. Harris’s conductors.

“ I can, therefore, only repeat my conviction that the *Beagle* was struck by lightning in the usual way, and certainly without any *lateral* explosion or other ill effects similar to those you insist on in your *Annals of Electricity*.”

35. Now these facts are totally subversive of all Mr. Sturgeon has advanced concerning his destructive lateral explosion

in the way of objection to the fixing conductors in ships' masts, and prove in the most conclusive manner the protecting power of such conductors: his statement, therefore, that "destructive lateral discharges will always take place when the vicinal bodies are capacious and near the primitive conductor or to any of its metallic appendages," is clearly fallacious.

36. It is allowed by writers on inductive science, that we wander from the true path of philosophical inquiry, and take up that of assumption and conjecture, directly we cease to verify our principles by an appeal to facts. In order to arrive at a general law of nature, it is requisite to examine carefully a great number of facts bearing directly on the question at issue, and show, that the principle we assume is common to them all; for if in any case the assumed principle is decidedly negatived, it is at least a powerful exception; and it *may* be sufficient to overturn our whole theory.

If such exceptions are numerous, any theory which cannot include them is decidedly untenable.

It has been well observed by Abercrombie* that in deducing a general principle, "when the deduction is made from a full examination of *all* the individual cases, and the general fact shown to apply to them all, this is truth; when it is deduced from a small number of observations and extended to others to which it *does not apply*, this is falsehood."

37. In applying these principles, we find Mr. Sturgeon's assumed lateral explosion decidedly negatived in all the cases just cited, since we do not find any such occur in the passage of heavy discharges of lightning along the masts, &c.; we do not find, as asserted by him, anything like electrical waves produced by the discharge through a conductor situated close to the magazine. Thus in the case of the *Hyacinth*, No. 1. the copper pump, *d e*, fig. 1, was a conductor near the after magazine. Yet the electric shock, in passing down this and through the ship's side, did not cause "intense sparks among the powder barrels, whose metallic linings and hoops reciprocally interchange them †."

38. Again, we do not find in the passage of a dense explosion of lightning that the sailors are necessarily subjected to lateral discharge, since in the case of the *Snake*, it may be observed that a seaman aloft on the cross-trees did not experience *any sensation* whatever, although the top-gallant mast was shivered, and a terrific shock darted from the heel of it to the chain topsail tye. Now if Mr. Sturgeon's views were

* On the Intellectual Powers.

† Sturgeon's Memoir.

practically sound, this man ought to have been killed on the spot by a "*lateral discharge*," as he says happened to a seaman called Wilson in the case of the Rodney.

39. Mr. Sturgeon, therefore, if he still adheres to his theory, is at last reduced to the necessity of supposing, that his lateral discharge may sometimes occur, and sometimes not, which is manifestly in the teeth of his own hypothesis. This instance just quoted of the little effect experienced by persons in the vicinity of heavy electrical discharges is by no means a solitary one, as the following extract from a letter from Admiral Hawker, with which he favoured me relative to the damage done to the Mignomne, very fully shows:—

"The circumstances of the Mignomne being struck by lightning were these: she had been on shore, and was going to Port Royal, Jamaica, attended by the *Désirée*; we had a day I think the hottest I ever experienced in the W. Indies, without a cloud. After sunset we observed clouds rising up from every part of the horizon with thunder and lightning. I ordered the topsails to be lowered in case of squalls, and we ran down towards Port Royal: about midnight the heavens seemed to be one continued flame, and soon after the main top-mast was shattered into probably fifty pieces, scattering the splinters in all directions; the mainmast was split down to the keelson, and a sulphurous smell came up from the hold, which occasioned some to cry out that the ship was on fire. Two men were killed in the main-top, being burnt black, and having some splinters sticking in them, and a man who was sleeping on the lower deck with his head on a bag (for the ship having been on the rocks for three days there were no hammocks) near the armourer's bench was found dead, with one black speck in his side; *another man sleeping by him was not hurt.*"

40. The number of instances in which dense explosions of lightning have passed very near to persons without causing any serious injury to them is remarkable.

Thus in the case of the Buzzard, No. 4, before mentioned; the explosion at the time of shivering the top-mast passed so near to a seaman called Robert Purk, that it actually tore the shirt from his arm: he very kindly showed me the shirt, and pointed out the place where he was standing. Lieut. Fox, who commanded this vessel, and who was good enough to send me an account of the damage, &c. sustained, says, in allusion to this circumstance, "The lightning took a strip out of the shirt about two inches wide from the shoulder to the wrist without hurting him."

No. 9.—In the instance of the Hawk cutter, lately struck

by lightning on the west coast of Erris, and seriously damaged, it appears that the electric matter in passing down the main hatchway passed between a man and a boy. Neither were hurt; the latter experienced a shock only. It also passed close to another man lying across a hammock about the same spot, who jumped up and thought his neck handkerchief was on fire; the latter experienced a temporary effect only in his right arm.

41. All these cases evidently show, that no damage occurs from a shock of lightning *out of its direct path*. It may, however, divide in the absence of any good conducting course, and branch out into a variety of other courses (as already observed) and seize either wholly or partially upon bodies which happen to lie in certain points, as clearly shown in all these cases, and in the partial fusion of the leaf-gold given in experiment 2, p. 124, of my last communication.

We may also expect to find an *expansive* effect of greater or less force in the vicinity of a discharge of *free electricity* under the form of a dense spark, in a *bad conducting interval*; as observed by Dr. Priestley, "the air being suddenly displaced gives a concussion to all the bodies which happen to be near it."

42. It is clear therefore that in all cases where injury or death has occurred, as in those before given in the Mignonne, Rodney, &c., it has been the result of the passage of the electric agency, either wholly or partially, through the animal body, and not from the result of any *lateral explosion* of electricity, such as described by Mr. Sturgeon. If, as he says, such explosions in all cases of proximity to the primitive charge necessarily arise, such proximity to the passage of a dense shock of lightning would be in all cases fatal, which is evidently not the case.

43. I have now to consider briefly a few instances of the power of metallic bodies to transmit heavy discharges of lightning.

In the case above quoted of the Hyacinth, we observe, as already remarked, that a flash of lightning which shivered the top-mast and top-gallant mast passed over a small iron chain and copper tube without fusing either. A similar result ensued in the second instance of the Hyacinth being struck by lightning; also in the case of the Athol and Buzzard, and Snake, and in a great variety of others too numerous to detail here.

In the case of the Fox, No. 5, it is seen that the shock of lightning which damaged the mast, was conducted without fusion or damage by sheet copper of $\frac{1}{2}$ nd of an in ch in

thickness placed in the wake of the gaff. This is conclusive of the fallacy of Mr. Sturgeon's assertion, that any conductor applied to the mast, would, under the operation of lightning, be "probably peeled from the wood."

In the case of the Kingsbridge spire, No. 6. The lightning which shivered the tower, fell on a cylindrical iron rod of an inch diameter without producing any effect on it.

In the case of the Rodney, the flash which set the top on fire and splintered the masts, was conducted by a short copper funnel for top-gallant rigging without fusion.

In the case of the Beagle, No. 7, a shock of lightning passed down the conductors without producing any effect on them.

No. 10. A house was struck at Tenterden; the lightning fell on an iron bar three-quarters of an inch square, but produced no effect on it.*

No. 11. A stroke of lightning fell on Mr. West's house at Philadelphia, having a conductor terminating in a brass rod ten inches long and *a quarter of an inch in diameter*; only a few inches of the point were melted, but no damage occurred to the building†.

No. 12. On the 19th of April 1827, one of the large New York packets, whilst in the Gulph Stream, was assailed by two most awful strokes of lightning twice in the same day. The first shock was productive of serious and destructive effects. The second shock fell on a pointed conductor subsequently hoisted to the main-mast head. This conductor consisted of an iron chain having links of a quarter of an inch thick and two feet in length and turned into hooks at each end, connected by rings of the same thickness, and one inch annular diameter. This conductor was *attached* to an iron rod placed at the mast head, half an inch thick and four feet long. The explosion fell in a *concentrated* form, and with an awful crash upon this rod. Although the small chain below was disjointed and some of the links fused, yet this pointed iron rod was only fused for a few inches. *The ship in the second case escaped danger.*

Now these are authenticated cases, and there are numerous others which I might adduce, to show how perfectly *capacious* and *continuous* conductors transmit shocks of lightning.

44. No good instance can be adduced in which conductors of great capacity have been even moderately heated by lightning. I do not admit Mr. Sturgeon's "on dit" respecting the conductor passing through the Nelson Monument in Edinburgh. It is really no evidence whatever on a scientific ques-

* Philosophical Transactions.

† Ibid.

tion. “*It is said* (observes Mr. Sturgeon) that the lightning rod passing through the Nelson Monument became so hot by lightning that it could not be touched by the hand by *the first person* who visited it afterwards. Allowing a few minutes to have elapsed between the flash and the person entering the monument, the probability would be that the conductor had been made red-hot.” This is of the same character with all Mr. Sturgeon’s data; it is generally surmise, the *show* without the reality; it just amounts to nothing.

45. I am aware that it has been also *supposed* that the great conductors of St. Paul’s church were heated by lightning, but it is only a *supposition*. The conductors were not examined before the lightning, which was said to have fallen on them, occurred, so that we cannot be certain that the observed appearances were not originally present after the forging of them; it is besides very unlikely, that a stroke of lightning should have fallen on this building, capable of rendering bars of iron, six inches wide and one inch and a half thick, red-hot, without destroying the thin copper covering the ball and cross on the dome of the building, and without the crash of the thunder having been heard over the whole city, no mention of which is made; when St. Bride’s steeple was struck, the latter was peculiarly remarkable.

46. There is another instance on record, of the effects of lightning on an iron rod, in Port Royal, Jamaica, mentioned in the Transactions of the Royal Society, the evidence of which seems very incomplete. Two men are said to have perished by lightning near a church wall: that is not improbable: but, on subsequently looking inside the wall, a bar of iron an inch thick, and a foot in length, was found in many places wasted away to the size of a fine wire. Now it does not appear that this bar was examined *previously* to the occurrence of the lightning; hence we cannot infer that the wasting was produced by the electric fluid; more especially as similar appearances are not uncommon in bars of iron erected in churchyards in this country, and which have evidently resulted from oxidation and time.

47. Seeing then how much evidence we have from actual experience of the protective effect of regular conductors of the *worst* kind, and their power of transmitting dense explosions of lightning, we may reasonably infer that a conductor of copper equal to a rod of an inch diameter, and *extending the whole length of the mast*, would be proof against any discharge of lightning ever experienced, as, I think, is shown by the cases in which ships fitted with my conductors have been struck by shocks of lightning without damage.

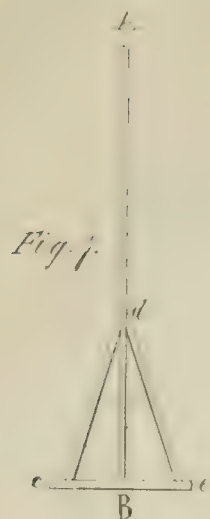
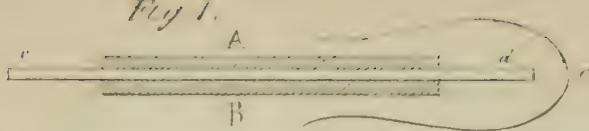


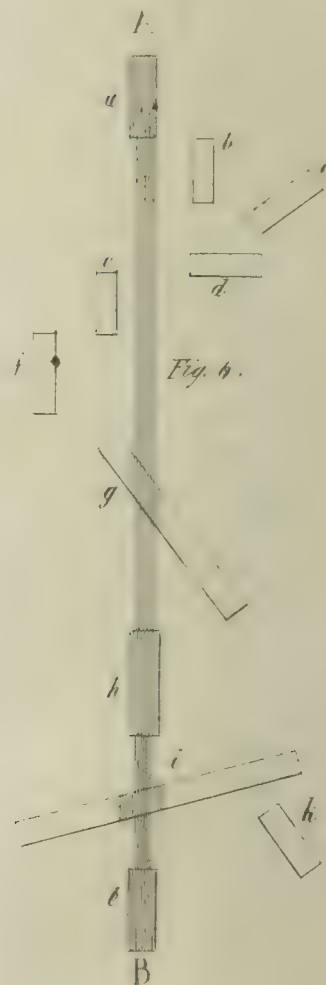
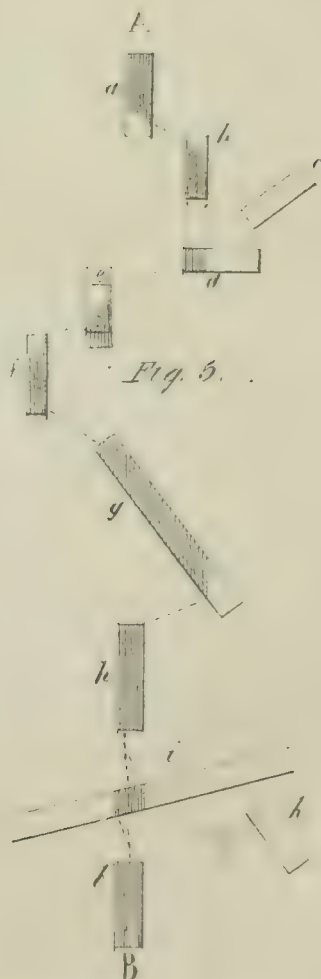
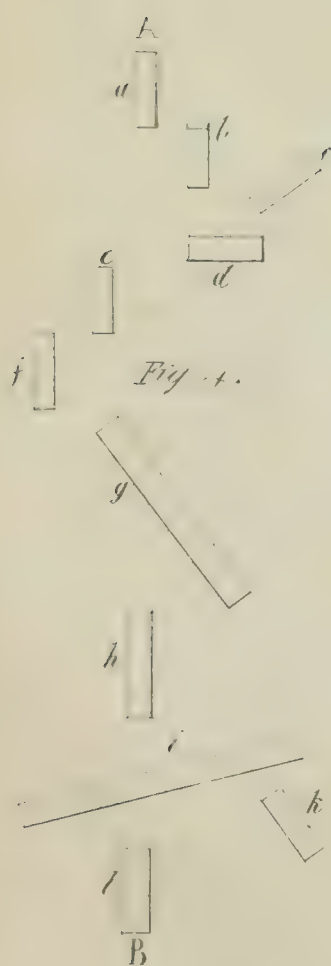
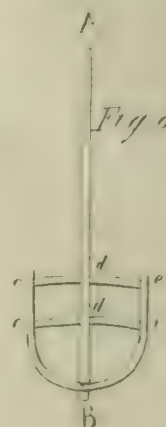
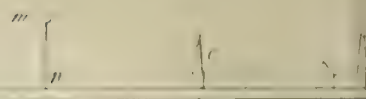
Fig. 3.

m



B

h



48. Exceptions, however, have been taken by Mr. Sturgeon to the phænomena described by the officers who either commanded or were in the ships. Thus Captain Turner, in describing the shock of lightning which fell on the *Dryad* frigate on the coast of Africa, says, that “he saw the lightning on the conductor on the fore-mast, and saw it during another flash run down the mizen-mast; that all the men there heard a loud whizzing noise.” Captain Fitzroy and Lieut. Sullivan also mention similar phænomena. Now the exceptions taken are these, viz. that no noise is ever produced by electricity entering a conductor, and that we cannot produce a “running light” upon a conductor carrying an electrical charge.

These exceptions, however, are rather captious objections to forms of expression, than to the facts themselves; it is easy to show from experience that luminous appearances are often attendant on discharges of both natural and artificial electricity.

Thus in the case of the *Hawk* (No. 9.) the account states that “the vessel was apparently enveloped in a flame of lightning;” whilst in the case of the *Beagle*, Lieut. Sullivan says, “on looking aloft the ship was apparently in a blaze of fire.” In the case of the *Snake* (No. 3.) the electric fluid is said to have *descended* with an instantaneous explosion of a vivid purple colour.

When H.M. ship *Norge* was struck by lightning in Port Royal harbour, the electric fluid was observed (to use Admiral Rodd’s expression) to “absolutely stream down a conductor attached to the mast of H.M. ship *Warrior*,” close by.

Such phænomena are besides remarkably close to the results of experiments: thus a heavy shock of electricity, passed over a metallic wire in a partially exhausted receiver, will exhibit a transiently passing light on its surface.

49. The whizzing noise is quite in accordance with common electrical effects. It invariably occurs when a good conductor receives and disarms an explosion by a pointed extremity. Mr. Sturgeon, however, asserts that “no such noise is ever produced by the *fluid entering* a metallic conductor.” This is mere sophistry; let any one attempt to discharge a highly charged battery by an acutely pointed conductor. A great part of the charge will immediately rush through or towards the point with a whizzing noise. Now the stratum of cloud may be either positively or negatively electrified, and whether the one or the other, it is clear that the rush of electricity from a charged surface toward a point, or from a point towards an

undercharged surface (according to Franklin's hypothesis) will be always attended by a whizzing noise.

50. The protection which continuous conductors would afford if well and efficiently applied to ships is, I think, apparent in all the preceding cases, and when we consider that the masts are themselves conductors of electricity, and that by their position alone they determine the course of the discharge into the body of the hull, it becomes the more requisite to affix to them good conductors, which quickly disperse and reduce the electrical action to a state of quiescence.

We have I think fair evidence of this in the trials hitherto made with the continuous fixed conductors applied to certain ships of the British navy.

51. These ships have been exposed more or less in all points of the world. Lightning has not fallen upon them *oftener* than other vessels not so fitted; and *when it has done so no damage* has arisen in any way, or has any destructive lateral effect, such as that contended for by Mr. Sturgeon, taken place. His comparison, therefore, of the effects of lightning on the Rodney with the "*probable effects*" (as he terms it) on my conductors, although he can find no instance of such *probable effects*, is therefore purely hypothetical. If Mr. Sturgeon has no good authenticated fact to oppose to the mass of evidence I have adduced, of what avail is any hypothetical or loose opinion he may find it *convenient* to advance?

52. Before concluding this communication, I cannot refrain from pointing out the apparent inconsistencies of his views on this point. Having described my conductors as dangerous and objectionable in every possible way, as calculated to induce oblique flashes of lightning to strike the ship to the destruction of the sailors' lives, the sails, rigging, &c. &c., he says, sect. 221, on discovering that he could not conveniently apply his own rods above the top-mast head, "*as however every chance of danger to the men and every species of damage to the vessel ought strictly to be avoided, it still appears desirable to furnish the top-gallant rigging with conductors; and perhaps those which would give the least trouble to the men, would be strips of copper let into grooves in the masts according to the plan proposed by Mr. Harris.*" Now, I think, it must be clear to any one, that if my system be so objectionable as he would have it believed, on the grounds above stated, it must be equally objectionable on the top-gallant masts; the lives of the sailors are just as much exposed there as at any other point, perhaps more so. Mr. Sturgeon himself admits that two men were killed there in

the case of the Rodney. But by his admission above-quoted, my method is not objectionable in the top-gallant mast, but is on the contrary calculated to avoid "every species of danger to the vessel and every chance of danger to the men;" if so, it must be equally efficient on the top-mast, lower mast, &c. This sort of traverse sailing, to use a nautical phrase, is not a little amusing, and is, I believe, quite unprecedented in any paper on science.

53. In order that no mistake may arise in respect of what I have advanced relating to lateral explosions, I may in conclusion simply state, that I do not deny the expansive force of a dense electrical explosion, and its destructive effect on *imperfect* and *non-conductors*. I do not deny its effect in causing expansion in the surrounding air, which I rather choose to call with Priestley, "the lateral force of electrical explosions," than a *lateral explosion of electricity*. I do not deny this in the absence of any regular system of conductors, or that the discharge may divide in several directions, and in distributing itself over the hull, may cause dense sparks and other electrical appearances in various parts of the vessel, but which would not appear, if a perfect system of conduction, such as I have proposed, were resorted to.

I do however deny the probability of any lateral discharge of electric matter from conducting bodies transmitting an accumulation between oppositely charged surfaces, as assumed by several persons imperfectly acquainted with ordinary electrical action, and lately by Mr. Sturgeon; and, I maintain, that neither artificially, nor in the course of nature, can any instance of such lateral explosion be authenticated.

I am, Gentlemen, yours, &c.

Plymouth, March 14, 1840.

W. SNOW HARRIS.

P.S. It has been insisted on by Mr. Sturgeon, that a shock of lightning, descending a continuous conductor on the mast, would magnetize every chronometer in the cabin, &c.—(Memoir, Sect. 207.)

This assumption is completely negatived by the cases above quoted. In fig. 1, an awful discharge descended an iron chain, and yet no magnetic effect was observable on the neighbouring compasses, or on the chronometer in the cabin. It is only in the *absence* of continuous conductors we find such magnetic effects, and even then their occurrence is comparatively rare. Really, Mr. Sturgeon makes so many random assertions, it is almost impossible to attend to them all.

LXI. *Remarks on the Compounds derived from the Stearopten of Oil of Peppermint.* By ROBERT KANE, M.D., M.R.I.A.*

IN my paper on the constitution of the essential oils, I noticed that the composition of the oil of peppermint as determined by my analyses approximated to that announced by Blanchet and Sell as belonging to the solid crystalline substance which often forms in it; but at the same time I stated, that from the utter discordance of Blanchet's results among themselves, no confidence could be placed in them. The formula I adopted for oil of peppermint is $C_{21}H_{20}O_2$, and in order to establish a more direct comparison I will subjoin two of the analyses by which that formula was established.

	Experiment.		Theory.	
	I.	II.		
Carbon.....	78·06	77·81	C_{21}	128·9 78·14
Hydrogen ..	12·32	12·11	H_{20}	20·0 12·12
Oxygen ...	9·62	10·08	O_2	16·0 9·74
	<hr/>	<hr/>		
	100·00	100·00		164·9 100·00

The two analyses of the stearopten made by Sell and Blanchet, gave results which I will also subjoin.

	Experiment.		Theory.	
	I.	II.		
Carbon.....	79·63	77·27	C_{10}	61·40 77·28
Hydrogen ..	11·25	12·96	H_{10}	10·00 12·59
Oxygen ...	9·12	9·77	O	8·00 10·12
				<hr/>
				79·40 100·00

The total discordance of these results, coupled with the fact, that on analysing the liquid oil of peppermint Blanchet and Sell had obtained numbers quite different from each other, and from the truth, shows that for purposes of further research the investigations of Blanchet and Sell cannot be taken as a standard.

Mr. Walter has recently published a memoir on the crystallized essence (stearopten) of peppermint, in which he lays down as the basis of his very interesting researches Blanchet's formula, and supposes that its truth is confirmed by his own analyses. With this I should not, however, have anything to do, had not Mr. Walter made the same observation with me, of how close my formula for the liquid oil approached to Blanchet's for the stearopten, and insinuated

* Communicated by the Author.

indeed pretty broadly, although without having analysed the liquid oil, which he might so readily have done, that the formula of Blanchet is the true one, and that mine, in as far as it differs from it, is likely to be incorrect. His words are: "Si l'essence de menthe poivrée liquide présente la même composition que l'essence de menthe concrète, ce qui est très probable d'après les observations de M. Robert Kane, qui est conduit cependant, quoique le rapport numérique qu'il a trouvé, soit le même, à adopter une formule différent de celle que je viens de présenter, formule du reste qui n'est déduite ni de la densité de la vapeur, ni d'aucune combinaison dans laquelle l'essence de menthe rentrerait," &c. Now I purpose to show in this notice that Mr. Walter has not done justice to his own analyses; for that by giving them their just interpretation, he would have found Blanchet's formula to be inexact, and that the stearopten possesses the precise constitution which I have assigned for the oil.

In an organic analysis the sources of error act in opposite directions on the carbon and on the hydrogen; there cannot be more carbon obtained than was present in the substance; and from the facility with which the hydrogen is burned, there is never less water obtained. In practice, a chemist very seldom succeeds in a complete combustion of the carbon; and almost in all cases, from the hygrometric nature of his materials, he gets more water than he ought. Hence in deducing a formula from a set of analyses, the highest number obtained for the carbon, and the lowest number obtained for the hydrogen, are those most likely to be true, and are those on which the formula should be constructed. There may be cases in which water may be lost, but it must arise either from an error in weighing or from bad management in the analysis. Setting out from these principles I will proceed to discuss Mr. Walter's numerical results.

In six analyses of the solid oil, the highest value he obtained for the carbon was 77.68, and the mean of five results was 77.36. The lowest result for hydrogen was 12.52 and the mean of four was 12.66. Mr. Walter's analyses and theory are therefore,—

	Analysis.	Analysis.		Theory.
Carbon	77.68	77.36	C ₂₀	77.27
Hydrogen ..	12.52	12.66	H ₂₀	12.62
Oxygen	9.80	9.98	O	10.11
	<hr/> 100.00	<hr/> 100.00		<hr/> 100.00

It is also quite evident that the results of Mr. Walter's analyses cannot be considered as exceedingly correct, when

we find that out of his six analyses, the carbonic acid was lost in one, and the water in two instances; probably from a too rapid and imperfect process of combustion. Mr. Walter's formula is therefore disproved by his own analyses, which give too much carbon and too little hydrogen; but they agree very well with the formula I proposed, if we allow his analyses to have been of the average degree of excellence.

The specific gravity of the vapour of the stearopten furnishes no test whereby to distinguish between the formulæ; $C_{20}H_{20}O_2$ gives 5.455, and $C_{21}H_{20}O_2$ gives 5.666. He obtained 5.62. It is true, the experimental density generally comes out a little higher than the calculated density; but on the other hand, with these oils there is almost universally a trace of decomposition by which the experimental density is thrown too low. Hence the density found agrees with one formula as well as the other.

Mr. Walter has given the name of menthene to a hydrocarbon, produced by the action of dry phosphoric acid on the stearopten. To this body he assigns the formula $C_{20}H_{18}$: his results are,

	I.	II.	III.	Theory.
Carbon	87.74	87.53	87.59	87.18
Hydrogen...	12.99	12.85	12.71	12.82
	<hr/>	<hr/>	<hr/>	<hr/>
	100.73	100.38	100.30	100.00

In every case Mr. Walter obtained an excess of weight which must be an error in the hydrogen, and in every case the hydrogen increased by this error comes only to about equal the hydrogen required by his formula. In every case also he obtained too much carbon, and this with a substance whose perfect combustion must evidently to every organic chemist be most difficult. His formula cannot be true: but the formula $C_{21}H_{18}$ gives the following numbers:—

C_{21}	=	128.9	87.76
H_{18}	=	18.0	12.24
		<hr/>	<hr/>
		146.9	100.00

coinciding perfectly with the analyses.

The density of the vapour of menthene Mr. Walter found to be 4.95. The formula $C_{20}H_{18}$ gives 4.835. The formula $C_{21}H_{18}$ gives 5.046. The difference here also is so very trifling that no stress can be laid upon it one way or the other.

The results obtained by the action of sulphuric acid, not being definite, do not require notice, further than to mention

that the formula I announced for the sulphodadylate of lime, was $\text{SO}_3 \cdot \text{Ca O} + \text{C}_{20} \text{H}_{16}$, and not the formula which Mr. Walter attributes to me.

The substance obtained by Mr. Walter in acting on the stearopten with perchloride of phosphorus is of great importance, as giving some evidence of how far this essential oil acts as an alcohol. The action appears to be very complicated, and hence it is to the analysis alone that we can look for explanation. Taking for the best analysis the highest carbon and the lowest hydrogen, the results of Mr. Walter are,

	Best analysis.	Mean of six.
Carbon	70.55	70.01
Hydrogen...	9.89	10.31

The chlorine, determined once, gave 20.87.

The most natural formula is to suppose that, as with alcohol or acetone, water is eliminated and decomposed, a sort of muriatic æther being produced. Hence the formula should be $\text{C}_{20} \text{H}_{19} \text{Cl}$ or $\text{C}_{21} \text{H}_{19} \text{Cl}$. The numbers are,

C_{20}	69.26	C_{21}	70.32
H_{19}	10.72	H_{19}	10.36
Cl	20.02	Cl	19.31
<hr/>			<hr/>	
100.00			100.00	

Here with the C_{20} of Blanchet, there is again too much carbon and too little hydrogen, but with the C_{21} as on my view, the numbers are more nearly true. But Mr. Walter suggests that the hydrogen may be 18 atoms in place of 19, and then the formulæ give

C_{20}	69.6	C_{21}	70.71
H_{18}	10.3	H_{18}	9.87
Cl	20.1	Cl	19.42
<hr/>		<hr/>	
100.0		100.00	

Thus we have still on Blanchet's basis too little carbon and too much hydrogen in the formula. On the basis of C_{21} the formula becomes much more likely to be true; at all events the third formula suggested by Mr. Walter $\text{C}_{20} \text{H}_{17} \text{Cl}$ is quite unnecessary. I consider this body to be chloride of menthene.

Although we should find similar examples in the products formed by the action of chlorine on the stearopten, yet I will not enter into any discussion relating to them, as from the analytical results, and other circumstances, it is evident that the final and definite products of that action have not yet been obtained. In Mr. Walter's formulæ there come into

play half equivalents of chlorine and hydrogen, indicating that the substances analysed were not yet definitely characterized.

The peculiar acid body formed by treating the essence with nitric acid has its origin in decompositions so complex, that, until its atomic weight and composition shall have been accurately determined by the analysis of its salts, it is totally useless to discuss it in relation to the present question. But here as in the other instances Mr. Walter has got too much carbon, although not too little hydrogen, and the additional quantity of carbon given by my number makes his analytical results more consonant to the theory. I do not wish, however, to be considered as applying the formula $C_{21}H_{20}O_2$ to explain the origin of this body, which indeed I consider to belong to a totally different series.

In concluding, I must observe that I admire very much the general exactness of Mr. Walter's results, and the skill which he has displayed in this and other difficult investigations, by which he has been so highly distinguished. In fact, it is greatly to his credit that his analyses were so good, although he had been beguiled by the authority of Blanchet to adopt an insecure basis at his outset; and I have written these remarks not to diminish Mr. Walter's merit, which none can be more ready to express the highest sense of, but to show that all Mr. Walter's investigations have but confirmed my former results, and that they have fully proved, that the liquid and the solid oil of peppermint have the same constitution, and that although I had not confirmed my formula by the accessory methods generally employed, the confirmations have come unconsciously from Mr. Walter's hands, and that the formula $C_{21}H_{20}O_2$ is that which alone accounts for his interesting results.

LXII. *On the Galvanic Properties of the Elementary Bodies, and on the Amalgamation of Zinc.* By ALFRED SMEE, Esq.

[In continuation of a former paper, p. 315.]

THE first non-metallic element we have to examine is carbon.

When a diamond is placed in contact with amalgamated zinc in dilute sulphuric acid, no gas is given off, nor copper precipitated on it from a solution of that metal when touched by zinc. Gas coke, however, recently ignited, or plumbago, placed under similar circumstances, copiously evolves hydrogen from its surface. The same circumstance is noticed with the various forms of porous coke and boxwood charcoal, but

in these cases no gas is given off for some little time. Observing this, it was a matter of great interest to know what became of the gas for the first few seconds, and it directly occurred that the first portions of gas were bound down in a nascent state with the charcoal: this was proved by placing it in a solution of sulphate of copper, when the charcoal and the coke became coated with a thin film of the metal. In the same way gold, silver, mercury, and lead were precipitated from their solutions, and iodine set free from iodic acid. Probably the other metals were also precipitated, but their colours render a thin film difficult to be distinguished. When charcoal or the porous coke is made to form the electrodes of a battery, the piece forming the kathode or platinode is found to have similar properties; but the anode or zincode, however, is found to possess nascent oxygen from its liberating chlorine from muriatic acid, though this is not quite so satisfactory as the experiment with the hydrogen. The gas coke and plumbago are found not to possess the property of retaining the gases. Occasionally charcoal will be found to precipitate gold and silver from their solutions, but in these cases copper, and those metals which have a greater affinity for oxygen, are not reduced.

View the importance of these experiments, as they demonstratively prove that which has hitherto been the prevailing theory, namely, that nascent hydrogen precipitates the metals, and that the precipitation may take place when the galvanic current is broken; for the coke will retain its hydrogen in some cases for forty-eight or more hours. Now in what state is the hydrogen when it has these properties? Is it in the form of minute bubbles adhering to the surface? This would appear to be a mystery. It is probably in a state analogous to solution; for if a piece of smooth platinum be placed in contact with zinc till minute bubbles are covering its whole surface, and then the zinc be removed and a solution of a metal be poured upon the platinum in such a way that the bubbles are not disturbed, no precipitation takes place; and even spongy platinum or spongy palladium fails under the same circumstances to precipitate the metal.

Much difficulty arises in naming the two poles of a battery; they are called the positive end and the negative end, the anode and the kathode, the platinode and zincode; now as each pole of a simple battery becomes reversed if the battery is doubled, it is better to name the two ends from the oxygen and hydrogen; since we have shown that the galvanic current owes its power of decomposing many substances entirely to these gases. The names which are proposed are the ox-

ode, at which oxygen is evolved, and the hydrogode, where the hydrogen is given off.

The soft and spongy charcoals, as those of deal, possess the property of evolving gas very imperfectly.

Various kinds of coal, such as anthracite and cannel, were tried, but none were found to evolve hydrogen, nor to have copper precipitated when the circuit was made in a solution of that metal.

From the above experiments we see that batteries may be constructed of carbon in the place of a negative metal; the hard coke or plumbago answering best, and the porous coke and box-wood charcoal next*. These may be used as an ordinary battery with sulphuric acid, but of course a battery thus constructed possesses but little power. If, however, the hydrogen is removed upon Professor Daniell's principle, then will the power be increased, and a charcoal battery may be made of surprising energy. The hydrogen may be removed by metallic solutions which have a feeble affinity for oxygen, and therefore those of gold, silver, platinum, or copper would answer best; the latter being the only one in use from its cheapness. The highly oxygenated acids, such as nitric, &c., are more powerful than these, and are now considerably employed, but disadvantages attend their action; for if the current is required to be continued for a long time, a large quantity of acid must be used, and the fumes arising from the battery are injurious to the animal œconomy: in addition, the strong acid is liable to be spilt over the fingers or clothes; and lastly, it always transudes through the porous tubes and acts upon the zinc, even when amalgamated, to a considerable extent.

It is perhaps worthy of notice, that the powers of the nitric acid battery are not to be attributed to the fluids alone, for no current is formed when platinum is used in both cells. Strong sulphuric acid produced scarcely any action, but the addition of nitric acid rendered it powerful, for a time proportionate to the quantity of the latter acid used. I have tried other substances which have an affinity for hydrogen, such as chlorine, iodine, chloride of lime, peroxide of iron (or a mixture of muriatic acid and peroxide of manganese), so that nascent chlorine may be evolved during the action of the battery; but I find that even with the latter, the action, though powerful, is one quarter less than with strong nitric acid.

A coke battery of two cells, with eight ounces of nitric

* The coke may be cut with a saw into any convenient shape, whilst plumbago, though softer, has the singular property of wearing down every tooth from the instrument in a very short time.

acid and dilute sulphuric acid, yields ten cubic inches of gas in five minutes. In this case about eight square inches and a half of carbon were exposed, and the communication was effected by means of thick platinum wires. The same quantity of gas was driven off from seven square inches of platinum. One piece of charcoal in a single cell gave one fifth of a cubic inch in twenty minutes.

Experiments were performed on the properties of selenium, sulphur, phosphorus, bromine, iodine, and chlorine; but as nothing very worthy of notice was discovered, it will be unnecessary to dwell upon these substances.

The rationale of amalgamating zinc would appear to be exactly the converse of platinizing platinum or the other metals, for the one favours the adhesion of the hydrogen and prevents thereby local action, the other favours the escape of the hydrogen by its non-adhesion, and in that way increases the power. The reasons which I have to offer in support of these opinions are the following:—

When zinc dissolved in mercury is placed in dilute sulphuric acid no action takes place, because the gas cannot be readily evolved, but coats the whole surface; but that there is action really produced, is shown by adding nitrate of silver or sulphate of copper, when the nascent hydrogen is evidenced by the reduction of these metals; as soon as the whole of the metal is reduced it is again inactive, although the elements of a powerful current are there, namely, zinc, silver, acid; now touch the mercury by a piece of silver or the negative metal, the gas will be immediately evolved from it. This explanation appears to me to account for every phænomenon connected with the remarkable properties of amalgamated zinc, which is further confirmed by the attempt to make a galvanic battery with zinc and mercury, the junction being effected exterior to the acid solution, as here great adhesion will be seen to exist between the hydrogen and the mercury.

In conclusion, I have to regret that the continuity of the paper has been necessarily interrupted, but it is a matter of no great consequence, as it treats of many rather dissimilar properties.

Bank of England, April 10, 1840.

LXIII. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron. By Dr. C. SCHAFHAEUTL, of Munich.*

[Continued from p. 304.]

TO illustrate this and to arrive at a nearer insight into the chemical composition of iron, I shall select three specimens of cast iron, the one of English, the other two of French production.

(A) Iron from the Maesteg iron-works near Neath in South Wales. Colour white, brilliant and granulated; on the lowest part of the pig appearances of contraction; yielding in some degree to the stroke of the hammer and being very difficult to break into fragments. I am given to understand it is produced from clay iron-stone mixed with some Cornish ore, probably red oxide of iron, and by means of a hot blast. Specific gravity 7·407.

(B) Iron from Vienne in France, département de l'Isere; produced from the ore of La Voulte, (that is red oxide of iron,) mixed with about one-third part of pea-iron-ore. The coke is from Rive de Gier*.

The hot blast was also used here. Colour gray, of great dark graphite-like brilliancy; the scaly appearance very much developed; easy to be broken into small fragments, with a species of tough resistance, making a similar sound as pure tin when beaten, but still difficult to reduce to very fine powder; specific gravity 6·898.

(C) Iron of Creuzot, département de Saone et Loire; produced from bog iron ore, with a mixture of brown iron ore of varying qualities and some puddling slag. The cold blast was applied. The fracture dead grayish; earthy; hardness considerable; brittle, not yielding to the hammer; specific gravity 7·378.

Thirty-five grains of iron, (A) and (B), in small fragments were put into two retorts, and four ounces of hydrochloric acid of 1·16 specific gravity poured over them; the beak of the retort was connected with two Woulfe's bottles, filled with a neutral solution of acetate of lead, temperature 62° Fahr.

* The coke from Rive de Gier contains, according to Berthier,

75·00	Carbon.
03·50	Volatile matter.
21·50	Ashes (alumina).

100·000

containing likewise 0·300 pyrites. As soon as this fuel is changed for purer coke, gray iron cannot be produced there.

The acid had scarcely been poured over the iron (B), when the whole powdered iron rose, under a rather violent evolution of gas, to the top of the acid. A short time after the acid became of a yellow colour, and the caseoid or cheese-like whitish foam increased each moment on the top of the acid during the evolution of hydrogen, and filling almost the whole of the upper part of the retort.

In Woulfe's bottles distinct glittering scales of sulphuret of lead were rapidly deposited; the gas escaping out of the last bottle had, in an extremely slight degree, in smell the character of hydrogen developed by means of acids from iron, but no longer affected a solution of nitrate or acetate of lead. The liquid was found next day opake, of a light gray colour, still developing gas.

The evolution from iron (A) had ceased long before. The iron (A) or white Welsh iron formed likewise a caseoid or cheese-like substance on the top of the acid, but its colour was dark gray approaching to black, and the liquid below was also of an opake dark gray.

The sulphuret of lead formed in the Woulfe's bottle was not scaly, like that from the gray iron, but resembling a dark-brown greasy viscid mass, making the whole liquid turbid, and only settling two days afterwards. The mass in the retort was then dark gray, with a somewhat lighter sediment.

The residuum in the retort of the gray French iron (B) cast upon a filter, washed without interruption with boiling hot distilled water, was of a soap-like greasy form, and had after being dried a grayish-white flowery appearance. As often as I poured fresh water on the filter, the already collapsed mass began to swell like a sponge, and almost filled the whole filter. This residuum, dried at a temperature of 212° Fahr., weighed 5.53 grains, and had an extremely light appearance resembling silica, chemically separated from minerals.

On 2.1 grains of powder were poured in a test-tube 5 drachms of concentrated caustic ammonia, the test-tube shut with a perforated cork, which contained as usual an S-like bent glass tube; a violent evolution of gas in extremely minute bubbles took place, which ceased only twenty-four hours afterwards. The powder lay on the bottom of the test tube in lenticular aggregates of a gray colour; the evolved gas collected over water measured at 65° Fahr. and 29.35 height of barometer, 0.605 cubic inches, corrected for water 0.586 cubic inches. It was, except small traces of oxygen and azote from the remaining air in the test tube, *pure hydrogen*.

The contents of the test glass poured on a filter were carefully washed with ammonia, and the liquid afterward evaporated to dryness in a platinum crucible. This dry remainder was perfectly white; only in the corner of the crucible some brown matter was collected, in all probability a species of humine. This dry residuum weighed 0.3 grains, was after ignition perfectly white, and had lost 0.08 grains. It dissolved in hydrochloric acid, left silica behind, which I was unable to weigh, and carbonate of ammonia threw down alumina mixed with some silica.

In consequence of this, 5.53 grains would have contained aluminum equal to 0.298793 grains. To account for 0.0188 hydrogen we ought to have 0.440893 aluminum. By actual analysis I found 0.352583; but it might very well have happened that, in separating alumina from phosphoric acid, a part remained combined with the acid.

The gray residuum which had been treated with ammonia, dried and weighed, was 1.82, and had therefore lost 0.28 grains, that is to say 0.02 grains less than the weight of alumina obtained. This extraordinary evolution of hydrogen takes place with all residuums of hot and cold-blast gray iron, but with the former more than the latter; and perfectly white iron never evolves any gas whatever; yet white iron which is nearly approaching to gray iron gives always traces of hydrogen.

What causes this extraordinary decomposition of water? We know no other chemical body which, left as a residuum after being treated with acids, possesses the power of decomposing water by the presence of ammonia, except aluminum; and as the ammonia had really dissolved alumina, which only occurs when it comes in contact with the metallic base of alumina, we may safely conclude, that the extraordinary evolution of hydrogen was here produced by the presence of metallic aluminum.

Solution of caustic potash ley likewise decomposes the gray residuums, but only at a higher degree of temperature, and then a species of slight explosion takes place and the fluid is thrown with some violence out of the crucible.

Viewed through a microscope, the residuum of this gray cast iron appears to be composed of white gelatinous transparent nodules, which generally surround a centre, consisting of some dull black spots and of a scale, sometimes shining like graphite, but of a more silvery whiteness. The mixture of these scales with the white nodules gives the powder, to the naked eye, the appearance of having a grayish colour; these scales remain after being treated with ammonia, though not at all apparently

diminished, having only lost somewhat of their brightness; the black spots sometimes disappear entirely.

1.72 grains of the remaining gray powder, separated from the filter and ignited, lost 0.203, and seemed in appearance to have undergone no alteration except having become a little more bulky and flocky.

Hydrochloric acid extracted oxide of iron, 0.036.

1.32 grains of this residuum deprived of oxide of iron was mixed with five times its weight of carbonate of soda and ignited: after cooling, the lid of the crucible was found covered with green drops of manganate of soda, and on the bottom was the fluxed mass crystallized, perfectly white on the borders, but having in the middle a dirty yellowish spot.

I separated in the common way

Silica	0.963
Iron and Manganese	0.076
Loss (of Carbon)	0.281
	<hr/>
	1.320

The same result was obtained, when, instead of melting the remaining gray powder with alkali, it was boiled in solution of caustic potash and strongly concentrated ley; the silicon is taken up by the potash and iron, and manganese, mixed with some carbonaceous matter, is left in blackish-green bulky flocks on the filter, oxidizing very rapidly.

35 grains of the same metal analysed in the common way gave

Silicon	1.702530 =	4.86430
Aluminum	0.352583 =	1.00738
Manganese	0.262960 =	0.75130
Phosphorus	0.189000 =	0.54000
Sulphur	0.062110 =	0.17740
Carbon	1.183000 =	3.38000
Iron.	31.152607 =	89.00740
Loss	00.095210 =	0.27222
	<hr/>	<hr/>
	35.000000	100.00000

The white transparent nodules are undoubtedly silica; before the blow-pipe they melt with soda into a transparent globule, separating at the same time black scales of graphite; melted with microcosmic salt, a skeleton of silica is separated as usual. After subtracting silica, nothing remains but a small quantity of iron and manganese, and the loss as already shown is = 0.281, which amounts for 35 grains of iron to 1.17; the actual quantity of carbon was, as we see, 1.183, and therefore we may safely assume the loss to be carbon. The graphite scales, freed by means of solution of caustic soda from

the gelatinous silica, and analysed like graphite, as stated in the beginning of this treatise, proved to be of the same composition as the graphite B.

I must here observe, that in fragments broken from the outside of the same pig-metal traces only of sulphur could be discovered; that the malleable iron produced from this according to my puddling process was extremely soft, and had the peculiar property of welding so easily, that tin-plates rolled from it adhered so firmly together by heating them a little too much, that it became impossible to separate them; but the *great quantity* of silicon contained in this iron was extremely destructive to the bottom and sides of the *puddling* furnace.

Let us now return to the residuum of iron (B.); white Welsh iron. This residuum was found to weigh 6·77 grains, and had a dark brown colour and a very strong unpleasant smell, peculiar to hydrogen obtained in this manner from cast iron. It was powerfully attracted by the magnet, and traces only of aluminum could be found.

3·13 grains of the dried residuum, heated cautiously in a weighed platinum crucible, began to glow around the periphery before the crucible became red-hot, and the ignition spread from thence very slowly towards the centre. The platinum crucible was now removed from the fire, covered with its lid and cooled near concentrated sulphuric acid under a bell glass. Its weight had increased 0·08 grains, and its colour changed from brown to black. On the crucible being again placed over the lamp and kept in a red heat for ten minutes, its black colour changed into a dirty light red, and its weight increased equal to 0·27

On a third repetition of this ignition 0·02

On a fourth 0·05

On a fifth. 0·00

0·42

The whole increase of 6·77 grains would therefore be 0·9084.

It was still as powerfully affected by the magnet as previously to ignition. A part of this powder, 3·55 grains, heated with hydrochloric acid of 1·16 sp. gr., in the same crucible, a grayish powder remained, which presented a distinct interspersion of dull black with white spots, and weighed 0·290; after ignition it had gained 0·028 and became perfectly white.

It consisted entirely of silica, for which we reckon 0·1528 silicon. As the black spots disappeared and it gained 0·028

grains in weight during ignition, we may conclude, that the black spots consisted of metallic silicon, which had become oxidized into silica during the process.

The solution had a greenish-yellow appearance, and after being concentrated by evaporation and diluted with distilled water was mixed with carbonate of magnesia. The precipitate dissolved in hydrochloric acid and again precipitated with succinate of ammonia, yielded 2.50 oxide of iron = 1.7335 metallic iron; no manganese could be discovered.

The filtered liquid now held in solution the protoxide of iron. Digested with nitric acid, precipitated with ammonia, ignited, dissolved again and freed from magnesia, it yielded oxide of iron = 1.100 = 0.8901 metallic iron. This method of separating the protoxide of iron from the peroxide is not sufficiently accurate, and I mention this part of the process only to show that the iron, even after ignition, remained in the state of protoxide mixed with peroxide. Having tried to ascertain the amount of protoxide and peroxide of iron in these solutions as accurately as possible, by converting the peroxide of iron by means of solution of sulphuretted hydrogen into protoxide, and ascertaining the quantity of separated sulphur &c. in one part of the solution, and mixing another part with liquid chloride of gold and sodium,—from the quantity of the reduced gold the quantity of protoxide of iron was very easily calculated, and I obtained for the greatest part proportions which approached to a rather strange formula,

$3\ddot{\text{F}} + 2\ddot{\text{F}}$. The carbon taken away by the escaping hydrogen is of far *less amount* than is generally asserted; and each atom of carbon volatilized in this way is at the *same time replaced by oxygen* or sometimes hydrogen.

The remaining part of the brown powder, = 3.39, was likewise put into a platinum crucible, heated like the first over a spirit lamp, till it began to glow on the periphery, and then quickly removed from the fire. The first smooth surface of the powder on the bottom of the crucible was now found to be intersected by small cracks, through which might be seen the interior of the powder in ignition for some time; its colour after cooling became darker, but it had neither gained nor lost in weight.

Hydrochloric acid of 1.16 sp. gr. did not visibly attack this powder; but heated in a sand bath, the action of the acid became at once very lively; a great quantity of hydrogen smelling slightly in the well-known way was disengaged, and a velvety black residuum of a rather oily appearance was left.

Its weight was found to be 0·75 grains, and it had therefore lost in the acid 2·64 grains.

During ignition it emitted very dense fumes—had changed its dark black colour into a dirty reddish-white; it had lost only 0·051 grains, notwithstanding the great apparent evolution of gases, and we must therefore conclude, that the black residuum contained silicon, taking in exchange for carbonaceous matter oxygen from the atmosphere.

By means of hydrochloric acid and succinate of ammonia iron was separated with a trace of manganese = 0·32 grains = 0·2219 grains of metallic iron, and silica = 0·332 grains = 0·1595 grains of silicon.

The oily residuum contained therefore, in 0·6989 grains,

Iron	0·2219 or peroxide of iron	0·3200
Silicon	0·1595 or silica	0·3320
	<hr/>	<hr/>
	0·3814	0·6520

The hydrochloric solution, from which the white powder just mentioned remained, was colourless as water; after oxidation with nitric acid, oxide of iron was separated equal to 3·12, viz.: iron 2·163408, and oxygen 0·956592; oxide of iron separated from the silica = 0·32 grains. The oxide of iron contained in the liquid amounted therefore, as we see, to 3·12 gr.; on the contrary, what the iron lost by this treatment with acids amounted only to 2·64; the iron therefore could be neither in the state of protoxide nor peroxide in the remainder.

Now the first residuum before ignition weighed 3·13, after ignition 3·55; and contained therefore

Silica	0·265
Silicon	0·025
	<hr/>
	0·290
Loss of protoxide of iron .	3·260
	<hr/>
	3·550 grains,

which is the weight of the residuum as before mentioned.

If we now consider the amount of iron and oxygen in the first separation of protoxide and peroxide of iron, we find

Peroxide:

Iron	1·7335	Oxygen	0·766500
--------------	--------	----------------	----------

Protoxide:

Iron	0·5869	Oxygen	0·173050
Total amount	2·3204	Total amount	0·939550

Silicon	0·127306
Do. mixed with the residuum	0·025000
	<hr/>
	0·152306

Oxygen	0·137694
Total amount of oxygen before the 2nd ignition.	1·077244
Oxygen after 2nd ignition	0·028000

Therefore we have

Metallic iron	2·320400
Silicon	0·152306
	<u>2·472706</u>

Oxygen	1·077244
Increase during ignition	0·420000
Difference	<u>0·657244</u>

Powder before ignition weighed as } already stated	3·130000
Amount of bases	2·472706
	<u>0·657294</u>

The difference is to be ascribed to the volatilized carbon, hydrogen and azote.

If we compare this difference with the difference between the real increase of the powder during ignition, and the calculated oxygen, we find the

Former difference	0·657294
Last difference	0·657244
	<u>0·000050</u> grains.

This difference is so extremely small that we may consider the oxygen only calculated as the real quantity taken up, instead of 0·65729 carbon, hydrogen and azote. The increase could therefore only be 0·420000, as the remaining quantity of the absorbed oxygen = 0·657244, was *counterbalanced* by the 0·657294 burnt carbon, hydrogen and azote.

The black residuum of the second part of the powder, which as already stated weighed 3·39 grains, was equal to 0·75; the loss therefore in 3·39 grains was = 2·64, consisting of iron and oxide of iron.

If we calculate from the hypothetically-assumed quantity of carbon, hydrogen and azote in the first part of the powder, the same relative quantity for the second part of the powder, we ought then to have for 3·39 grains of residuum 0·71188 grains of carbon, hydrogen and azote.

If we therefore assume the black residuum to contain only silicon, and iron in its oxidized state, we obtain as already mentioned,

Silicon	0·1595
Oxide of iron	0·3200
	<u>0·4795</u>

This difference between the original weight of the remainder = 0·7500, which amounts to 0·2705, we may consider as carbon.

Now we calculated the amount of carbon to be 0·71188, but as we find only 0·2705 grains of carbon, the difference = 0·7118 — 0·2705 = 0·44138 carbon, must have been counterbalanced by an equal quantity of oxygen to produce *oxidum ferroso-ferricum* in the following way:—

Oxygen	0·44138
Iron	1·12268
<i>Oxidum ferroso-ferricum</i> . .	1·26406
Remaining iron	1·040728.
The	2·604788

will be therefore the calculated loss; the actual loss on the contrary was found as before mentioned = 2·64; the difference between experiment and calculation amounts only to 0·035212.

[To be continued.]

LXIV.—*On the Heat of Vapours and on Astronomical Refractions.* By JOHN WILLIAM LUBBOCK, Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.*

PREFACE.

THE connexion between the temperature and the pressure (or elasticity) of elastic vapours is a desideratum in Physics. A knowledge of it is indispensable to an exact theory of the Steam Engine, to an exact theory of Astronomical Refractions, and to an accurate solution of other important problems. The want of it has hitherto been supplied by unsatisfactory approximations; but these questions cannot be completely investigated without a more careful attention to the premises than has hitherto been possible, owing to a want of the proper key to these researches, which consists in a knowledge of the mathematical law which connects the temperature and the pressure in elastic fluids, and which is required in addition to the law of Mariotte and Gay Lussac to complete their theory.

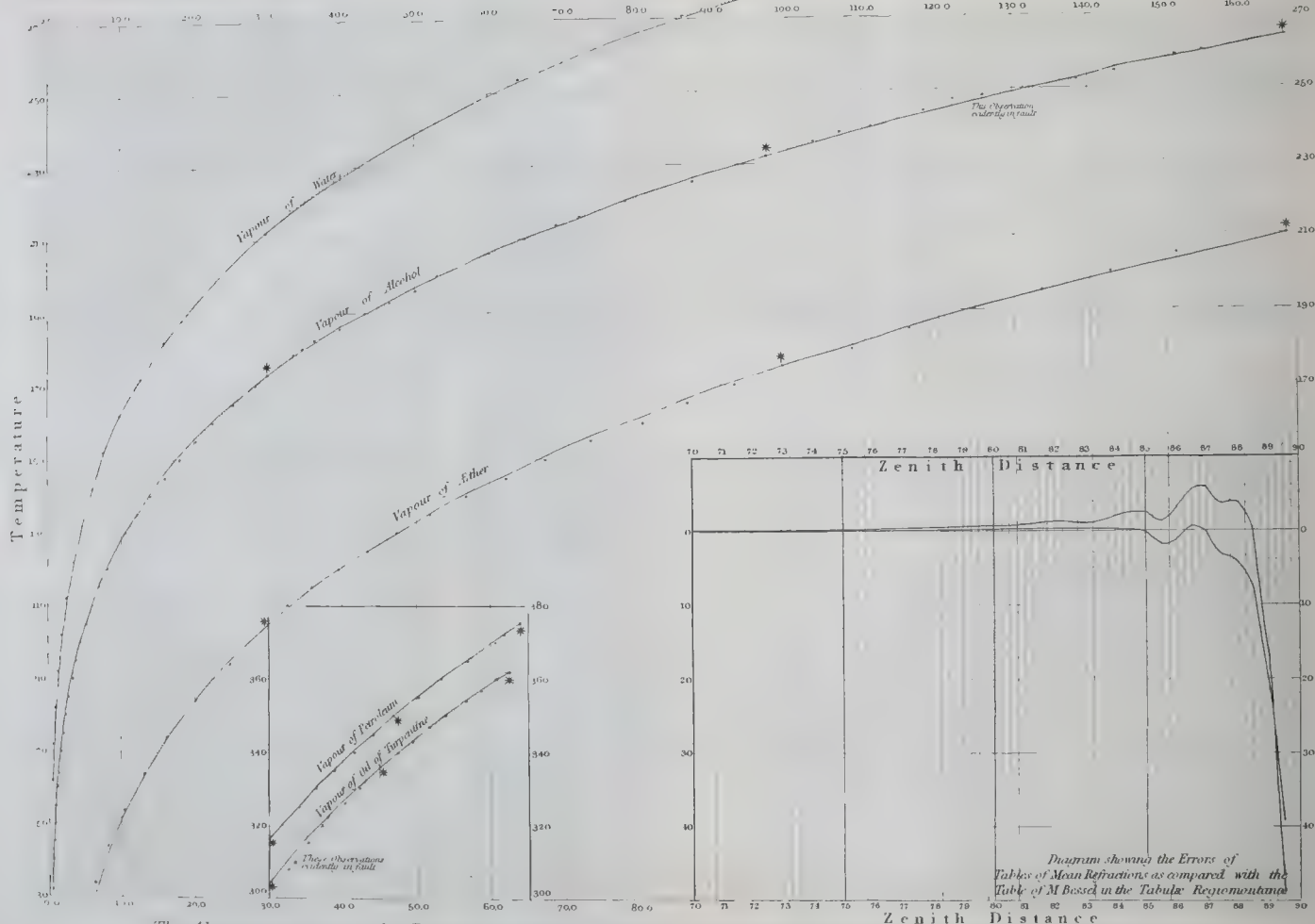
If V represent the absolute heat or *caloric*, i the *latent* heat, c the *sensible* heat or that which affects the *thermometer*,

$$V = i + c.$$

* Reprinted, by the obliging permission of the author, from the original memoir lately published. London, 1840. 8vo.

*M. Labbeck on the Heat of Vapours
and on Astronomical Refractions*

Pressure



*The Abscissa represents the Pressure in inches of Mercury, and the Ordinate the Temperature in Fahrenheit's Scale
Calculated ——— Observed.*

The Observations marked with an Asterisk are those which were employed in procuring the Constants R & E

If θ be the *temperature* as indicated by a thermometer, there can be little doubt that V is capable of being expressed in a series proceeding according to positive powers of θ , so that

$$V = a + b\theta + c\theta^2 + \&c.$$

a , b , c , &c., have a certain signification in Taylor's theorem, but without being able to determine their values, *à priori*, or to obtain any relations between them, they may be treated as constants. If the *latent* heat be constant, which is probable, and if the effect indicated by the thermometer is proportional to the sensible heat,

$$c = b\theta, \quad V = a + b\theta.$$

It must, however, be left to experiment to decide how many terms are to be taken into account for any given substance, within any given range of the thermometric scale, and in order to satisfy the results of observation within any given quantity. The other suppositions upon which my theory is founded are those of Laplace, viz. that the quantity called γ by M. Poisson is constant for the same substance at different temperatures, and that the equation

$$V = A + B \frac{1}{\theta^{\gamma}}$$

is the solution of a certain differential equation. See *Méc. Cél.*, vol. v. p. 108. Poisson, *Méc.*, vol. ii. p. 640.

The theorems which are given by M. Poisson in the second volume of the *Mécanique*, and which are also to be found in the works of Pouillet and Navier, rest upon the condition that the absolute heat is constant, while the sensible heat varies. This is the most restricted hypothesis which can be made upon the nature of heat, and it does not satisfy the observations. In this Treatise I have gone a step further, by supposing the absolute heat to vary with the sensible heat, or to be represented by an expression of the form $a + b\theta$, (or what is the same, $V = C + D(1 + \alpha\theta)$. See p. 2.) θ being the temperature reckoned from some fixed point, a and b constants. This includes implicitly the other hypothesis, which if true, in determining a and b by means of observations, the constant b should come out zero. This in the case of steam is certainly not the case, nor is it so in any case which I have examined.

The experiments of Dulong and Arago upon steam at high temperatures, those of Southern and Dalton, and those of Dr. Ure, furnish data by which the supposition I have adopted and the formulæ which flow from it can be scrutinized; and if the expressions which result from it fail to represent those observations, we have at least arrived at this conclusion, that the condition of the invariability of the quantity called γ by M. Poisson does not ob-

tain in nature, or that the absolute heat cannot be represented by so simple a function of the temperature or sensible heat. Recourse must then be had to more complicated expressions. If, on the contrary, my formula represents the observations of the temperature of vapours with accuracy, its origin in a simple theoretical notion of the quantity of absolute heat, and its simplicity, are great additional recommendations in its favour. The formula which I have obtained does, I believe, represent the observations better than any hitherto devised; at low temperatures and pressures it deviates a little, but a very slight error in the observed pressures may account for this discrepancy. Dalton says that it is next to impossible to free any liquid entirely from air; of course if any air enter, it unites its force to that of the vapour. Moreover, when the pressures are small, the variation of temperature becomes great for a small variation of pressure; so that the agreement of theory with observation may be considered as complete, even if the absolute amount of the error of the calculated temperature is then more considerable.

My formula has also been compared with the observations of Dr. Ure, on the vapour of alcohol, æther, petroleum, and oil of turpentine, recorded in the *Philosophical Transactions* for 1818*.

I think that the comparisons contained in this treatise afford sufficient evidence that my formula is established, and that the deviations of the calculated results from those of observation are within the limits of the errors of the latter; but this point I leave to be decided by those more conversant with the nature of the experiments. It would not militate against my views if it were found necessary to take in an additional term and to make

$$V = C + D(1 + \alpha \theta) + E(1 + \alpha \theta)^2 + \&c.$$

but the expressions for the temperature and density in terms of the pressure would not be quite so simple, although more pliable.

As the same principles must be applicable to the constitution of the atmosphere, I have examined the observations made by M. Gay Lussac in his *aéronautic* ascent from Paris, and which are published by M. Biot in the *Conn. des Temps*. My calculated temperatures may be considered as identical with the *températures régularisées* of M. Biot, which are given by that distinguished philosopher as representing the condition of the atmosphere divested of the irregularities and errors incidental to observations made under circumstances so difficult and so disadvantageous. But the altitude to which man can ascend is so limited, that observations of the temperature made in *aéronautic* ascents will never fur-

[* Dr. Ure's paper containing these observations was reprinted in *Phil. Mag.*, First Series, vol. liii. p. 38, *et seq.*—EDIT.]

nish so complete a test of the accuracy of any formula professing to give the relation between the pressure and the temperature in elastic fluids, as observations of the temperature of the vapour of water and other substances, which can be carried through a greater range of the thermometric scale, and above all through the low pressures where the character of the curve is more decided.

M. Biot has dwelt with reason upon the importance of introducing into the theory of Astronomical Refractions a greater conformity with the conditions of the problem than has hitherto been attempted; and he has also noticed the imperfection in principle of the present mode of calculating heights by observations of the barometer, a method which must of course be abandoned (at least in any accurate exposition of this theory) whenever the discovery of the true connexion between the temperature and the pressure of the higher regions of the atmosphere renders it possible to adopt a more rigorous mode of eliminating the density from the differential equation which connects dp and dz . The correct expression which connects the difference of altitude with the pressures at the upper and lower stations ought to be the foundation of the theory of Refractions. Considering on the one hand the notions upon which my formula is ultimately founded, its identity with the results offered by the observations of steam and other vapours, and moreover the agreement afforded by the direct comparison with the observations of M. Gay Lussac, there can be no doubt that it represents the density of the atmosphere at different altitudes with greater fidelity than any hypothesis which has up to the present time been made the basis of the theory of Astronomical Refractions.

I think that my table of mean refractions represents the observed quantities within the limits of their probable errors, and I have obtained this result without any arbitrary alterations of the constants.

In the higher regions of the atmosphere the cold is intense*, depriving the air of its elasticity and converting it into a liquid or solid substance. My formula of course is only applicable as long as the air continues in the state of an elastic vapour; and if at any altitude it ceases to maintain that condition, the density must be represented by a discontinuous function. But the density of this frozen air must be extremely small, and it probably has little effect upon the amount of Refraction.

I am indebted to Mr. Russell for his kind assistance in the numerical calculations which accompany this treatise.

29, *Eaton Place*, March 2, 1840.

* See Poisson, *Théorie de la Chaleur*, p. 460.

CONTENTS.

General Expressions.—On the Pressure of Steam.—On the Steam Engine.—On the Vapours of *Æther*, Alcohol, Petroleum, and Oil of Turpentine.—On the Conditions of the Atmosphere and on the Calculation of Heights by the Barometer.—On Astronomical Refractions.

GENERAL EXPRESSIONS.

Let V be the quantity of absolute heat, considered as a function of the sensible heat or temperature θ ,

$$\frac{dV}{d\theta} = \frac{dV}{d\varrho} \frac{d\varrho}{d\theta} + \frac{dV}{dp} \frac{dp}{d\theta}$$

$$p = k \varrho (1 + \alpha \theta).$$

ϱ being the density, p the pressure, k and α constants,

$$\frac{d\varrho}{d\theta} = -\frac{\alpha \varrho}{1 + \alpha \theta}$$

$$\frac{dp}{d\theta} = \frac{\alpha p}{1 + \alpha \theta}$$

if

$$-\frac{dV}{d\varrho} \frac{\alpha \varrho}{(1 + \alpha \theta)} = \gamma \frac{dV}{dp} \frac{\alpha p}{(1 + \alpha \theta)}$$

$$\varrho \frac{dV}{d\varrho} + \gamma p \frac{dV}{dp} = 0.$$

If γ be considered as a constant quantity the integral of this partial differential equation is

$$\frac{1}{\varrho} \frac{p^\gamma}{\gamma} = \text{funct}^n. V^*.$$

The simplest form which can be assigned to this function of V is such that

$$V = A + B \frac{1}{\varrho} \frac{p^\gamma}{\gamma}$$

A and B being constants.

* So far the reasoning is identical with that contained in the *Mécanique* of M. Poisson; but M. Poisson proceeds further upon the limited supposition of V being constant.

Laplace arrived at this equation (*Méc. Cél.* vol. v. p. 128.).

See Poisson, *Annales de Chimie et de Physique*, tom. xxiii. p. 342*; *Mécanique*, vol. ii. p. 648; Navier, *Leçons données à l'Ecole des Ponts et Chaussées*, tom. ii.

$$V = A + B \frac{p}{\rho}^{\frac{1}{\gamma}} \quad V' = A + B \frac{p'}{\rho'}^{\frac{1}{\gamma}}$$

$$p = k \rho (1 + \alpha \theta) \quad p' = k \rho' (1 + \alpha \theta')$$

k and α being constants.

I will now introduce the additional condition that the heat is *proportional* to the temperature, in which case

$$V = C + D(1 + \alpha \theta) \\ V' = C + D(1 + \alpha \theta')$$

C and D being constants. These equations include implicitly the hypothesis attributed to Watt and also that of Southern, respecting the vapour of water: on the former $D=0$. Hence

$$V = C + D(1 + \alpha \theta) = A + B \frac{p}{\rho}^{\frac{1}{\gamma}}$$

$$V' = C + D(1 + \alpha \theta') = A + B \frac{p'}{\rho'}^{\frac{1}{\gamma}}$$

$$1 + \alpha \theta' = (1 + \alpha \theta) \frac{p'^{\frac{\gamma-1}{\gamma}}}{p^{\frac{\gamma-1}{\gamma}}} \left\{ \frac{1 - \frac{D}{k B} p^{\frac{\gamma-1}{\gamma}}}{1 - \frac{D}{k B} p'^{\frac{\gamma-1}{\gamma}}} \right\}$$

If $\frac{D}{k B} = E$ and if θ correspond to the boiling point, $\theta = 180^\circ$ in Fahrenheit's scale, if the pressure be measured in atmospheres $p = 1$, but generally

$$1 + \alpha \theta' = (1 + \alpha \theta) \frac{(p^{\frac{1-\gamma}{\gamma}} - E)^\dagger}{(p'^{\frac{1-\gamma}{\gamma}} - E)} \quad [1]$$

[* A translation of M. Poisson's memoir here referred to will be found in *Phil. Mag.*, First Series, vol. lxii. p. 328.—EDIT.]

† This equation must not be confounded with another equation which may be deduced from it by making $E = 0$, and which is not reconcileable with phenomena, as was long since noticed by M. Poisson in the case of steam. An equation of this kind is given by M. Pouillet in the form

$$x = .2669 \left(\frac{760}{p} \right)^{1 - \frac{1}{1.375}}$$

as $p' = k \varrho' (1 + \alpha \theta')$

$$\frac{\varrho'}{\varrho} = \frac{p' (p'^{\frac{1-\gamma}{\gamma}} - E)}{p (p^{\frac{1-\gamma}{\gamma}} - E)}, \quad [2]$$

$$= \left(\frac{p'}{p}\right)^{\frac{1}{\gamma}} \left\{ \frac{1 - E p'^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} \right\}$$

if
$$\frac{E p^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} = -H$$

$$\left(\frac{p'}{p}\right)^{\frac{\gamma-1}{\gamma}} = 1 - q \quad \frac{1 - E p'^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} = 1 - Hq$$

$$\frac{\varrho'}{\varrho} = (1 - q)^{\frac{1}{\gamma-1}} (1 - Hq),$$

if $\log (1 - Hq) = -u \quad c^{-u} = 1 - Hq$

c being the number of which the hyperbolic logarithm equals unity

$$\frac{\varrho'}{\varrho} = H^{\frac{1}{1-\gamma}} c^{-u} \left\{ c^{-u} - 1 + H \right\}^{\frac{1}{\gamma-1}}.$$

if $\frac{\varrho'}{\varrho} = 1 - \omega$

$$\omega = 1 - H^{\frac{1}{1-\gamma}} c^{-u} \left\{ c^{-u} - 1 + H \right\}^{\frac{1}{\gamma-1}}$$

Since $C + D (1 + \alpha \theta) = A + B \frac{p}{\varrho}^{\frac{1}{\gamma}}.$

for atmospheric air. *Elémens de Physique*, vol. i. p. 400, and by Navier, *Leçons données à l'Ecole des Ponts et Chaussées*, vol. ii. p. 310, in the form

$$v' = \frac{(1 + \alpha v)}{\alpha} \left(\frac{\varpi'}{\varpi} \right)^{.3748} - \frac{1}{\alpha}.$$

$$\text{If } \frac{1}{\varrho} = v$$

$$\alpha D(\theta' - \theta) = B p^{\frac{1}{\gamma}} \{v' - v\} = V' - V,$$

supposing the heat and the volume to vary, the pressure remaining constant.

According to Dulong the following laws obtain, which however, are not admitted by Dr. Apjohn (see L. & E. Phil. Mag. 1838, vol. xiii. p. 339):

“1°. Des volumes égaux de tous les fluides élastiques pris à une même température et sous une même pression, étant comprimés ou dilatés subitement d’une même fraction de leur volume, dégagent ou absorbent la même quantité absolue de chaleur. 2°. Les variations de température qui en résultent sont en raison inverse de leur chaleur spécifique à volume constant.”—*Mém. de l’Institut*, tom. x. p. 188.

According to the first of these laws the quantity B must be the same for different vapours; of the second I am unable to offer any satisfactory interpretation.

In what follows I propose to ascertain how far the equations [1] and [2] satisfy the best observations on record. The general relation gives

$$1 + \alpha \theta'' = (1 + \alpha \theta) \frac{p^{\frac{1-\gamma}{\gamma}} - E}{(p''^{\frac{1-\gamma}{\gamma}} - E)}.$$

Eliminating E between this equation and that which connects θ' and p' ,

$$- \theta) (1 + \alpha \theta') (p'^{\frac{1-\gamma}{\gamma}} - p^{\frac{1-\gamma}{\gamma}}) = (\theta' - \theta) (1 + \alpha \theta'') (p''^{\frac{1-\gamma}{\gamma}} - p^{\frac{1-\gamma}{\gamma}})$$

$$\text{If } \frac{1-\gamma}{\gamma} = \beta$$

$$\frac{\left(\frac{p''}{p}\right)^{\beta} - 1}{\left(\frac{p'}{p}\right)^{\beta} - 1} = \frac{(\theta'' - \theta) \left(\frac{1}{\alpha} + \theta'\right)}{(\theta' - \theta) \left(\frac{1}{\alpha} + \theta''\right)}.$$

From this equation, knowing θ'' , θ' , θ , p'' , p' , p ; β may be determined for any gas or vapour. Knowing β , E may be found from the equation

$$E = \frac{p'^{\beta} \left(\frac{1}{\alpha} + \theta'\right) - p^{\beta} \left(\frac{1}{\alpha} + \theta\right)}{\theta' - \theta}.$$

[To be continued.]

LXV. *Memoir on the Law of Substitutions, and the Theory of Chemical Types.* By M. DUMAS.

[Continued from p. 329.]

Chemical Types.

CONSIDERED in itself, the law of substitutions has a practical importance quite sufficient to justify the necessity of distinguishing it from the more general actions of chemistry. But this distinction becomes necessary in quite another way, when to this kind of instinct which led us to view it at first as a law of nature, succeeds the nearly perfect certainty that it is connected with one of the most mysterious and most important phænomena of the science.

I mean the existence of chemical types, capable, without being destroyed, of undergoing the most singular transformations, and of which all the elements might successively disappear, others being substituted for them. I mean those organic types, the admission of which into the domain of organic chemistry seemed to me to be henceforward inevitable, in consequence of the best-characterized experiments which had been suggested by the law of substitutions.

Thus it being possible to convert acetic acid into chloracetic acid by the action of chlorine without its at all losing its capacity for saturation, I have considered the chloracetic acid which results from it as acetic acid in which chlorine had been substituted for hydrogen: these two bodies have appeared to me to belong to one type, to one kind.

Now, when I established, five years ago, the analogy of iodoform, of bromoform, of chloroform, and of anhydrous formic acid, when I added that electro-negative bodies, such as sulphur, phosphorus, and arsenic, might be substituted for iodine, chlorine, and oxygen, no one raised the least difficulty; this series of formulæ entered the science without obstacle.

It is then because the development of the experiment has led us to look upon chlorine and hydrogen as susceptible of taking the place of each other in a compound, that certain convictions revolt. We are then led to examine what we should understand by an *organic type*, and to discuss this point: for example, it being possible for iodine, bromine, chlorine, and oxygen to take each other's place in a compound without destroying its type, can this power (*rôle*) be denied to hydrogen? In other terms, iodoform, bromoform, chloroform, oxiform, being admitted as species of the same genus, can we refuse this character to hydroform, that is to say, to the

gas of the acetates, because it contains hydrogen, and no longer an electro-negative body?

Before mentioning my opinion, which is however well known, it is necessary that the three points which make the difficulty, and which are the definition of chemical types, should be well understood, that of the fundamental properties, and the confounding of the function (*rôle*) which hydrogen and chlorine perform among chemical bodies. Prepossessed, for a long while with the necessity of establishing a good natural classification of organic bodies, I have sought for its basis in their chief characters. The discovery of chloracetic acid gave me an opportunity of developing a view of this nature. Acetic acid and chloracetic acid, as distinct bodies, constitute two species, which I have classed in one genus, by reason of the analogy of their fundamental properties and of the identity of their formulæ.

Thus I propose uniting into one genus all the compounds which unite identical formulæ with similar chemical properties. Chloroform, bromoform, iodoform, constitute one genus; olefiant gas and the various chloridated bodies, products which are derived from it, constitute another; acetic acid and chloracetic acid represent a third, &c.

I class then in one genus, or what comes to the same thing, I consider as belonging to the same chemical type, *the bodies which contain the same number of equivalents, united in the same manner and which possess the same fundamental chemical properties.*

The definition of a chemical type carries with it then that of the properties which I call fundamental. Now, by what do we recognize a fundamental property? This is a question easily answered by examples which appear to be conclusive. When we boil chloracetic acid with an alkali, it is at once destroyed and is changed into carbonic acid and chloroform. If we class, as I have done, acetic acid and chloracetic acid in one genus, we are compelled to conclude from this that acetic acid treated with alkalies will change to its turn into carbonic gas, and into a carburetted hydrogen corresponding to chloroform, and into marsh gas (*gas des marais*). This is precisely the result which is given by experiment.

But, say MM. Pelouze and Millon, these approximations are purely fortuitous. If acetic acid heated with barytes changes into carbonic acid and into marsh gas, it is that barytes simply determines the formation of the carbonic acid, that it takes from acetic acid all the carbonic acid which its constitution allows it to supply.

Let us admit this first point for the moment; why should

the remainder of the elements of the acetic acid constitute marsh gas rather than anything else? There are four equations, which, at the low temperature at which the action takes place, are alike admissible. The acetic acid always furnishing carbonic gas, it may give besides,

1. Carbon and hydrogen..... $C^4 + H^8$,
2. Methylene and hydrogen $C^4 H^4 + H^4$,
3. Olefiant gas and hydrogen $2 C^2 H^2 + H^4$,
4. Marsh gas $C^4 H^8$.

Thus, when we only consult the general forces of chemistry, at least four suppositions are presented between which there is nothing to authorize a choice; these are the possible actions. When the consideration of the types is allowed to intervene, it chooses between these four possible actions the necessary action, that which will give rise to four volumes of a hydrocarburetted gas corresponding to chloroform, which it represents in this decomposition.

It is not sufficient then to explain the decomposition of the acetic acid by the alkalies, by saying that those determine the formation of all the carbonic acid which can be produced; we must besides give an account of the production of marsh gas. Now if four equations alike possible are presented, how shall we choose?

We see well that the notion of organic types meets the same difficulty as the law of substitutions. The types are sent back to the general forces of chemistry, as the substitutions were sent back to the equivalents.

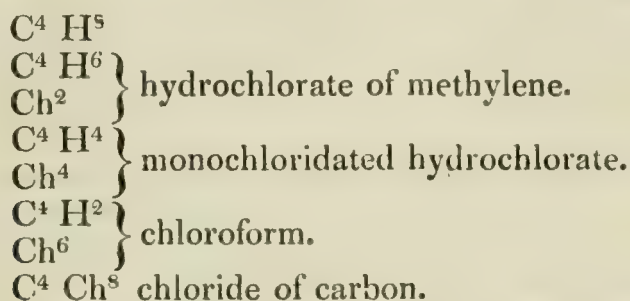
The reply is then the same: when we put into play the general forces of chemistry only, the decomposition of acetic acid into carbonic acid and marsh gas is a possible fact. When we set out from the analogy which exists between acetic acid and chloracetic acid, it is a necessary fact. In the first case we should have perfectly understood that some carbon was deposited, that some methylene or olefiant gas was disengaged. In the second, some marsh gas must absolutely be disengaged.

But it is very evident, that the production of carbonic gas, and of marsh gas by the decomposition of acetic acid by means of alkalies is a fact which does not shock the general ideas of chemistry, who explains herself by the play of the general affinities at her disposal. That need not have been demonstrated: a true fact is always possible.

Thus in the view taken by general chemistry, marsh gas might be formed; but viewed by the theory of organic types, it was necessary that that compound should be formed.

Now let us go further: let us suppose that the marsh gas be subjected to the action of chlorine; very different actions might be produced, if we only consult the general forces of chemistry.

Viewed precisely by the theory of organic types, if the marsh gas corresponds to chloroform, to methylic æther, &c., it realizes the carburet of hydrogen, which constitutes the starting point of this series, and by means of chlorine it should give:



We know, by the experiments which I recently made known to the Academy, that marsh gas obtained from the acetates changes under the influence of chlorine into this chloride of carbon $\text{C}^4 \text{Ch}^8$, which the theory of types had predicted as the necessary product of the action.

Let me add too, that before giving this chloride of carbon, it also produces some chloroform. But if marsh gas (*gas des marais*) corresponds to the chloroform as the acetic acid does to chloracetic acid, the conversion of marsh gas into chloroform is as necessary a fact as the conversion of the acetic acid into marsh gas.

If when these necessary facts have been recognized as true be experience, it be then proved that they were possible, that they did not disagree with the general laws of chemistry, I contend that the difficulty has not been met. What ought to be done in such a case, is to show how the general theory allows us to foresee that acetic acid should give marsh gas, and that marsh gas should give chloroform.

Far from thinking that I have gone too far in establishing, as I have done, genera for uniting acetic acid and chloracetic acid, marsh gas, and chloroform, I have on the contrary been too cautious.

I therefore persist in my opinion as to the propriety of uniting into one genus those *bodies which contain the same number of equivalents united in the same manner, and which are endowed with the same fundamental chemical properties.*

In this discussion of the characters of chemical types and of the true acceptation of the fundamental properties of bodies, I have said nothing of the identical function attributed

to the chlorine and to the hydrogen in acetic acid and in chloracetic acid, in the chloroform and in marsh gas.

Here, however, as it was easy to foresee, is the point which particularly arrested the attention of M. Berzelius, and which he combated by changing all my formulæ and substituting new ones for them.

Down to the present time I have made no reply. Indeed, what could I have added to the following note that M. Liebig authorized me to publish in his name?

“In my interest for the science,” says M. Liebig, “I must declare that I do not share the opinions of M. Berzelius, because they rest upon a mass of suppositions which cannot be proved.

“In mineral chemistry the singular observation has been made that chlorine may be substituted for manganese in permanganic acid, without the form of the salts produced by this acid being changed. Nevertheless it is hardly possible to find two bodies between which there exists a greater difference in chemical properties than there is between chlorine and manganese.

“An experiment of this kind is not to be discussed; we must leave to the fact all its value, and say, chlorine and manganese may take each other’s place without the nature of the combination being changed by it. From that time I do not see why this manner of acting should be considered as impossible for other bodies, such for example as chlorine and hydrogen.

“The interpretation of these phænomena, such as it has been laid down by M. Dumas, appears to me to give the key to most of the phænomena of organic chemistry.

“Without denying that bodies take each other’s places in a great number of combinations, according to their place in the electric order, I think, from the manner of acting of organic combinations, we should draw this conclusion;—that *a reciprocal substitution of simple or compound bodies, acting in the manner of isomorphous bodies, should be considered as a true law of nature.* This substitution may take place between bodies which neither have the same form nor are analogous in composition. It depends exclusively on the chemical force which we call affinity.”

These opinions are, in fact, quite conformable to those which I myself published, when I compared the principle of substitutions to the principle of isomorphism, and the bodies of the same chemical type to the isomorphous bodies themselves.

I do not pretend to say, that bodies of the same chemical

type should offer the same form ; everything leads us to believe that this condition does not always exist, but up to the present time researches are wanting upon this subject.

[To be continued.]

PROCEEDINGS AT THE FRIDAY-EVENING MEETINGS OF THE
ROYAL INSTITUTION.

March 20.—Mr. Schomburgk on the aboriginal inhabitants of Guiana ; their manners, customs, and present condition.

March 27.—Dr. George Gregory. Statistics of disease and mortality in the metropolis.

April 3.—Mr. Cowper on the manufacture of cotton.

April 10.—Mr. Nasmyth on the functions of the mouth and the structure of recent and fossil teeth.

April 17.— { No meetings.
April 24.— }

METEOROLOGICAL OBSERVATIONS FOR MARCH, 1840.

Chiswick.—March 1, 2. Cold and dry. 3. Cloudy. 4. Bleak and cold. 5. Frosty : cold and dry : sharp frost at night. 6, 7. Frosty haze : fine. 8, 9. Clear and frosty : fine. 10. Very fine. 11. Drizzly. 12. Cloudy. 13. Hazy : fine. 14. Overcast : very fine. 15. Slight rain. 16. Fine but cold. 17. Clear. 18. Overcast. 19, 20. Cloudy and cold : clear. 21. Very clear. 22. Overcast. 23. Fine but cold. 24, 25. Clear and cold. 26—28. Cloudy and cold. 29, 30. Cloudy and fine. 31. Drizzly.

It may be observed that the quantity of rain in this month was less than 3-10ths of an inch. The barometer stood remarkably high and in general very steady.

Boston.—March 1—3. Fine. 4. Stormy. 5—8. Fine. 9. Cloudy. 10. Fine. 11, 12. Cloudy. 13. Fine. 14. Rain. 15. Cloudy : rain p.m. 16, 17. Cloudy. 18. Rain : rain p.m. 19, 20. Cloudy. 21. Fine : snow early a.m. 22. Cloudy : rain p.m. 23. Cloudy : snow early a.m. 24. Hail : snow early a.m. 25. Fine : snow early a.m. 26. Fine : snow p.m. 27, 28. Cloudy. 29. Cloudy : rain p.m. 30, 31. Cloudy.

Applegarth Manse, Dumfries-shire.—March 1, 2. Fine clear day : frosty. 3. The same : getting cloudy p.m. 4, 5. The same : still freezing. 6. Remarkably fine day : gentle frost. 7—9. The same : hoar frost a.m. 10. The same, but threatening change. 11. The same, but cloudy : no frost. 12. The same : continuing cloudy : no frost. 13. Dry and boisterous : cloudy. 14. Dry but cloudy. 15. Fine day : rain a.m. 16. Slight rain morning : cleared up. 17. Fine : frosty early a.m. 18. Fine : the same. 19. Fine : without frost. 20. Fine : hoar frost. 21. Fine : strong frost. 22. Fine : getting cloudy. 23. Passing showers of snow and hail : frosty. 24, 25. The same : very cold : frosty. 26. Fair but cloudy. 27. Fine but dull. 28. Remarkably fine day. 29. The same after a shower a.m. 30. Wet morning : drizzly all day. 31. Occasional showers.

Sun shone out 29 days. Rain fell 5 days. Snow and hail 1 day. Frost and hoar frost 17 days.

Wind north $1\frac{1}{2}$ day. North-east $8\frac{1}{2}$ days. East 2 days. South 4 days. South-west 3 days. West 2 days. North-west 6 days. North-north-west 1 day. North-north-east 1 day. Variable 2 days.

Calm 15 days. Moderate 9 days. Brisk 5 days. Strong breeze 2 days.

of the Horticultural Society at Chiswick, near London; by Mr. VEALL at Chiswick, and by Mr. DUNBAR at Applegarth Manse, Dumfries-shire.

Days of Month. 1840. March.	Barometer.				Thermometer.				Wind.		Rain.			Dew point. Lond.: Roy. Soc. 9 a.m.					
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire. 9 a.m.	Dumfries-shire. 8½ p.m.	London: Roy. Soc. Self-register.		Chiswick.	Dumfries-shire. Max. Min.	London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.		Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.
		Max.	Min.				Max.	Min.											
1.	30·374	30·422	30·399	30·12	30·42	30·45	32·7	33·2 30·7	39	29	35	33	26	E.N.E.	E.N.E.	28
2.	30·458	30·503	30·459	30·20	30·51	30·52	36·8	37·4 31·2	45	32	37·5	33	27	N.E.	E.	25
3.	30·430	30·481	30·408	30·12	30·51	30·53	38·8	39·6 35·0	48	30	41	39	28	N.	calm	32
4.	30·386	30·425	30·382	30·10	30·50	30·45	35·8	36·3 32·8	48	24	37	41	30½	N.E.	E.	30
5.	30·408	30·538	30·413	30·10	30·38	30·33	35·4	36·0 32·6	50	19	57	46	28	E.	calm	29
6.	30·454	30·570	30·479	30·06	30·36	30·38	36·7	37·2 32·0	53	20	37·5	50	30½	E.N.E.	calm	30
7.	30·564	30·664	30·580	30·20	30·45	30·53	37·2	37·8 32·7	52	21	35	50	27½	N.	calm	32
8.	30·656	30·726	30·683	30·27	30·57	30·59	37·7	39·0 32·7	54	20	33	49	26½	N.	calm	30
9.	30·640	30·676	30·593	30·24	30·59	30·49	37·2	38·0 31·7	56	25	33	58	27½	N.	calm	31
10.	30·392	30·430	30·322	29·95	30·35	30·32	39·0	47·4 35·3	56	41	37	53	29½	NNW.	calm	33
11.	30·284	30·369	30·301	29·88	30·32	30·30	42·8	53·7 38·6	46	34	43	49	40½	N.	E.	40
12.	30·274	30·295	30·244	29·82	30·20	30·10	43·8	46·3 40·8	51	29	42	49½	35	NNW.	calm	38
13.	30·106	30·143	30·050	29·62	29·97	29·95	45·0	46·0 40·7	53	39	43	47½	42	NW.	calm	39
14.	30·050	30·069	30·060	29·59	29·94	29·93	45·8	46·5 42·0	49	35	43	52	39	WNW.	calm	40
15.	29·974	29·994	29·844	29·48	29·70	29·82	43·2	43·8 40·4	48	38	43	52	40	W.	calm	38
16.	30·042	30·283	30·019	29·65	30·10	30·28	42·4	43·0 42·0	42	35	42·5	44½	38	N.E.	calm	39
17.	30·282	30·300	30·279	29·90	30·26	30·26	39·8	40·6 37·3	47	39	38	51½	30	N.	calm	29
18.	30·174	30·218	30·143	29·75	30·20	30·20	41·7	45·7 39·2	46	39	45	50	29	NNW.	var.	38
19.	30·198	30·333	30·202	29·83	30·23	30·25	41·3	42·0 40·0	49	29	42	49	38½	N.	N.	37
20.	30·318	30·399	30·319	29·87	30·27	30·37	41·3	41·8 36·4	52	29	42	48½	30	WNW.	calm	37
21.	30·412	30·443	30·432	30·3	30·43	30·32	37·7	39·3 32·6	46	25	34	44	28	NW.	N.	31
22.	30·310	30·346	30·154	29·86	30·16	30·00	39·7	40·6 32·0	50	34	40	46	36	NW.	calm	33
23.	29·998	30·202	30·032	29·60	30·12	30·25	39·0	46·4 37·4	50	30	37	43	30	NW.	calm	37
24.	30·152	30·235	30·175	29·80	30·27	30·33	36·7	38·2 33·0	43	30	34	38	32	N. var.	N.	32
25.	30·298	30·384	30·317	30·	30·34	30·33	37·4	41·0 32·0	44	26	36·5	42½	30½	NNW.	N.	29
26.	30·324	30·362	30·237	29·93	30·32	30·22	35·3	40·3 30·9	40	31	35	45	30	NW.	calm	30
27.	30·124	30·165	30·150	29·84	30·20	30·18	37·4	38·2 34·0	41	34	38·5	46	35	N.	N.	32
28.	30·116	30·155	29·957	29·78	30·05	29·87	38·7	39·5 35·8	42	36	38	50	33½	NW.	calm	32
29.	29·788	29·858	29·827	29·40	29·67	29·66	39·5	42·0 38·2	50	34	42	55	36	W.	calm	33
30.	29·894	29·930	29·892	29·36	29·73	29·70	46·7	48·3 38·2	54	43	44	44	42½	S.	calm	36
31.	29·840	29·866	29·823	29·43	29·61	29·55	46·8	47·6 45·2	47	42	47	49½	40	NNW.	calm	40
Mean.	30·248	30·315	30·231	29·86	30·217	30·208	39·7	41·7 35·9	44·87	31·35	39·0	46·7	32·7			Sum. 2·15	·70	·15	Mean. 33·5

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

J U N E 1840.

LXVI. *On crystallized Native Oxalate of Lime.* By H. J. BROOKE, Esq., F.R.S.*

I HAVE had in my possession for some months a specimen of calcite† in compound crystals of the metastatic form of Haiiy, from $\frac{1}{2}$ to $\frac{2}{3}$ of an inch long, with irregular and curved surfaces occasioned by the intersections of the numerous individuals of which the crystals are severally composed, and which stand on a rather soft light gray matrix. It is uncertain from whence the specimen was brought, but Mr. Heuland supposes it to have come from Hungary.

On the crystals of calcite are deposited a few small crystals, from $\frac{1}{10}$ th to $\frac{1}{4}$ th of an inch long, of oxalate of lime, which had from their high lustre been supposed to be some ore of lead. It was, however, apparent that they differed in form from all the known lead ores, and on examination by Mr. Sandall at Mr. R. Phillips's laboratory, they were found to be oxalate of lime with one proportional of water.

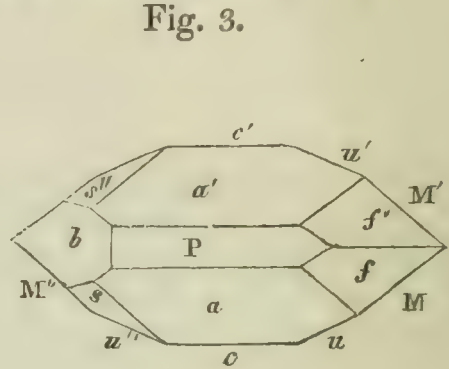
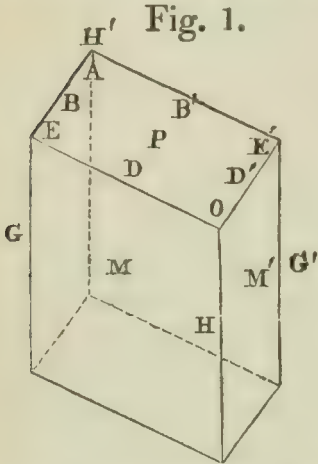
The crystals appear to have been formed contemporaneously with those of the calcite, in which some of them are partly imbedded, a circumstance which excludes the supposition of their being of vegetable origin; and as only one other oxalate, that of iron, is known to exist in the mineral state, and as it occurs in a bed of wood coal, and the oxalic acid contained in it may be presumed to have been derived from vegetable matter, this oxalate of lime will afford the first instance of the occurrence of oxalic acid as a distinct mineral product.

The primary form of the crystals is an oblique rhombic prism, fig. 1. P on M measuring $103^{\circ} 14'$ and M on M' $100^{\circ} 36'$: a cleavage parallel to P has induced me to adopt

* Communicated by the Author.

† In a list of minerals published a few years since, I proposed this name for the common rhombohedral carbonate of lime; calcite and aragonite thus denoting the two varieties of this substance.

that as one of the primary planes, and there are other cleavages parallel to M , and to c of fig. 2. Most of the crystals are twins, and remarkably symmetrical in their form. Fig. 3 is a projection of the terminal planes; and fig. 4, a projection of the twin crystal in profile, the relation of which to fig. 2, and the other figures, is rendered sufficiently obvious by the corresponding letters. All the planes are bright and perfect except M , which is striated by its alternations with u ; and f , which is also striated parallel to the edge between f and f' .



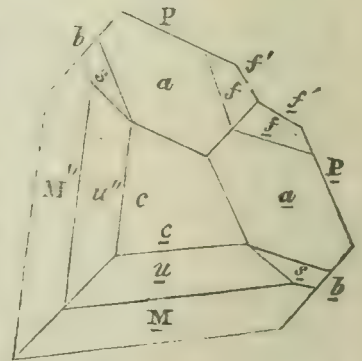
The following are the laws and measurements of the planes, for taking the trouble of calculating which I am indebted to the kindness of my friend Professor Miller of Cambridge.

Fig. 2.



a is B1 D1 G1
 b is B1 B'1 H'1
 c is B1 D1 G1
 f is B1 D ∞ G1
 s is B2 D1 G2
 u is B3 D1 G ∞

Fig. 4.



P on M = $103^{\circ} 14'$

a = 127 25

c = 90

b = 109 28

s = 136 48

f = 143 4

a on s = 154 19

f = 143 18

c = 142 36

b = 101 41

M on M' = $100^{\circ} 36'$

u = 160 45

c = 129 42

a = 111 37

b' = 128 4

s' = 136 48

f = 142 15

Very brittle. Fracture conchoidal. Hardness rather less than calcite. Spec. grav. 1.833.

Colourless. Transparent to opaque. Lustre similar to that of sulphate of lead.

H. J. B.

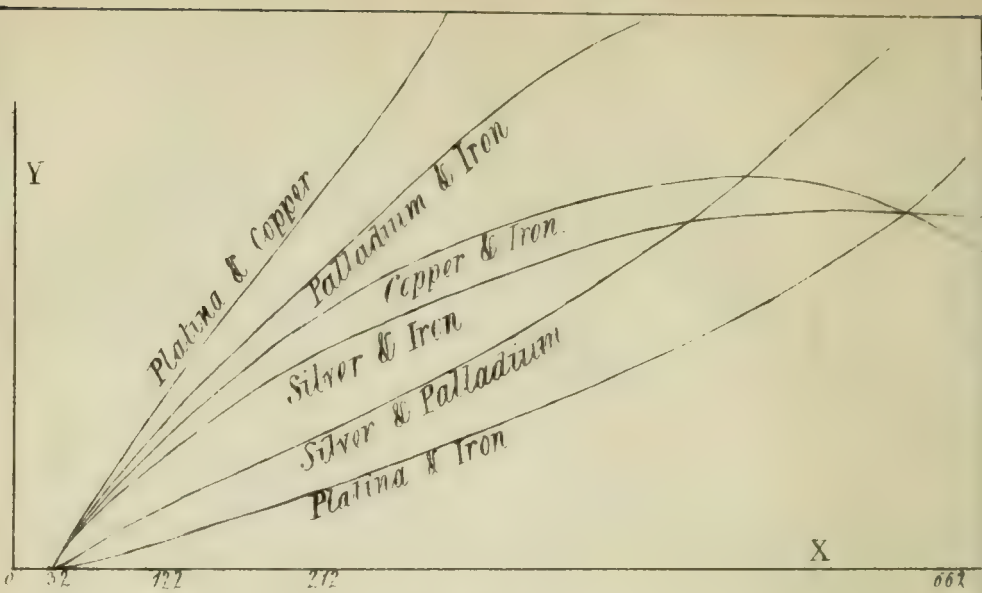


Fig. 2.

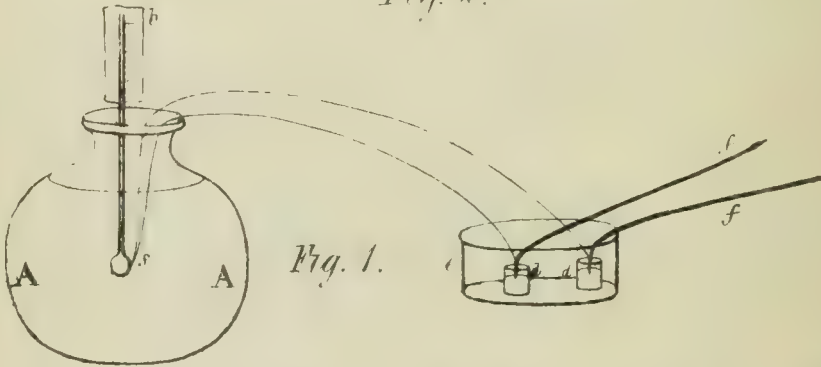


Fig. 1.

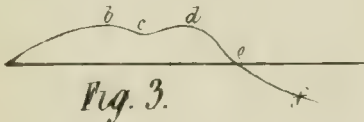


Fig. 3.

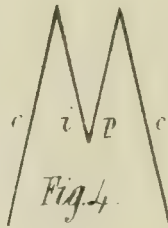


Fig. 4.

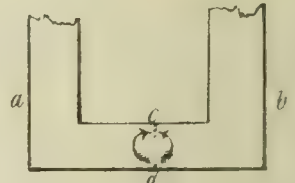


Fig. 5.

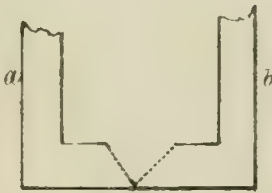


Fig. 6.



Fig. 7.

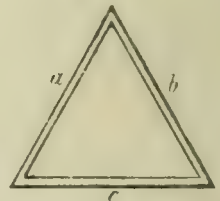


Fig. 8.

J. Basire lith.

LXVII. *On the Electro-motive Power of Heat.* By JOHN W. DRAPER, M.D., *Professor of Chemistry in the University of New York.**

[Illustrated by Plate VI.]

FROM the memoir of M. Melloni, on the Polarization of Heat, inserted in the second part of the first volume of the Scientific Memoirs, we learn, that M. Becquerel, as well as himself, has made experiments to determine the quantities of electricity set in motion by known increments of heat. From these experiments they conclude, that through the whole range of the thermometric scale, those quantities are directly proportional to each other.

But as thermo-electric currents are now employed in a variety of delicate physical investigations, and as there appears to be much misconception as to their character, I propose in this memoir to show,

1st. That equal increments of heat do not set in motion equal quantities of electricity.

2ndly. That the tension undergoes a slight increase with increase of temperature, a phænomenon due to the increased resistance to conduction of metals, when their temperature rises.

3rdly. That the quantity of electricity evolved at any given temperature, is independent of the amount of heated surface; a mere point being just as efficacious as an indefinitely extended surface.

4thly. That the quantities of electricity evolved in a pile of pairs, are directly proportional to the number of the elements.

First, then, as to the comparative march of electric development, with the rise of temperature, in the case of pairs of different metals.

The experimental arrangement which I have employed, is represented in fig. 1. (Plate VI.) A A is a glass vessel, about three inches in diameter, with a wide neck, through which can be inserted a mercurial thermometer *b*, and one extremity of a pair of electro-motoric wires. The wires I have employed have generally been a foot long, and $\frac{1}{16}$ th of an inch in diameter. The extremity *s* of the wires thus introduced into the vessel, ought to be soldered with hard solder: their free extremities dip into the glass cups *d d*, filled with mercury, and immersed in a trough *e* containing water and pounded ice. By means of the copper wires *f f*, $\frac{1}{6}$ th of an inch thick, communication is established with the mercury

* Communicated by the Author.

cups of the galvanometer. The coil of this galvanometer is of copper wire $\frac{1}{8}$ th of an inch thick, and making twelve turns only, round the needles, which are astatic. The deviations were determined by the torsion of a glass thread, in the way described in the number of this Journal for October 1839.

It is surprising to those who have never before seen the experiment, with what promptitude and accuracy a copper and iron wire, soldered thus together, will indicate temperatures.

In the arrangement now described, when an experiment has to be made, the vessel A A is to be filled two-thirds full of water, the bulb of the thermometer being so adjusted as to be in the middle of the vessel, and the soldered extremity *s* of the two wires, being placed in contact* with it, and a small cover with suitable apertures adjusted on the top of the vessel, so that the steam as it is generated may rush up alongside of the tube of the thermometer, and bring the mercurial column in it to an uniform temperature. The communicating wires *ff*, are then placed in the cups, and the trough *e* filled with water and pounded ice, and carefully surrounded with a flannel cloth. The water in the vessel A A, is then gradually raised to the boiling point by means of a spirit-lamp, and kept at that temperature until the galvanometer needles and the thermometer are quite steady. The same plan must be followed, when any other temperature than 212 is under trial, for the thermo-electric wires changing their temperature more rapidly than the mercury in the thermometer, it is absolutely necessary to continue the experiment for some minutes, to bring both to the same state of equilibrium.

When a temperature higher than 212° Fahr., but under a red heat, is required, I substitute in place of the vessel A A, a tubulated retort, the tubulure of which is large enough to allow the passage of the bulb of the thermometer and the wires. A quantity of mercury, sufficient to fill the retort half full, is then introduced, and the tubulure, being closed by appropriate pieces of soapstone, the neck of the retort is inclined upwards, so that the vapour as it rises may condense and drop back again, without incommoding the operator. As in the former case, it is here also necessary to continue each experiment for a few minutes, to bring the thermometer and thermal pair to the same condition. There is not much difficulty in obtaining any required temperature, by raising or lowering the wick of the lamp.

* If the extremity of the thermo-electric pair be allowed to rest on the bottom of the glass vessel, no accurate results can be obtained; the pair does not then indicate the temperature of the water.

The metals I have tried were in the form of wires. They were in the state found in commerce, and therefore not pure; they were obtained in the shops of Philadelphia.

TABLE I.

Names of the pairs of Metals.	Temperatures (Fahr.)			
	32 F.	122 F.	212 F.	662 F.
Copper and iron.....	0	93	176	233
Silver and palladium...	0	65	147	613
Iron and palladium	0	112	223	631
Platina and copper	0	11	26	122
Iron and silver	0	89	137	244
Iron and platina.	0	28	56	248

Quantities of Electricity.

In this table I have estimated the temperature of boiling mercury at 662° Fahr. The quantities of electricity evolved, as estimated by the torsion of a glass thread, are ranged in columns under their corresponding temperatures. Each series of numbers is the mean of four trials, the differences of which were often imperceptible, and hardly ever amounted to more than one degree.

Now if this table be constructed, the temperatures being ranged along the axis of abscissas, and the quantities of electricity being represented by corresponding ordinates, we shall have results similar to those given in fig. 2, in which it is to be observed, that the curves given by the systems of silver and iron, copper and iron, and palladium and iron, are concave to the axis of abscissas; but those given by platina and copper, silver and palladium, and platina and iron, are convex.

Let us now apply the numbers obtained by these several pairs, for the calculation of temperatures, which will set their action in a more striking point of view. The following table contains such a calculation, on the supposition that for the 90 degrees from 32° Fahr. to 122° Fahr., the increments of electricity are proportional to the temperatures.

TABLE II.

		Temperatures by the Mercurial Thermometer.			
		32 F.	122 F.	Water boils.	Mercury boils.
Temperature by a pair of	Copper and iron.....	32	122	202	257
	Silver and palladium	32	122	235	880
	Iron and palladium ..	32	122	211	539
	Platina and copper...	32	122	244	1030
	Iron and silver.....	32	122	170	279
	Iron and platina	32	122	212	829

We are therefore led to the general conclusion, that in *these six different systems of metals, the developments of electricity do not increase proportionally with the temperatures, but in some with greater rapidity, and in others with less.*

The results here given, I have corroborated in a variety of ways and with a variety of wires. A pair consisting of copper and platina, gave for the temperature of tin when in the act of congealing 452° Fahr. instead of 442° Fahr. the point usually taken. For the melting point of lead, it gave $942\frac{1}{2}^{\circ}$ Fahr., instead of 612° Fahr. The melting points of tin, lead, zinc, and occasionally of antimony and bismuth, were in this manner employed, for they allow time for the working of the torsion balance, and with the exception of bismuth, their temperature appears to be steady all the while they are in a granular condition, before they finally solidify. The action of these metals on the thermo-electric pair is easily prevented by dipping it into a cream of pipe-clay.

A pair of copper and platina gave for a dull red heat 1416° Fahr., and for a bright red 2103° Fahr.

A pair of palladium and platina gave for a dull red 1850° Fahr., and for a bright red 2923° Fahr.

Some of the combinations into which iron enters as an element, give rise to remarkable results; thus if we project the curve given by a system of copper and iron, we shall find it resembling fig. 3, where the maximum ordinate b occurs at a temperature of about 650° Fahr.; the point c appears to be given between 700 and 800 degrees; d by a dull red heat; e is very nearly the point at which an alloy of equal parts of brass and silver melts, for if the pair be soldered with this substance, it fuses when the needles have returned almost exactly to the zero point. With harder solders or with wires simply twisted, the curve may be traced on the opposite side of the axis towards f , its ordinate increasing with regularity. At 60° Fahr., taking the length of the ordinate corresponding to a temperature of 212° Fahr. as unity, the length of the maximum ordinate at b , is 1.85 very nearly.

A system of silver and iron gives also a similar curve, the point b occurring at a temperature rather higher than the analogous one for the preceding system, but still below the boiling point of mercury.

Now all these things serve to show, that we cannot determine with accuracy unknown temperatures by the aid of thermo-electric currents, on the supposition that the increments of the quantities of electricity are proportional to the

increments of temperature throughout the range of the mercurial thermometer.

Let us now proceed to the second proposition, "That the tension undergoes a slight increase with increase of temperature, a phenomenon due to the increased resistance to conduction of metals when their temperature rises."

It will be seen, on consulting the following table, that pairs of *different metals*, at the same temperature, have tensions which are apparently very different.

The currents, the tensions of which are here indicated, were generated by keeping one end of the thermal pair in boiling water, the other ends being maintained at a temperature of 32° Fahr.

TABLE III.

A pair of	Tension.	A pair of	Tension.
Antimony and bismuth	·137	Platina and iron.....	·470
Copper and iron	·183	Copper and platina	·473
Silver and lead	·307	Platina and palladium	·500
Lead and palladium...	·313	Tin and iron	·518
Silver and platina.....	·380	Platina and tin	·567

We perceive, therefore, that there apparently exist specific differences in the qualities of electric currents derived from different sources. If, for example, we take a pair of platina and palladium, and expose it to a temperature which shall generate a current capable of deflecting the torsion balance through 1000 degrees, and then abstract it by a wire of such dimensions as to stop one half, or only allow 500 degrees to pass, and repeat the experiment with a current generated by bismuth and antimony, the temperature being still so adjusted as to give a deflection of 1000 degrees, on making this pass through the same intercepting wire, perhaps not much more than one eighth of it will go through the galvanometer.

It might be supposed that these characteristic differences of thermal currents, derived from different sources, were due to some modification of the electricity itself, similar to those of radiant heat, derived from different sources, or at different temperatures, which M. Melloni has attempted to show are analogous to the colours of light, being like them of different degrees of refrangibility, and permeating absorbent media with different degrees of facility. For in the same way that we regard glass as transparent to light, and rock salt as transparent to heat, so too we might regard a copper wire or any conducting medium as transparent to electricity.

But this peculiarity of thermo-electric currents depends on the conducting resistance of the system that generates them. It is possible to give a current a higher or a lower tension, by simply making use of thin or thick wires to generate or to carry it. In the foregoing table the current from platina and palladium had a high tension, because slender wires of those metals happened to be used to generate it; and the current from antimony and bismuth had a low tension, because thick bars of those substances were employed. In the former case, the conducting resistance was greater than in the latter, and hence the tension of the current was higher.

That this is strictly true, will appear on examining the current evolved by any number of systems, under the same condition of resistance to conduction. I took a pair of copper and iron, and soldered it to a similar pair of platina and copper, as is shown in fig. 4, so as to form one continuous metallic line. The point of junction formed by the wires *i* (iron) and *p* (platina), was kept carefully at 63° Fahr., by immersion in a water-bath: the point of junction *p* (platina) and *c* (copper) was treated in like manner; but that of *e* and *i* was raised to 212° Fahr. Under these circumstances, it was found that 181 degrees of electricity were evolved, of which 50 went through a given secondary wire. Then raising the junction *p* and *c* to 212° Fahr., and bringing *e* and *i* to 63° Fahr., there passed at the galvanometer 71 degrees, of which 19 could traverse the same secondary wire, but

$$\text{As } 181 : 50 :: 71 : 19\cdot6$$

and hence I infer, that where the conducting resistance is the same, the tension of currents from different sources does not differ.

These results inform us how much the tension of a current depends on the resistance to conduction of the system which it traverses, as well as on the dimensions of the system itself; an observation, the value of which we shall presently see.

In a great number of trials which I made, I failed in getting any trustworthy results, as respects tension of currents at high temperatures, on account of the difficulty of maintaining the thermo-electric pair at the same degree without variation. By employing, however, a small black-lead furnace, to which was adapted a covered sand-bath, into which the wires could be plunged, I succeeded at last; for with this arrangement a regulated temperature could be kept up for a length of time.

The experiment was made with care in the case of two systems of metals: 1st, copper and platina; 2nd, copper and iron.

1st. At the boiling point of water, a pair of copper and platina, the unexcited extremity of which was carefully maintained at 67° Fahr., evolved as a mean of four trials, three of which were absolutely identical, 123 degrees of electricity, of which 23 could pass a secondary wire.

Then, by the aid of the furnace and sand-bath, the temperature was raised until the pair evolved 783 degrees, as a mean of four trials; of these 163 could pass the secondary wire. Now,

As $783 : 163 :: 123 : 25\frac{1}{2}$ instead of 23
showing therefore a slight rise of tension.

2nd. The pair of *copper* and *iron* gave at the boiling point of water 300 degrees, of which 57 passed the secondary wire. The temperature was now raised, with the following results:

490 degrees passing the primary, 95 the secondary wire.

553	—	—	—	113	—	—
545	—	—	—	112	—	—
493	—	—	—	110	—	—

It will be understood, that although the quantities of electricity indicated in the first column do not regularly increase, that the temperatures were notwithstanding going regularly upwards: to this peculiarity of the systems into which iron enters I have already alluded. Let us now compare these measures with those obtained for the boiling point of water:

As $490 : 95 :: 300 : 58$ instead of 57.

$553 : 113 :: 300 : 61$ —

$545 : 112 :: 300 : 61$ —

$493 : 110 :: 300 : 67$ —

we find, therefore, that in the case of both these systems of metals, the tension slowly rises with increase of temperature, being much better marked in the latter than in the former instance.

The increase of tension here detected, depends unquestionably on increased resistance to conduction, which the wires exhibit as their temperature rises, as the following experiments show.

A pair of copper and iron evolved a current at the boiling point of water, which passing through a wire of copper eight feet long, was determined at the galvanometer to be 176 degrees. Having twisted a part of this wire into a spiral, so as to go over the flame of a spirit-lamp, 8 inches of it were thereby brought to a red heat; the deviation of the needle fell now to 165, being a deficit of 11 degrees. In this experiment, care was taken that no heat should be transmitted along the wire to the connecting cups.

The same was repeated with a piece of iron wire, of the same length and under the same circumstances. The current at first being 90 degrees, as soon as the spiral was made red hot, it fell to 61 degrees, being a deficit therefore of nearly one third the whole amount.

To the increased resistance to conduction, occasioned by an increased temperature, we are to impute the slight rise of tension observed in thermo-electric currents. The quantities are of the same order.

We have next to show, "that the quantity of electricity evolved at any given temperature, is independent of the amount of heated surface; a mere point being just as efficacious as an indefinitely extended surface."

The quantities of electricity evolved by hydroelectric pairs has been shown to increase with their surfaces, but it is not so in thermo-electric arrangements. A pair of disks of copper and iron, two inches in diameter, were soldered together; they had continuous straps projecting from them, which served to connect them with the galvanometer cups. At the boiling point of water they gave 62 degrees; on being cut down to half an inch in diameter, they still gave 62. On the disk being entirely removed, and the copper made to touch the iron by a mere point, its extremity being roughly sharpened, the deflection was still 62.

By means of a common deflecting multiplier, I obtained the following results; 1st, a copper wire being placed in a bath of mercury, the temperature of which was 240° Fahr., I dipped into it a second copper wire, the temperature of which was about 60° Fahr.; the galvanometer needles moved through 15 degrees.

2nd. The cold wire being sharpened to a point, and plunged deliberately into the mercury to the bottom of the bath, the deflection was 19 degrees.

3rd. But when I touched the surface of the mercury with the *very point* of the cold wire, there was a deflection of 60 degrees.

Having laid a plate of tinned iron upon the surface of some hot mercury, it was touched with the point of the cold wire. There was a strong deflection of the needles in the opposite direction to what would have been the case had the mercury been touched and not the iron. The under surface of the iron was therefore acting as a hot face, and the parts round the point as a cold face, being temporarily chilled by the touch of the wire.

These results explain the anomalies observed by some of those who investigated the course of thermo-electric currents by means of small metallic fragments.

It would therefore seem, that when wires of the same metal are used as electromotors, the quantity of electricity evolved depends on the quantity of caloric that can be communicated in a given time. Time, therefore, under these circumstances must enter as an element of thermo-electric action. In the case of a single metal, the maximum effect would be produced, when the hot element is a mass, and the cold one a point.

And lastly, "That the quantities of electricity evolved in a pile of pairs are directly proportional to the number of the elements."

In the first trials which I made, to determine the effect of increasing the number of pairs in a pile, the results obtained were contradictory; by operating however in the following way, the proposition was at last satisfactorily determined.

1st. The resistance to conduction was made nearly constant by uniting all the pairs intended to be worked with, at once. The current, therefore, whether generated by one, two, three, four, or more pairs, had always to run through the same length of wire, and experienced in all cases an uniform resistance.

2ndly. By making each individual element of considerable length, the liability of transmission from the hot to the cold extremity was diminished.

Having, therefore, taken six pairs of copper and iron wires, $\frac{1}{16}$ th of an inch thick and each element 38 inches long, I formed them, by soldering their alternate ends, into a continuous battery. Then I successively immersed in boiling water one, two, three, &c. of the extremities, their length allowing freedom of motion, and the other extremities not differing perceptibly from the temperature of the room.

The following table exhibits one of this series of experiments.

TABLE IV.

No. of pairs.	Calculated Deviations.	Observed Deviations.
1.	55	55
2.	110	111
3.	165	165
4.	220	220
5.	275	272
6.	330	332

Hence there cannot be any doubt, that the quantities of electricity evolved by compound batteries, at the same temperature, are directly proportional to the number of the pairs.

With some general remarks, arising from the foregoing subjects, I shall conclude this communication.

1. It is of importance to remember, that thermo-electric currents traverse metallic masses only on account of differences of temperature existing at different points.

2. When a current of electricity, flowing from the poles of a battery, is made to traverse a metallic sheet, the whole of it does not pass in a straight line from one pole to the other, but diffuses itself through the metal, diverging from one point and converging to the other. The greater part of the current is found, however, to take the shortest route.

3. Combining therefore the foregoing observations (1.2), we perceive, that there are certain forms of construction which will give to thermo-electric arrangements peculiar advantages. For example, the surfaces united by soldering must not be too massive. Let *a*, fig. 5, be a bar of antimony, and *b* a bar of bismuth; let them be soldered together along the line *cd*, and at the point *d* let the temperature be raised; a current is immediately excited; but this does not pass around the bars *a*, *b*, in as much as it finds a shorter and readier channel through the metals, between *c* and *d*, circulating therefore as indicated by the arrows. Nor will the whole current pass round the bars, until the temperature of the soldered surface has become uniform.

An obvious improvement in such a combination is shown in fig. 6, which consists of the former arrangement, cut out along the dotted lines: here the whole current so soon as it exists is forced to pass along the bars. And because the mass of metal has been diminished at the line of junction, such a pair will change its temperature very quickly.

One of the very best forms for a thermo-electric couple is given in fig. 7, where *a* is a semicylindrical bar of antimony, *b* one of bismuth, united together by the opposite corners of a lozenge-shaped piece of copper *c*. From its exposing so much surface, the copper becomes hot and cold with the greatest promptitude, and from its good conducting power it may be made very thin without injury to the current. With a pair of bars $\frac{5}{4}$ ths of an inch thick, and a circular copper plate *c*, having both surfaces blackened, I have repeated the greater part of those experiments which M. Melloni made with his multiplier.

4. The currents which circulate in a steel magnet are to

all appearance perpetual. I thought for some time it might be possible to procure similar perpetual currents, by compound thermo-electric arrangements. Fig. 8 will serve to show the character of these combinations, and also the cause of their failure. Let a, b, c , be wires of three different metals soldered together so as to form a triangle. Now if these metals were selected, so that a and b could form a more powerful thermo-electric pair than a and c , or b and c , it might be expected that at all temperatures an incessant current would run round the system. Such, however, will not be found to be the case. In effect, any one of these three serves simply as a connecting solder to the other two, and hence no current is excited; for the ends that have the third metal between them, although that metal intervenes, are under exactly the same condition as the other ends which are in contact.

5. Thermo-electric currents, evolved by pairs of different metals, do not appear to differ specifically. As different gases during combustion burn with differently-coloured flames, and as different sources of caloric evolve rays of heat which are absorbed differently by different media, it might be expected that a pair of wires of copper and platina would give out a current of electricity unlike that of iron and palladium. I have made many trials on this point, adjusting a wire of copper and one of lead to each other, so as to stop equal quantities of electricity flowing from a pair of copper and platina, the galvanometer needles being brought to the same point, whether the long wire of copper or the short wire of lead was employed. But, in the case of every combination which I tried, these two wires acted alike, nor could I ever evolve a current which would pass with more or less absorption along the lead than along the copper.

LXVIII. *Illustrations of the History of the Inductive Sciences.*

No. I. *The Reception of the Copernican Theory in England.*

By J. O. HALLIWELL, Esq., F.R.S., F.R.A.S., F.S.A., M.R.S.L., &c. of Jesus College, Cambridge.*

THE most zealous reader would not require, nor the most enthusiastic inquirer expect, to find the results of new and deep antiquarian research in so comprehensive a work as that by Professor Whewell on the History of the Inductive Sciences; much less would any one for a moment consider the credit of that eminently distinguished author in any de-

* Communicated by the Author.

gree shaken by the discovery of documents tending to alter any of his conclusions. All will agree, that with the materials before him, Professor Whewell has performed his task most admirably, for it would have been the labour of a hundred lives to have carefully examined every available document connected with the facts there brought together. Anxious to place my mite towards the discovery of truth in a secure position, I intend, with the permission of the Editors of the Philosophical Magazine, to commence a series of papers on some portions of the history of the inductive sciences which do not appear to have been as yet thoroughly investigated.

As regards the history of science in our own country, Professor Whewell has laboured under very great disadvantages. Thrown almost entirely on continental writers for his information, he has overlooked many important works of the early English authors. For instance, the *Quæstiones Naturales** of Athelard of Bath, and the *Dialogus de philosophia*† of Gulielmus de Conchis, ought to have been analysed as grand landmarks in the period preceding the splendid epoch of Roger Bacon; and the contents of the present paper will prove how neglected the claims of the English writers have been in their early reception of one of the greatest advances ever made in natural knowledge.

Prejudice in favour of the authority of Aristotle, even in the sixteenth century the master of all the Universities, forbade any public belief in the Copernican system, and on that account we find many compilers of astronomical tables grounding them upon the new system, without professing, and sometimes denying, a belief in the heliocentric doctrine on which they were founded. Copernicus himself says, “neque enim necesse est eas hypotheses esse veras, imo ne verisimiles quidem; sed sufficit hoc unum, si calculum observationibus congruentem exhibeant:” so dangerous was it to invade the established belief. Astrologers then received more encouragement than those skilled in real science; a rascal of the name of Nicholas Kratzer, being the chosen “astronomyer” of Queen Mary, at the same time that Robert Recorde was her physician. Kratzer was the author of a little volume on astronomy, in which he even denies the rotundity of the earth:

“ He saw with his own eyes the moon was round,
Was also certain that the earth was square,
Because he had journey’d fifty miles and found
No sign that it was circular any where.”

* MS. Cotton. Galba, E. iv. Mus. Brit.

† MS. Arundel. Mus. Brit. 377.

And his work is not a solitary specimen. In this state of popular ignorance it speaks volumes for the genius of Britain to be able to prove, that at least as early as the year 1556, our country rejoiced in three minds perfectly convinced of the truth of the new system and able to appreciate its value.

To Professor De Morgan we are indebted for having discovered in the *Castle of Knowledge* (fol. Lond. 1556) by Robert Recorde, a passage showing that the author was in heart a Copernican*; its extreme curiosity will well permit a repetition in modern orthography:—

“*Master.* But as for the stability of the earth, I need not waste any time in proving it, because that opinion is so firmly fixed in most men’s heads, that they think it mere madness to bring the question in doubt. And therefore it is as much folly to labour to prove that which no man denieth, as it were with great study to dissuade that thing which no man covets nor any man grants; or to blame that which no man praises nor any man likes.

“*Scholar.* Yet some time it chanches, that the opinion most generally received is not most true.

“*Master.* And so do some men judge of this matter; for not only Eraclides Ponticus, a great philosopher, and two great scholars of the school of Pythagoras, Philolaus and Ecphantus, were of the contrary opinion, but also Nicias Syracusius and Aristarchus Samius seem with strong arguments to approve it; but the reasons are too difficult for this first introduction, and therefore I will omit them till another time. And so will I do the reasons that Ptolemy, Theon, and others do allege, to prove the earth to be without motion; and the rather because those reasons do not proceed so demonstrably, but they may be answered fully of him that holdeth the contrary. I mean, concerning circular motion: any direct motion out of the centre of the world seemeth more easy to be confuted, and that by the same reasons which were before alleged for proving the earth to be in the middle and centre of the world.

“*Scholar.* I perceive it well: for as if the earth were always out of the centre of the world, those former absurdities would at all times appear: so that if at any time the earth should move out of its place, those inconveniences would then appear.

“*Master.* That is truly to be gathered: howbeit Copernicus, a man of great learning, of much experience, and of wonderful diligence in observation, has renewed the opinion

of Aristarchus Samius, and affirmeth that the earth not only moveth circularly about its own centre, but also may be, yea and is, continually out of the precise centre thirty-eight thousand miles : but because the understanding of that controversy depends upon deeper knowledge than in this introduction may be uttered conveniently, I will let it pass till some other time.

“*Scholar*. Nay, sir, in good faith, I desire not to hear such vain phantasies, so far against common reason, and repugnant to the consent of all the learned multitude of writers, and therefore let it pass for ever and a day longer.

“*Master*. You are too young to be a good judge in so great a matter : it passeth far your learning, and theirs also that are much better learned than you, to disprove his supposition by good arguments, and therefore you were best to condemn nothing that you do not well understand ; but another time, as I said, I will so declare his supposition, that you shall not only wonder to hear it, but also peradventure be as earnest then to credit it, as you are now to condemn it.”

Who will not regret to learn that such a writer as Robert Recorde died in Newgate? the Newton of the sixteenth century perished in a jail !

In a communication to the Royal Astronomical Society, the Rev. Joseph Hunter was the first who noticed the Ephemeris for 1557, by John Feild, “juxta Copernici et Reinholdi canones,” in the preface to which he avows his conviction of the truth of the Copernican theory. Now there is no precise date to Recorde’s “Castle of Knowledge,” other than the year of its publication ; and Feild’s preface is dated on the calends of June 1556 : there is also prefixed to Feild’s work a letter by Dr. John Dee, dated the 3rd of July 1556, in which he also avows his belief in the new system. But neither Feild nor Dee speak of their concurrence with the Copernican theory as anything new, and it is therefore quite impossible to say who of these three properly claims the priority. We may then safely award to JOHN DEE, JOHN FEILD, and ROBERT RECORDE, the high distinction of being the first who adopted the Copernican system, or as Mr. Hunter would call them, the PROTO-COPERNICANS OF ENGLAND.

We do not find, however, many other early supporters of this theory in England. The elder Digges certainly was not, although after his death his son professed his belief in it*. The edition of the “Prognostication Everlasting,” published

* See three papers by me in the Magazine of Popular Science, vol. iii. and iv.

in 1578, contains an appendix by Thomas Digges, entitled "A perfitte description of the Celestiall Orbes according to the most auncient doctrine of the Pythagoreans, lately revived by Copernicus, and by Geometricall Demonstrations approved." This appendix opens with the following powerful and well-written passage, which we cannot resist giving at full length :—

" Having of late (gentle reader) corrected and reformed sondry faultes that by negligence in printing have crept into my father's Generall Prognostication : Amonge other thinges I founde a description or modill of the world and situation of spheres cœlestiall and elementare according to the doctrine of Ptolome, whereunto all Universities (ledde therto chiefly by the aucturity of Aristotle) sithens have consented. But in this our age one rare witte (seeing the continuall errors that from time to time more and more have bin discovered, besides the infinite absurdities in their theorickes, which they have bin forced to admit that woulde not confesse any mobilitie in the ball of the earth) hath by long studie, painfull practise, and rare invention delivered a new theorick or model of the world, shewing that the Earth resteth not in the centre of the whole world, but onely in the center of this our mortall world or globe of Elementes which environed and enclosed in the moones orbe, and together with the whole globe of mortalitie is caried yearely rounde aboute the Sunne, which like a king in the midst of all raigneth and geeveth lawes of motion to the rest, sphærically dispearsing his glorious beames of light through al this sacred cœlestiall temple. And the earth itselfe to be one of the planets having his peculiar and strayinge courses tourning everye 24 houres rounde upon his owne center whereby the sunne and great globe of fixed starres seeme to sway about and tourne, albeit indeede they remaine fixed. So many wayes is the sense of mortall men abused, but reason and deepe discourse of witte having opened these things to Copernicus, and the same being with demonstrations mathematicall most apparently by him to the world delivered, I thought it convenient together with the olde theorick also to publish this, to the ende such noble English minds (as delight to reache above the baser sort of men) might not be altogether defrauded of so noble a part of philosophy. And to the ende it might manifestly appeare that Copernicus mente not as some have fondly excused him to deliver these grounds of the earthes mobility onely as mathematicall principles, fayned and not as philosophicall trully averred, I have also from him delivered both the philosophicall reasons by

Aristotle and others produced to maintaine the earthes stability, and also their solutions and insufficiency, wherein I cannot a little commend the modestie of that grave philosopher Aristotle, who seeing (no doubt) the insufficiency of his owne reasons in seeking to confute the earthes motion, useth these words:—*De his explicatum est ea qua potuimus facultate*. Howbeit his disciples have not with like sobriety maintayned the same. Thus much for my owne parte in this case I will onely say there is no doubt but of a true grounde truer effects may be produced than of principles that are false, and of true principles falshod or absurditie cannot be inferred. If therefore the earth be situate immoveable in the center of the worlde, why finde we not theoricke upon that grounde to produce effects as true and certain as these of Copernicus? Why cast we not away those *Circulos Æquantes*, and motions irregulare, seinge our owne philosopher Aristotle himselfe the light of our Universities hath taught us, *Simplicis corporis simplicem oportet esse motum*? But if contrary it be founde impossible (the earthes stability being graunted) but that we must necessarily fall into these absurdities, and cannot by any meane avoyde them, why shall we so much dote in the apparance of our sences, which many wayes may be abused, and not suffer ourselves to be directed by the rule of Reason, which the great God hath given us as a lampe to lighten the darcknes of our understandinge and the perfit guide to leade us to the golden braunche of veritie amide the forrest of errours?”

Robert Tanner, in his “*Mirroure for Mathematiques*,” published at London in the year 1587, makes no allusion to the Copernican theory: at fol. 37, he computes the number of miles that the *sun* has traversed since the creation at 43,688,316,000! Dee, in his later works, does not allude to his former opinion, and the next notice I find is in 1596, when John Blagrove of Reading again revived the theory in its full sway, in his *Astrolabium Uranicum Generale*.

Professor Whewell does not mention one English author of the *sixteenth* century as having adopted the heliocentric theory. He evidently fixes the year 1600 as the earliest period which received a glimpse of the new system, and gives Giordano Bremono, a twaddling Italian writer, the merit of having had “a considerable share in introducing the new opinions into England.” But however little effect the writings of Recorde, Dee, or Feilde may have had, yet the extreme popularity of Digges’s work places the conjecture out of the limits of probability. Moreover, there were doubtless several

Copernicans who never expressed their opinion in print, and it must be noted that Harriot had been styled a “profounde mathematician” as early as 1593*.

In Caxton’s “Ymage or Myroure of the Worlde,” printed in 1482, the world and its inhabitants are compared to a large apple surrounded with flies. I mention this, not that it has any relation to the subject in hand, but because I met with it while pursuing these researches, and because it ought always to be quoted in connexion with the most improbable and absurd tale of Sir Isaac Newton and the gravitation apple, which is gravely inserted in work after work. What a pity it is that no such tale has been invented to commemorate the discovery of Copernicus! for notwithstanding the prophecy of Rheinhold, his reputation requires it more than that of Newton:—“Tota posteritas grato animo Copernici nomen celebravit, cujus labore et studio, doctrina ipsa cœlestium motuum propemodum collapsa iterum restituta est: et magna ejus quoque lux, Dei beneficio accensa, inventis et patefactis ab eo multis, quæ ad hanc usque ætatem vel ignota vel obscura.”

35, Alfred Place, London, March 7, 1840.

LXIX. *On certain Modifications of the Powers of Heat and Light when transmitted through Glass.* By CHARLES T. COATHUPE, Esq.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

SHOULD the following observations “On certain modifications of the powers of heat and light when transmitted through glass” appear to you to be a pertinent sequel to some of the experiments of Professor Draper of New York, which are recorded in the London and Edinb. Phil. Magazine for February 1840, they are presented to you with the author’s best respects.

In a room about 16 feet square, having a window about 6 feet square in its eastern side, a small stove in the centre of its southern side, a door communicating with a passage at the northern end of the western side, and a blank wall constituting the north side, the following experiments were made.

(a.) A small 4-ounce stoppered bottle containing a fragment of camphor was placed upon a table near the centre of the window that occupied the middle portion of the eastern side of the room.

* Pierce’s Supererogation, by Gabriel Harvey, p. 190.

(b.) A similar bottle containing camphor was placed exactly opposite to the first bottle, upon a stand near the west wall (the stove being thus at equal distances from each bottle, and not in a line between them).

(c.) A third bottle containing camphor was placed in the centre of a line drawn between the door and the window, and exactly opposite to the stove, although at a distance of about 13 feet from it.

The temperature of the room varied at its extremes from 44° Fahr. to 54° .

After three days the camphor in the bottle in experiment (a.) had crystallized beautifully towards the east, \therefore towards the light.

That in bottle, experiment (b.) towards the west, \therefore opposite to the light.

That in bottle, experiment (c.) towards the east and west, \therefore towards the coolest places.

There was certainly rather more crystallization towards the east than towards the opposite side. A little also was apparent towards the north wall, opposite to the stove.

(d.) A clean bottle containing a fragment of camphor was now placed near the window, which was covered by two glass receivers, leaving air spaces between the bottle and the first and second receiver of about $\frac{5}{4}$ ths of an inch.

The camphor showed no such particular tendency to crystallize towards the east as it had in experiment (a.), but it crystallized nearly uniformly all over the upper concavity of the bottle.

(e.) Decanters containing water were next substituted for the camphor bottles, and in each position the dew was invariably deposited upon the same relative side of the decanter, as the camphor had crystallized when exposed in the stoppered bottles.

In one of our glass houses (for I am unfortunately connected with the manufacture of glass) there is a small apartment about 9 feet long, 6 feet wide, and $7\frac{1}{2}$ feet high, separated from the interior of the glass-house by a lath and plaster partition, which is hollow within, and comprehends a total thickness of $4\frac{1}{2}$ inches. This little apartment is occupied by a superintendent of the founder's crew. Opposite to a wide bench upon which this person reclines, there is a window through which he watches the progress of the furnace. This window is so constructed that one half of it can be slid behind the other half, to enable the "surveillant" to protrude his head at a moment's notice, to issue his orders. The window is glazed with glass of $\frac{1}{16}$ th of an inch in thickness. It is 24 feet from the nearest point of the furnace which

emits flame, and 38 feet from the most distant flame that can emanate from the same side of the furnace.

(*f.*) A thermometer suspended from a pin fixed into one of the window bars during the process of founding, and hanging at a distance of one inch from the glass, exterior to the apartment, usually indicates a temperature of from 180° to 190° Fahrenheit, when a thermometer suspended at precisely the same distance from the same pane of glass in the interior of the apartment indicates only a temperature of from 120° to 130° of Fahrenheit. A difference of distance of only two inches, when influenced by the interposition of a plate of glass only $\frac{1}{16}$ th of an inch thick, effects a difference of 60 degrees of temperature under the circumstances stated above.

(*g.*) Having suspended a thermometer in a flint glass bottle of 5 inches in diameter, and placed the stopper loosely *in situ*, I placed the bottle upon a non-conductor of heat (several layers of felt) just exterior to this window. The thermometer gradually rose, as might be expected, to 178 degrees of Fahrenheit. It was then placed upon the window frame, by sliding to one side the moveable part of the window until there was just sufficient space to admit the diameter of the bottle; another thermometer was suspended round the neck of the bottle, and adjusted so as to hang close to that surface of the bottle which presented to the interior of the apartment. The internal thermometer remained constant at 178 degrees, and the external thermometer, which faced the interior of the apartment, never indicated a higher temperature than 110 degrees of Fahrenheit.

(*h.*) A little moisture adhered to the interior of the bottle when thus introduced, and the dew became deposited upon that side which was most remote from the fire.

(*i.*) A bottle containing a piece of camphor was now substituted for that which contained the thermometer. In four minutes the side of the bottle which was most distant from the fire became beautifully spangled with crystals of camphor.

(*k.*) Cress seed which had been sown in a wooden trough supported beneath this window in the interior of the apartment, vegetated rapidly; but although it remained constantly exposed to a very brilliant fire light, its appearance was precisely similar to that which vegetated in the dark (*viz.* exhibiting white and attenuated stems, with pale yellow leaves). There was no tendency in the plants to grow towards the fire light.

I believe that whensoever dew is perceived upon one side only of a vessel containing water, the side upon which

the dew is deposited is *the coldest side*, and that *light* has no influence whatsoever upon such a deposition. My experiments with camphor tend to a similar inference with respect to the deposition of crystals upon the sides of vessels containing this substance. How that surface which is presented towards the sun (as in Professor Draper's experiments) becomes the coldest, is a paradoxical problem yet to elucidate.

(l) If paper which has been prepared for heliographic purposes by a solution of nitrate of silver be partially covered by a piece of crown window glass, and then exposed to the direct rays of the sun, that part of the paper which has been thus covered will be rendered darker after a few minutes' exposure, than that which has been equally exposed, but uncovered.

The experiments which I made during the last summer upon the effects of *solar light* upon paper prepared with nitrate of silver, indicated that the maximum depth of tint ensued from its transmission through ordinary unstained crown window glass; that the next tint in intensity was produced by the direct solar rays upon the uncovered, and perfectly exposed surface of the paper. The third in intensity, and almost equal to the second, was produced by the transmission of the sun's rays through glass of a "Waterloo blue" colour.

The fourth, through dark violet coloured glass.

The fifth, through purple do.

The sixth, through amber do.

The seventh, through brown yellow do.

The eighth, through dark green do.

The ninth, through light olive green do.

The tenth, through blood red do.

The eleventh, through crimson do.

The twelfth, through bright red do.

From the ninth to the twelfth there was no very remarkable effect produced by the solar rays after an hour and a half's exposure.

The crown glass, the blue, the violet, and the purple glasses produced deep tints, with which none produced from the other coloured glasses could be compared but as contrasts. The colours here mentioned were the only colours tried.

There are one or two other peculiarities about crown glass which deserve notice.

1st. Why should the colour of unannealed window glass be much lighter and brighter than that of similar glass when annealed?

2ndly. The specific gravity of a specimen of very pure doubly-terminated quartz at 64° Fahrenheit was 2.6577.

The spec. grav. of Isle of Wight sand at 64° Fahrenheit was 2.644. The spec. grav. of crown window glass (as made at Nailsea,) is 2.532. Its ingredients are sand, soda, and lime.

Now, if we leave the silex a constant quantity, how is it that any increase of lime, or of soda, increases the specific gravity of the resulting glass?

Any addition of alumina will produce the same effect.

In mentioning an increase of lime, or of soda, or of alumina, I mean of course, an increase beyond the usual proportions of each ingredient commonly employed.

I have the honour to remain,

Gentlemen, yours, &c.

Wraxall, near Bristol,
March 4, 1840.

CHARLES THORNTON COATHUPE.

LXX. *On the Mineral Structure of the South of Ireland, with correlative matter on Devon and Cornwall, Belgium, the Eifel, &c.* By THOMAS WEAVER, Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.

[Continued from p. 404, and concluded.]

POSTSCRIPT.

SINCE the preceding pages were committed to the press, a paper by Mr. Griffith, entitled "On the True Order and Succession of the Older Stratified Rocks in the neighbourhood of Killarney and to the north of Dublin*," has reached my hands. In reference to the vicinage of Killarney, I feel it incumbent on me to offer a few remarks.

I think it unfortunate that Mr. Griffith should persevere in placing in the same parallel, and designating by the same name, two series of strata which by his own showing are clearly in a different order and in a different position; thus pursuing the same course in Kerry as in Waterford, alike productive of obscurity and confusion, by a misapplication of the term "old red sandstone." Thus this formation, which is so well characterized in the Slieve Meesh range (Cahirconrée of Mr. Griffith), by its peculiar beds, horizontal disposition, and in overlying *unconformably* the older stratified rocks situated on the west (which latter generally approach the vertical position), is placed in parallel with those beds of conglomerate, sandstone, quartz-rock (greywacke), and clay-slate, occasionally coloured of a reddish hue, which form incidentally intercalated *conformable* portions of the consecu-

* Lond. and Edinb. Phil. Mag. for March 1840, with a plan and two sections.

tive transition series, and to all, or some, of such beds as the case may be, when thus coloured, affixing also the term "old red sandstone;" e. g., in Macgillicuddy's Reeks, in Dunloe Gap, in Purple, Tomies, and Glenaa mountains, in Brickeen island and Muckruss peninsula, and in the valley of Kenmare. Yet Mr. Griffith himself, in combating the positions of Mr. Charles W. Hamilton, insists that the red sandstone, where occurring in these localities, has been deposited *conformably* on the older slate (which latter he seems disposed to refer to the Cambrian or Silurian æras), and in a descending order graduates imperceptibly into that rock*. This being admitted, why not designate this a *transition* red sandstone (as I have always done) in contradistinction to the *old red sandstone* of the carboniferous epoch, found in *unconformable* position, and commonly distinguished, in some parts of its extent at least, by beds of red clay, red marly clay, and red slaty clay; e. g., in the Slieve Meesh range and in Kerry Head? Much ambiguity would be avoided by observing this difference in language. Again, after such an admission, it may be asked, why are these rocks, together with the limestone of Muckruss, of the islands of the Lower Lake of Killarney, and extending toward that town, blended by Mr. Griffith with the carboniferous series? This limestone within its own area not only alternates with certain beds of the older stratified rocks, the prevailing dip being to the south, but in their lateral extension it is enveloped by and interlocked with them, which latter relation is clearly proved by the position of the greywacke, sandstone, and slaty-rocks in the north-western portion of Muckruss peninsula, intervening between the limestone of the islands on the north and that of the south-eastern portion of the peninsula. Mr. Griffith conceives he has solved the question by the introduction of a *fault* ranging from Dunloe Gap through Purple, Tomies, Glenaa, and the southern part of Brickeen Island, and *under Turk Lake* to the main land east, where it is confessedly not visible, diluvial matter occupying the surface. This supposed fault does not appear to rest on any proof of actual disruption by an *up* or *down throw*, or by a *lateral movement*, but to be merely an inference drawn from the difference of strike observable between the strata that have been interrupted in their eastern and western continuation by the excavation of Dunloe Gap or by the formation of Turk Lake or the Lower Lake. I have seen no such dislocation in the masses extending from Dunloe Gap through Purple, Tomies and Glenaa

* L. & E. Phil. Mag. March, p. 162, 166.

mountains, to Brickeen island (beyond which to the east everything must be imaginary), as is indicated in Mr. Griffith's plan and sections; nor does he clearly aver that he has seen such himself, his idea of dislocation appearing mainly to rest on a seeming unconformity arising from some differences of strike*. The whole question, I think, is simply solved by two considerations: 1st, by the inosculation of the several strata, whether schistose, conglomerated, or calcareous, in the line of their strike, while all dip conformably south; 2nd, that where the line of strike has been irregularly broken through by superficial excavations on the existing dry land, as in Dunloe Gap, or by the intervention of spaces occupied by water, as in the Lower Lake of Killarney, and in Turk Lake, thus separating and removing from observation the direct continuity of strata, if we find some difference of strike in the opposite dismembered masses, it is not necessary to imagine a fault in the case, and to represent the distant strata as forming, when protracted, abutting angles upon such a line of fault, since a simple flexure of the strata upon the line of strike explains the whole matter at once. And this is quite in accordance with the general observation I have made on the older stratified rocks of the south of Ireland, as possessing, when viewed on the large scale, an east and west strike, yet subject to inflections from that line, which are locally of greater or less extent†. The opposite shores of Turk Lake, and those of the Lower Lake of Killarney also, being thus brought into connexion by a simple curvature of the strata, remove all difficulty, bearing in mind that the constituent strata are not persistently continuous, but interlock with each other; and the same may be affirmed with respect to the relations in Dunloe Gap.

But if the supposed fault were even real and not imaginary, how would it prove that all the strata north of it belong to one æra, and those to the south of it to another, while analogous strata in the valley of Kenmare are in uninterrupted connexion with each other, being there also included in and interlocked one with the other, and not persistently continuous

* Lond. and Edinb. Phil. Mag. for March 1840, pp. 163, 166, 171.

† Geol. Trans., vol. v., second series, Memoir on the South of Ireland, § 7.—It is as if we were to draw a straight line to denote the general east and west strike, and then upon this straight line to trace an undulated one, the successive curvatures of which rising above and falling below the straight line, would express the local strikes. In such a view it is obvious, that if the inflected line be divided into parts, and certain intervening portions be removed, that the remaining separate parts may appear to have a different strike relatively to each other, although in fact constituting portions of the same series, as, for example, in the opposite shores of Turk Lake.

beyond a certain extent? The key presented by Mr. Griffith does not appear to me to answer the purpose, nor can I perceive the anomaly which he conceives to arise from the southerly dip being persistent*; since among transition strata some are presented to us merely as intercalated bands with a corresponding dip, while others of a similar character are deposited in troughs, e. g., in Nassau and Belgium contrasted with the Eifel, as noticed in a preceding part of this paper.

The whole subject, so far from being of a mysterious character†, strikes me as sufficiently clear, and which I think may be made to appear by merely taking Mr. Griffith's own statements for our guide. Let us, in the first instance, follow him from the entrance of Dunloe Gap on the north and ascend to the summit of the Reeks on the south; and in the second, consider the view taken by him of a proposed section drawn across Dingle peninsula from Brandon bay on the north, to Feilaturrive on the south; in both cases employing his own language, more or less condensed, yet placing the series under numbers for the sake of greater distinctness.

From Dunloe Gap upward, the succession is thus given‡:

1. Reddish-grey quartzose rock; coarse-grained reddish-grey conglomerate; coarse-grained brownish-red slate (quarried for roofing slate); red quartzose sandstone alternating with coarse slate, the sandstone presenting occasionally a conglomeritic character§.

2. Chloritic quartz-rock, alternating occasionally with thin beds of green and purple clayslate; grey quartzose beds, alternating with thin beds of purplish clayslate.

3. Reddish-grey quartzose beds, alternating with thin beds of purplish clayslate.

4. Higher up, in ascending to the summits of the Coumeen Peest or eastern ridge of the Reeks, the strata become more red, and pass into a brick or cherry-red quartz-rock with some beds of conglomerate, identical in colour, composition, and structure with the red sandstone situated to the north of

* Lond. and Edinb. Phil. Mag. for March 1840, p. 163.

† *Ibid.*, p. 166.

‡ *Ibid.* pp. 163 to 165.

§ Compare this with the description which I have given of the entrance to Dunloe Gap, in my Memoir on the South of Ireland, in § 10, in which I have shown that these strata vary in their dip between the vertical and the horizontal, subject to undulations from north to south, yet with a general dip to the south. It is here Mr. Griffith introduces his supposed fault, but which, as before stated, I conceive to be merely an apparent deviation in the line of strike proceeding from an interrupted curvature of the strata.

the (supposed) fault in the Gap of Dunloe, but not so coarse-grained*.

The conglomerate on the top of the Reeks is perfectly *conformable* with the underlying strata, and, in fact, a regular gradation may be traced from the lower or chloritic portion of the series through the reddish-grey into the brick-red quartz-rock and conglomerate.

Proceeding now to the Dingle peninsula, the succession given also from north to south, namely, from Brandon bay to Feilaturrive, is as follows †:

1. Dark blackish-gray clayslate, the upper beds of which alternate with reddish-purple slate, some of which contain Silurian fossils.

2. Red slaty conglomerates, alternating with red and green slate and brown quartz-rock.

3. Chloritic quartz-rocks, with alternating purplish and reddish-grey clayslate, similar in composition and character to those of the Gap of Dunloe, and of that district generally.

Now, on the preceding I have to observe, that, as the prevailing dip of the strata is to the south, these rocks of the Dingle peninsula may be considered as lying deeper in the series than those which range in a more southerly parallel, namely, as extending from Dingle bay to the lakes of Killarney; yet the whole, on both sides of the bay, may assuredly be considered, in reference to their general composition, structure, and characters, as closely related to each other, and as forming one sequence; and taken in this point of view, there appears no difficulty in assigning the red conglomerate, red sandstone, and red slate at the entrance to and in Dunloe Gap to their proper position in the series. Mr. Griffith admits that in the higher parts of the Reeks (No. 4, as given above) the same red conglomerate is found as occurs below in the Gap of Dunloe (No. 1), the latter being succeeded by the chloritic quartz-rock (No. 2), which alternates with thin beds of green and purple clayslate. Again in the Dingle peninsula we find also red slaty conglomerates (No. 2 of that series), succeeded by chloritic quartz-rocks (No. 3), which alternate with purplish and reddish clayslate. Surely the analogy here is very close, bespeaking alternations of similar rocks in a line from north to south. In the Dingle peninsula the red conglomerates (No. 2) repose upon the blackish-grey clayslate (No. 1), with beds of reddish-purple slate, and some

* Lond. and Edinb. Phil. Mag. for March 1840, p. 165. But the conglomerate visible in the western ranges of the Reeks, in Lisbug mountain, is fully as coarse-grained as that of the Gap of Dunloe.

† *Ibid.*, p. 167.

Silurian fossils. The immediate substrata of the Dunloe red conglomerate, sandstone, and red slate are not visible, being *unconformably* overlaid on the north by the carboniferous limestone in the western quarter, and by the coal formation in the eastern; while, on the other hand, the transition series of the Dingle peninsula, just described, are *unconformably* overlaid on the east by the old red sandstone of the Slieve Meesh range (Cahirconrée of Mr. Griffith); the difference in the æras of production between the transition series and the carboniferous series being thus in both cases clearly marked*.

To the south of the strata of which we have been speaking, there is a band of blackish-grey clayslate, which may deserve Mr. Griffith's attention. It is traversed by the new line of road from Killarney to Kenmare, as it passes up by the line of the Upper Lake, and if duly examined might perhaps be found productive of fossils. This instance may suffice to show that there is no want of repetition of similar beds in a line traversing the series from north to south. In the general succession occur also well-defined greywacke and greywacke slate, terms which being out of favour with some geologists, Mr. Griffith appears to avoid using, although highly distinctive and useful when employed in a legitimate sense.

Upon the whole, I see no reason to depart from the opinion which I have formerly given, namely, that the limestone of the region of Muckruss extending to Killarney, is a local deposit enveloped and intercalated in the general transition series. And the same view, as to their forming portions of the consecutive series, applies to the other limestone bands in the south of Ireland, whether inclosed in and interstratified with the adjacent rocks, or merely superimposed and interstratified in the form of a trough with the subjacent series. In the valley of Kenmare this latter position appears to be established by the detailed researches of Mr. Griffith, which, I confess, escaped my observation.

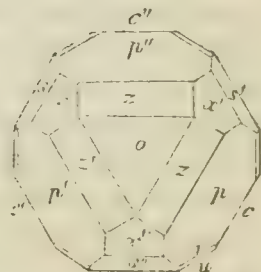
That among the strata in immediate association with the bands of limestone in the south of Ireland, some should be found, whether bearing the character of sandstone or clayslate, containing certain vegetable remains, cannot be held sufficient to invalidate the general view which I have taken; 1st, because such remains are not wholly foreign to a transition country; and 2nd, because it has been shown that the older stratified rocks of the south of Ireland form one consecutive series.

* See my Geological Map of the South of Ireland in Geol. Trans., vol. v., second series.

LXXI. *On the Form of Eudialyte.* By Professor MILLER*.

THE angle between normals to two adjacent faces p , of one of the rhombohedrons of eudialyte, is stated to be $106^{\circ} 20'$ in some mineralogical treatises, and $106^{\circ} 36'$ in others. A very accurate determination of its form is difficult on account of the unevenness and dullness of its faces. In order if possible to render it a little less uncertain, I measured some very perfect crystals, for which I am indebted to Dr. A. Smith of Dublin, and also some of the best crystals in Mr. Brooke's collection. The resulting values of the angle between normals to the faces o and p , either given immediately by observation or computed from the angles between other pairs of faces, after excluding all the observations that were unsatisfactory on account of the largeness of their difference from the mean, or the indistinctness of the reflected images, are $67^{\circ} 42'—40'—44'—48'—41'—39'—45'—41'—35'—43'—41'—47'—41'—36'—40'—47'—42'—47'$. The angles between normals to the faces calculated from the mean of the preceding values of $o p$, are

ou	$90^{\circ} 0'$	cu	$30^{\circ} 0'$
oc	$90 \quad 0$	zz'	$53 \quad 35$
oz	$31 \quad 22$	xx'	$84 \quad 4$
ox	$50 \quad 38$	pp'	$106 \quad 30$
op	$67 \quad 42$	ss'	$116 \quad 4$
os	$78 \quad 25$	ut	$13 \quad 59$
ot	$81 \quad 11$	pt	$22 \quad 46$



The symbols of the forms to which the different faces belong are, in the notation which I have adopted in my treatise on crystallography,

$$o \{111\}, \quad c \{2\bar{1}\bar{1}\}, \quad u \{10\bar{1}\}, \quad p \{100\}, \quad x \{110\}, \\ z \{211\}, \quad s \{11\bar{1}\}, \quad t \{201\}.$$

The faces t , which occur on two of the crystals in the possession of Mr. Brooke, do not appear to have been observed before.

St. John's College, Cambridge,
May 6, 1840.

* Communicated by the Author.

LXXII. *On some Phænomena of the Voltaic disruptive Discharge.* By W. R. GROVE, Esq., M.A., M.R.S.

MY DEAR SIR,

I SHALL be much obliged by your insertion of the inclosed in the Philosophical Magazine at your earliest convenience.

Yours very sincerely,

To Richard Phillips, Esq., F.R.S.

W. R. GROVE.

In the number of the *Bibliothèque Universelle de Genève* for March, is published an extract from a letter of mine to Professor Schœnbein, giving an account of some experiments on the voltaic disruptive discharge*. In the kind and flattering remarks which follow, my friend Dr. Schœnbein deduces a conclusion from those experiments which I had not ventured to make, but as many of the readers of the Philosophical Magazine may not have seen the article in question, I must beg leave to give a summary of its contents.

In considering the experiment communicated to the Philosophical Magazine for 1839, by Mr. Gassiot, in which he points out the remarkable difference between the heating effects at the anode and cathode during the disruptive discharge, it occurred to me that it might be due to the interposed medium, and that were there any analogy between the state assumed by voltaic electrodes in elastic media, and that which they assume in electrolytes, it would follow that the chemical action at the positive electrode in atmospheric air would be more violent than that at the negative, and that if the chemical action were more violent, the heat would necessarily be more intense. Several experiments confirmed this view: the battery with which they were made consisted of 36 elements, each consisting of a square inch of platina foil and of zinc; it was charged with concentrated nitric and dilute sulphuric acid as in my first experiments (Phil. Mag., May 1839) and arranged in single series.

I cannot avoid here a short digression as to the œconomy of this combination: this little battery required a pound of nitric and an equal weight of sulphuric acid† to charge it; and thus for the expense of about a shilling I could experiment for 8 or 9 hours without fresh charge; the arc of light

* After writing that letter I had determined to delay the publication of the experiments, in order to add to them some others, but these have been unavoidably postponed.

† The dilute sulphuric acid should be of sp. gr. 1.2 and four or five times the volume of the nitric; where there is not this disproportion between the cells it will be well previously to mix the nitric with one or two measures of dilute sulphuric acid. By proper attention to these proportions I obtain by electrolysis oxyhydrogen gas at 6d. per cubic foot including zinc consumed, &c.

obtained was 0.4 of an inch long. I am anxious to avoid the bad taste of eulogizing my own productions, but I think it ought to be generally known that space is not the only thing œconomized in this combination. Professor Jacobi, who has recently written a paper on my battery, and who has wrought on a large scale, states that he has readily fused iridium, &c. &c. after it has been at work for a whole day. An obvious point in the practical œconomy of the voltaic battery is, that the more intense the power of a combination the greater the œconomy, e. g. if one combination can effect with a series of two pair, what another can only effect by a series of 20, the equivalents consumed are as 1 to 10 in favour of the former.

But to return: experiments made with this battery established the following points:

1st. If zinc, mercury, or any oxidable metal constitute the positive electrode, and platina the negative one, in atmospheric air, while the disruptive discharge is taken between them, a voltameter inclosed in the circuit yields considerably more gas than with the reverse arrangement.

2nd. In an oxidating medium the brilliancy and length of the arc are (with some conditions to be presently noticed) directly as the oxidability of the metals between which the discharge is taken. N.B. Platina is to be regarded as slightly oxidable when influenced by the voltaic discharge; if this be taken for some time between platina points in oxygen, the volume of the gas is diminished.

3rd. In an oxidating medium the heat and consumption of metal is, as observed by Mr. Gassiot, incomparably greater at the anode than at the cathode.

4th. If the disruptive discharge be taken in dry hydrogen, in azote, or in a vacuum*, no difference is observable between the light and heat, whether the metals be oxidable or inoxidable, or whether the oxidable metal constitute the positive or negative electrode.

5th. The volume of oxygen absorbed by the disruptive discharge taken between a positive electrode of zinc and a negative one of platina in a vessel of atmospheric air, is equal to that evolved by a voltameter included in the same circuit.

These experiments present a remarkable analogy between the electrolytic and disruptive discharges. There are, however, two important elements, alluded to in art. 2. which obtain in the latter, and which have little or no influence on the former; these are the volatility and the state of aggregation or tenacity of the metal or conducting body. This is remarkably shown in the case of iron. Iron in air or oxygen gives a most

* I have not been able to experiment in a Torricellian vacuum; in a well-exhausted receiver, the difference, if any, was very slight.

brilliant voltaic arc, while in hydrogen or a vacuum with the same power a feeble spark only is perceptible at the moment of disruption. Mercury on the other hand gives a tolerably brilliant spark in hydrogen, azote, or a vacuum, and one more nearly approaching to that which it gives in air. Thus in an oxidating medium there are three requisites for a brilliant discharge, viz. oxidability, volatility, and looseness of aggregation: in other media the two latter alone obtain; and the brilliant arc given by charcoal appears to depend principally upon the last; thus wood charcoal gives a larger and more diffuse flame than the carbon from gas retorts.

In the following list the metals are arranged, as nearly as may be, in respect to the length and brilliancy of the arc they give in atmospheric air, the discharge being taken between two points of the same metal:

Potassium,	Antimony,
Sodium,	Bismuth,
Zinc,	Copper,
Mercury,	Silver,
Iron,	Gold,
Tin,	Platinum.
Lead,	

It has been noticed by Sir H. Davy, Dr. Hare, and Mr. Daniell, that a certain portion of matter is projected from the positive to the negative electrode: the quantity thus perceptibly projected is indeed very small; but I observed that when the discharge was taken in a close vessel the whole interior was lined with a pulverulent deposit, which if the vessel contained atmospheric air was an oxide of the metal employed, but if it contained azote or hydrogen was a reguline precipitate of the metal. Faraday's researches have established, that in electrolysis a voltaic current can only pass by a derangement of the molecules of matter; that the quantity of the current* which passes, is directly proportional to the atomic disturbance it occasions: he deduces from this that the quantity of electricity united with the atoms of bodies is as their equivalent numbers, or in other words, that the equivalent numbers of different bodies serve as the exponents of the comparative quantities of electricity associated with them. Now what takes place in the disruptive discharge? When we see the dazzling flame between the terminals of a voltaic battery, do we see electricity, or do we not rather see matter, detached, as Davy supposed, by the mysterious agency

* However unpredicable the words quantity, current, &c. may be of an agent imponderable, and having no definite relation to space, these words express understood functions, and it seems impossible to devise others which would not be open to similar objections.

of electricity, and thrown into a state of intense chemical or mechanical action? Matter is undoubtedly detached during the disruptive discharge, and this discharge takes its tone and colour from the matter employed. Now as this separation is effected by electricity, electricity must convey with it either the identical quantity of matter with which it is associated, or more or less; more it can hardly convey, and if less, some portion of electricity must pass in an insulated state, or unassociated with matter, and some with it. This seemed a highly improbable effect, and these considerations, immediately deducible from Faraday's researches, led me to suppose that the third alternative was most probably the true one, and consequently that the quantity of matter detached by the voltaic disruptive discharge was definite for a definite current, or bore a direct equivalent relation to the quantity electrolyzed in the liquid portions of the same circuit. The difficulties of testing this view by the weight lost, being insuperable without incurring an unjustifiable expense, led me to have recourse to Experiment 5. Zinc having only one oxide which sublimes when deflagrated in air by voltaic electricity, it should follow that if the particles of zinc detached were the equivalents of the matter electrolyzed in the same circuit, the quantity of oxygen absorbed by these particles would be exactly equal to that evolved in a voltameter placed in the circuit. The experiment presented more difficulties than I had at first anticipated: if the intensity of the battery was high, I frequently observed the metal, from the heat it had acquired, burn independently of the discharge; while if the intensity were lowered, the discharge could not be kept up without frequent contacts which gave a gush of gas to the voltameter. The following means afforded me the most uniform results. Between a positive electrode of distilled zinc of such size as to prevent by the cooling effect of its mass an independent combustion, and a negative electrode of platina, a moderated discharge was taken in a graduated vessel filled with atmospheric air: an average of 40 experiments gave me 1.17 to 1.00 as the inverse ratio between the volume of oxygen evolved in the voltameter, and that absorbed by the deflagration, and in several of these experiments the volumes were exactly equal. The nature of the experiment defied perfect accuracy, but considering a few unavoidable contacts, it appears to me to afford strong ground for presumption if not for conviction, that the separation of matter in the voltaic arc is definite for a definite quantity of electricity, and that the all-important law of Faraday is capable of much extension. Uniting this view with the experiments of Faraday

on the identity of electricity from different sources, and with those of Fusinieri on the statical electrical discharge, it would follow as a corollary that every disturbance of electrical equilibrium is inseparably connected with an equivalent disturbance of the molecules of matter. In the remarks of Professor Schoenbein to which I have alluded, he says that my experiments seem to prove the transmission of the current from one electrode to the other to be effected only by chemical action. In Exp. 5, I employed the diminution of oxygen as a measure of the quantity detached, conceiving that at the intense heat which is produced not a particle of zinc would escape oxidation, and without concluding that chemical action was absolutely essential to the existence of the voltaic arc. I must at the same time state that the passage of the current is, as proved in these experiments, materially modified by the nature of the elastic medium through which it passes, and is greatly aided when such medium is capable of uniting chemically with the electrodes. In pure dry hydrogen, I have never yet been able to maintain a continuous arc, except with charcoal, which forms carburetted hydrogen; and Davy in reference to his experiment of burning charcoal *in vacuo* states, that a gas was formed which detonated with oxygen; the probability is that some slight portion of air obtained access, and thus carbonic oxide was formed. On the peculiar relation between the electrodes and the elastic intermedium, I feel at present unable to give a clear opinion; the subject is peculiarly difficult; I will content myself with stating my present notion to be, that the voltaic arc bears a similar relation to common flame, to that which electrolysis bears to ordinary chemical action.

4, Hare Court, Temple, May 7, 1840.

LXXIII. *On two Norwegian Cobalt Ores from the Skutterud Mine.* By TH. SCHEERER.*

IN the cobalt mines of Modum in Norway (near Christiania) are found, besides the common cobalt glance, two other distinct cobalt minerals, which differ in their external characters from the usual ore. The following are the descriptions of these minerals:—

First kind. It occurs massive as well as crystallized, has the lustre of arsenical iron, and exactly the same crystalline form, even the characteristic brachydiagonal striæ. In hardness likewise, and also its spec. grav. = 6.23, it does not differ much from that mineral. It is shown by the blowpipe,

* An extract, communicated by the Author, from the original article published in Poggendorff's *Annalen*, vol. xlii. p. 546.

however, that it contains a considerable quantity of cobalt; three analyses gave the following composition: —

1.	2.	3.
17.57 sulphur	17.34 sulphur	18.06 sulphur.
47.55 arsenic	46.76 arsenic	46.01 arsenic.
26.54 iron	26.36 iron	26.97 iron.
8.31 cobalt	9.01 cobalt	8.38 cobalt.
<hr/>		
99.97	99.47	99.42

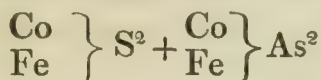
In a fourth analysis merely the quantities of iron and cobalt were determined, and were found to be 28.77 iron, 6.50 cobalt. In a fifth the cobalt was 10.8.

All these analyses were performed with quantities of different crystals.

From the difference in the relative quantities of iron and cobalt it follows, that these two metals can remplace one another, which is still more evident if the quantities of both be added together, when each time the same sum is obtained.

	1.	2.	3.	4.
Iron	26.54	26.36	26.97	28.77
Cobalt	8.31	9.01	8.38	6.50
<hr/>				
	34.85	35.37	35.35	35.27

The formula for this mineral would therefore be the same as that for arsenical iron, only that iron and cobalt are united in the combination, viz.



On measuring the crystals the following angles were obtained.

Fig. 1.

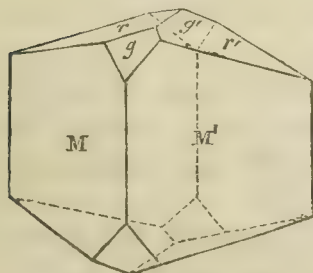
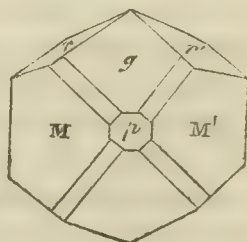


Fig. 2.



1. $M : M' = 111^\circ 40'$
2. $M : M' = 111^\circ 49'$
3. $M : M' = 112^\circ 2'$

In arsenical iron it is $M : M' = 111^\circ 53'$, consequently but slightly deviating.

The surfaces g, g' were found on different crystals to form the following angles:—

- | | |
|------------------------------|------------------------------|
| 1. $g : g' = 58^{\circ} 30'$ | 4. $g : g' = 58^{\circ} 28'$ |
| 2. $g : g' = 58 \quad 32$ | 5. $g : g' = 58 \quad 30$ |
| 3. $g : g' = 58 \quad 29$ | |

while in the arsenical iron $g : g' = 59^{\circ} 22'$.

The proportion of cobalt appears therefore, as the deviation amounts to nearly a degree, to have acted in this way on the crystalline form of the mineral. From its properties it may most properly be called cobaltic arsenical iron.

Second kind. This is characterized by its strong tin lustre, approaching even to that of silver, and still more by its high specific gravity, which = 6.78. It occurs massive, with more or less distinct tessular surfaces of cleavage, and also in single crystals. There can be no doubt that it is the mineral described some years since by M. Breithaupt, under the name of Tessular pyrites (*Tesseralkies*). The analysis gave,

77.84 arsenic.
20.01 cobalt.
0.69 sulphur.
1.51 iron.
trace of copper.

100.05

which accords pretty accurately with the formula Co As^3 , according to which the combination should consist of

79.26 arsenic.
20.74 cobalt.

100.00

if we admit that a quantity of arsenic is replaced by sulphur and a quantity of cobalt by iron.

The octahedron is constantly predominating in the crystals of this mineral, with subordinate surfaces of the cube, rhombic dodecahedron and icositetrahedron. The last-mentioned surfaces are wanting in none of the crystals. I measured the angles made by two such surfaces with those of the octahedron and found $160^{\circ} 33'$, whence it results that the parameters of the icositetrahedron are as 2 : 1 : 2, therefore just as in the common tin-white cobalt (*Speiskobalt*). It may be called Arsenical Cobalt-pyrites (*Arsenikkobaltkies*).

LXXIV. *On Galvanic Circuits composed of two Fluids, and of two Metals not in contact.* By Professor J. C. POGGENDORFF.*

IN support of the so-called Chemical Theory of galvanism, or rather of that view which places the source of voltaic electricity solely in the chemical affinity of the positive metal of the circuit, viz. the zinc, for the electro-negative constituent of the fluid, Mr. Faraday has of late brought forward in an especial manner the three following arguments:—

1. The production of a spark on completing the junction of a simple circuit.
2. The electrolytic law.
3. The preponderance of a circuit of zinc, platina, and sulphuric acid, over one formed of the same metals and a solution of the iodide of potassium.

With respect to the first point, the production of a spark on making the connexion between a single pair of plates, Faraday, in the Eighth Series of his Experimental Researches, lays much stress upon it, and for this reason;—because it must have passed *before* the metals could have come into contact, and thus not only proves its origin from pure chemical forces, but also the superfluity of metallic contact for exciting voltaic electricity†; in the further progress of his labours the English philosopher seems, however, to have some doubt as to the reality of a spark on junction; at least he expresses himself in his ninth series in a manner which induces the belief that he considers this spark to be a heating and ignition of the mercury employed in the experiment *at* or *after* the moment of junction‡.

Moreover, Professor Jacobi, in an experiment performed for the very purpose of solving this doubt, in which the wire of junction of a simple, very powerful zinc-platina circuit was interrupted by a layer of air only 0.00005 inch, was not able to perceive the least sign of the passing of a spark at the place of disjunction§. And recently Professor Draper, in New York, could not even observe the spark in a perfect vacuum before direct contact between the mercury and the wire which formed the arc of junction of a simple circuit||.

* From Poggendorff's *Annalen*, vol. xlix. January, 1840: translated by Mr. W. Francis.

† Experimental Researches, Ser. viii. §. 915.

‡ *Ibid.* §. 1074.

§ London and Edinburgh Philosophical Magazine, vol. xiii. p. 401.

|| *Ibid.* vol. xv. p. 349.

It can no longer, therefore, be a question whether there is a true spark on junction without metallic contact; moreover, other good reasons render very improbable the existence of the high tension requisite in the still open simple circuit*.

The electrolytic law is otherwise circumstanced. The correctness of this important law, with which Faraday has enriched the science of electricity, can be subject to no doubt; but many well-founded objections may be raised against its interpretation in favour of the chemical theory. The law consists in the fact that the quantities of the bodies decomposed in the single cells of the voltaic battery are in proportion to the chemical equivalents. It proves that the passage of like quantities of electricity is necessary for the decomposition of equivalent masses, but nothing more. It takes no part in the question respecting the origin of galvanic electricity. It is equally correct, whether the voltaic current originate from the contact of the metals, or from the chemical action on the zinc, or from any other cause†.

A proof in favour of the one or the other origin might perhaps be drawn from it, were it exclusively peculiar to the voltaic current, which would at the same time establish quite an essential distinction between voltaic electricity and magnetic, thermal, frictional, and animal electricity. But if, on the other hand, it is no peculiarity of the voltaic electricity, if it is rather

* [At the time when the sheets of the German original were sent to press, Prof. Poggendorff had not seen the collective edition of the *Experimental Researches*, in the preface to which Mr. Faraday acknowledges himself to have been in error with respect to the production of the spark without contact.—W. F.]

† The error with respect to using this law as an argument has originated from presupposing what first ought to have been proved by it, that the excitation of the electricity was effected by the solution of the zinc; while in reality, this solution, when amalgamated zinc and dilute sulphuric acid are employed, is solely the effect, the product of the electric current, and, when common zinc is used, arises partly from this current and in part from a pure chemical process entirely foreign to the circuit. It has happened with this law as with the well-known fact that in general the easily oxidable metals are the more positive. A connexion between oxidability and positiveness is accordingly evident, but which of these is causal, this or that, remains totally undecided. That it is, nevertheless, still continued to be interpreted in favour of the chemical theory, is the less justifiable, as several cases are known (and may every moment be increased by new ones) where the negative metal, notwithstanding that it is actually much more powerfully acted upon by acids than the positive one, still remains negative towards this. We need only call to mind the old experiment of Berzelius (*Gilb. Annalen*, vol. xxxv. p. 27), which, it is true, De la Rive conceives he has refuted, but which has long since been satisfactorily confirmed by Ohm. Moreover, the fact observed by Ritter, Davy, and many others, and which I have recently confirmed, that amalgamated zinc, notwithstanding it is little or not at all attacked by dilute acids, is in these considerably positive towards the so easily soluble unamalgamated zinc, deserves to be rescued from oblivion.

a property of *all* electric currents to decompose on their passage through a series of different fluids equivalent quantities of each, then *à priori*, every consequent conclusion from the law with respect to the origin of voltaic electricity is impossible. That the latter is then actually the case, that in fact the electrolytic law is common to *all* electric currents, that consequently the identity of electricities of various origin (manifoldly proved by Faraday himself in other respects) is established with reference to this law also, the simple experiment, published by me last year, on the simultaneous decomposition of two portions of water by the same magneto-electric current, can leave not the least room for doubt*.

Of the arguments advanced in favour of the chemical theory, there is now left in force for the present only the third.

The experiment upon which this is mainly founded consists in the fact, that two strips, one of zinc and the other of platina, are separated at their extremities, on the one side by sulphuric acid, on the other by a solution of the iodide of potassium. An electric current then occurs in a direction which indicates the preponderance of the sulphuric acid circuit over that of the iodide of potassium. The iodide of potassium, which, in case the two metallic slips are in direct contact at their other end, is decomposed in such manner, that its electro-negative constituent, the iodine, passes over to the zinc, gives it to the platina as soon as metallic contact is suspended at those ends and sulphuric acid inserted there.

Faraday places this experiment at the head of his researches on the origin of voltaic electricity. He regards it, as it were, as a scale for weighing two chemical affinities, that of the oxygen and that of the iodine for the zinc. Both endeavour, according to him, to excite an electric current; but that of the oxygen being the strongest, sets more electricity in movement than that of the iodine; the latter is therefore overpowered, and a current thus originates in the direction of the affinity of the oxygen, which, at the same time, since the two metals do not touch, affords another proof of the non-necessity of metallic contact to excite voltaic electricity.

The experiment is so remarkable, and the explanation given has in appearance so much plausibility, that it is not to be wondered at if the supporters of the chemical theory of galvanism have regarded it as a main prop of their opinion. Upon the defenders of the contact theory it made, however, but little impression, probably from their believing that no

* Poggendorff's *Annalen*, vol. xlv. p. 642.

regard need be had to an isolated fact speaking apparently in favour of the chemical theory, considering the numerous objections which may be urged against it.

In general they may have contented themselves with this otherwise perfectly correct position, that one metal, as soon as it is in contact with two fluids, can no longer be regarded as a single metal; so, for instance, that in Faraday's experiment that end of the zinc-bar which touched the sulphuric acid would be positive towards that which was moistened by the solution of the iodide of potassium. This at least is the opinion which Pfaff advances in his *Revision der Lehre vom Galvano-Voltaismus**. Pfaff is also, as far as I am aware, the only philosopher on the continent who has hitherto publicly discussed this experiment, without, however, performing more than a mere repetition of it experimentally.

From the great importance which, be it with justice or not, is assigned by the majority of the philosophers of the present day to the decision of the question respecting the origin of voltaic electricity, a more accurate examination of the process in the Faradayan circuit appeared to me not to be without value, and I therefore undertook the experiments, the results of which will be enumerated in the following pages as briefly and clearly as possible.

Galvanic circuits of two fluids and two metals not in contact may truly, as is readily conceived, be constructed without number. The English philosopher has examined a few, and these few always composed only of zinc and platina with various fluids. With this slight number of cases he always obtained results favourable to his view.

It appeared in the first instance to me to depend on this point: let it be seen whether among the great number of possible cases there may not be some which could not be brought under this view. If this question were affirmed by experiment, another demand would arise; namely, to subject Faraday's explanation even for the apparently favourable cases to a more rigorous examination.

Above all, I considered a greater change with the metals to be necessary, as, from long-known experience in other cases, it seemed to me highly improbable that the negative metal of the

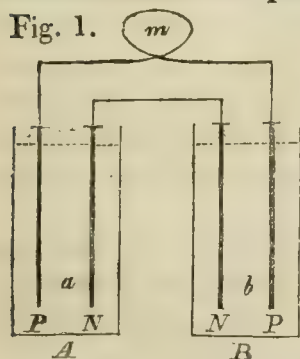
* P. 81. It stands there, it is true, quite reversed, that the platina was positive in the acid, and the zinc negative; but the connexion shows that this is only founded in a permutation or typographical error. In this memoir that metal is always termed positive which acts towards a second as zinc to copper in the common circuit.

circuit should act so passive a part as is assigned to it according to the theory at present prevailing in England.

I therefore made choice of the six metals: *platina*, *silver*, *copper*, *tin*, *iron*, and *zinc*, (*common* as well as *distilled* and *amalgamated*). In some cases I examined all the combinations which might be formed into pairs from these elements; in most cases, however, I was satisfied with circuits containing *zinc*, *iron*, or *tin*, as the positive element, since the three noble metals combined *inter se* give rise only to very slight effects. All the metals were as pure as they could possibly be obtained, and formed plates of nearly equal size, viz. somewhat about 3.5 inch in length, and 1 inch in breadth.

The fluids employed were: *water*, dilute *sulphuric acid* (acid of 1.827 spec. gr. diluted with 9 times its volume of water), dilute *nitric acid* (acid of 1.321 sp. gr. with 6 times its vol. of water), dilute *hydrochloric acid* (acid of 1.138 sp. gr. with 6 times its vol. of water), saturated solution of *chlorine*, liquid *caustic ammonia* (of 0.97 spec. gr. diluted with 4 times its vol. of water), and solutions of *caustic potassa* (1 part in weight to 4 parts in weight of water), *carbonate of soda* (1 in 3 water), *sulphate of magnesia* (1 to 3 water), *borax* (saturated), *sulphate of zinc* (1 to 4 water), *salt* (saturated), *sal-ammoniac* (saturated), and *iodide of potassium* (1 part in weight to 4 in weight of water). Distilled water was employed for the solutions; but where the water alone was tested, spring water was generally made use of, as it conducts better than distilled water, and does not sensibly differ in its electromotive action. All the above substances were as pure as possible; thus the sulphuric acid was free from nitric acid, the hydrochloric acid from chlorine.

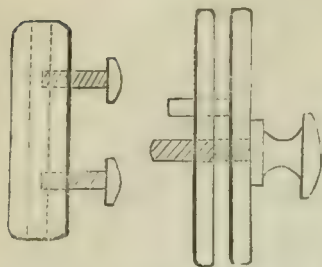
The mode of performing the experiments was as follows:



into two small glass vessels (*A*, *B*) I poured two of the above fluids (*a*, *b*) to the height of 2.5 inches, placed in each a heterogeneous pair of plates (*P*, *N*), and connected the plates of like nature by copper wires, of which one was the wire of the multiplier (*m*), whose needle had to indicate by its deflection the presence, the direction, and also comparatively the force of the electric current.

Two arrangements, which I will here describe more fully, from their great usefulness and manifold applicability in all galvanic experiments, served to effect an easy and certain combination of the wires with the plates; the first, it is true, has long been known to some experimentalists. They are both represented of the actual size in the annexed Fig. 2. The first

Fig. 2.



consists of a bored copper cylinder with two side screws. In this is inserted from the one side the end of the connecting wire, and from the other the piece of wire soldered to the plate, or instead of that the plate itself, which can be so cut that it may be inserted in it. When the screws are tightened connexion is made. The

second is constructed of two copper plates, which can be pressed against one another by a screw. In order that this may be effected without disturbing the plates, the one is provided with a pin which fits into an aperture in the other; this latter has also in the centre, on its inner side, a wedge-shaped furrow for its whole length. By means of this second clamp, thin plates, to which it is not desirable to solder pieces of wire, may be, as will easily be conceived, combined with wires, for the better insertion of which the above-mentioned furrow is made. The two clamps in this respect serve perfectly well all the purposes of mercury, without possessing its numerous inconveniences.

In general the current of a circuit of the kind described has but slight intensity; nevertheless it is always very perceptible with a sensitive multiplier. Frequently it was even so energetic that the magnetic needle of the instrument beat with violence against the pins which were erected at the points 90° to prevent its entire reversion; but often the deflections were but feeble, and in these cases especially I always took the precaution to exchange reciprocally the four plates employed for each experiment, and to take the mean from all the readings. In this way, it is true, each experiment became four experiments; but this exchange is quite necessary, as we never find, especially in plates of non-noble metals, iron and zinc, two homogeneous to such a degree, even when cut out of the same mass, as not to produce, when immersed alone in a conducting fluid, a frequently considerable current, which might easily overpower that which is intended to be observed.

Only in some cases where I was already acquainted with the direction of the current, for instance on repeating some experiments which I had previously performed, was I satisfied with employing the zinc plates in the combination, so that their heterogeneity should act against the current, instead of exchanging them. If the current was merely weakened by this, and not reversed, the result can be noted as certain.

I had hoped that the heterogeneity of the zinc would be less with the *distilled* metal, but found it nearly quite as great as with the common zinc, and even, as with the latter, increasing

on long use in acids, so that plates which fresh were nearly homogeneous, acquired with time a considerable heterogeneity. Filing the surfaces thoroughly bright was the sole means of restoring them to their original state. Nevertheless I employed in the further progress of my inquiries, and on the repetition of experiments, only distilled zinc, to be more certain.

For a similar reason I also had recourse to *amalgamated* zinc. Plates of ordinary zinc, possessing a very considerable degree of heterogeneity, lose it in fact by amalgamation (produced by rubbing them with dilute sulphuric acid and mercury), and become almost perfectly homogeneous; but they remain so only immediately after this operation, so long as the surface of the metal is, as it were, fluid. After some time the amalgam hardens to a crystalline mass, generally sooner or to a greater extent on one plate than on the other, and then the heterogeneity again makes its appearance. The plate with the fluid surface is in acids positive to that with the crystalline dull appearance. By rubbing the latter with mercury the heterogeneity may again be suppressed. Although it is in our power to bring amalgamated zinc plates to any degree of homogeneity previous to each experiment, yet it did not appear advisable to employ solely such, as amalgamated zinc must in a certain degree be regarded as a different metal from ordinary zinc, and its easy liability to change might produce disturbances. The sequence, however, showed that in general it affords the same results as the non-amalgamated zinc, and in numerous cases possesses considerable advantages over the latter.

Besides this, no precaution was neglected that is indispensable in this kind of experiments*; for instance, the cleansing of the plates after each experiment, by immersion in water,

* Here among other things might be reckoned the order of immersion, for it is a well-known fact, that of two plates of one and the same metal, on being immersed in a like fluid, that dipped in last is always *negative* to the one first inserted. I have found this to be perfectly true, but have likewise observed that on the *contemporaneous* immersion of two zinc plates in water slightly acidulated with sulphuric acid, several reversions in the direction of the current are produced; for instance, in the first moment a westerly deflection of 10° , followed by an easterly of 70° , which was soon succeeded by a permanent easterly deflection of 20° , that gradually sunk down to 0° , and passed into a westerly deflection of 38° ; sometimes a couple more reversions occurred. After several contemporaneous takings out and immersions only the westerly deflection was evident. A mere knocking one of the plates against the bottom of the vessel, which produced an elevation of about $\frac{1}{2}$ line, rendered this plate negative, or, causing with respect to the deflection, an increase or diminution in this deflection, although but transitory. The plates employed for this purpose were recently scoured, and at the termination of the experiments, which lasted nearly an hour, they had lost from the weakness of the acid little or nothing of their brightness. Both became covered with bubbles of hydro-

drying with bibulous paper, or, if it seemed requisite, by rubbing with sand-paper, scouring with sand and acids or water; although these operations are exceedingly troublesome from frequent repetition, yet they were never neglected. Moreover, the platina was, previous to each experiment, heated over an alcohol lamp after having been cleansed, as it otherwise only produces weak effects. I therefore believe that the following results merit some confidence, especially as most of them are deduced from several experiments repeated on various days.

Before, however, communicating these results, I must still add one remark.

By whatever cause the electricity may be produced in circuits of this kind, it is evident that there can be no doubt respecting the *where*, that it can only occur at the places of contact of the fluids with the metals,—since a contact of heterogeneous metals does not take place, or rather each of the two metallic slips contains two such contacts, which, from their being of opposite nature, must necessarily nullify each other. There are, therefore, in this kind of circuits four possible places of excitation, two in each vessel; and if we combine the electromotive force developed in each vessel, we have two such forces, e and e' , which act in opposition to each other. If, moreover, we call r the total resistance of the circuit, then, according to Ohm's fundamental law for the intensity of the resulting current, we obtain the expression $\frac{e - e'}{r}$.

Accordingly, the direction of the current, *i. e.* the direction of the deflection of the needle of the multiplier, depends solely on the sign of the difference $e - e'$; on the other hand, the intensity of the current, or the magnitude of the deflection, *both* on the value of the difference $e - e'$ and on the value r of the resistance. The amount of the deflection of the needle affords

gen, the temporarily negative one, so it seemed, always to a greater extent; yet but few bubbles ascended from it.

Amalgamated plates exhibit the same phænomena; but since in this case stronger acid may be employed without giving rise to any disturbance, it can be observed that the mere raising one of the plates about one inch renders it considerably negative; re-immersion again increases the negativeness. I observed this with sulphuric acid of 1.827 spec. gr. diluted with 9 times its volume of water, into which the plates were immersed to the depth of 2.5 inch.

These enigmatical currents are all of them, however, but of transitory duration, and they can therefore, in the following experiments, in which the positive metal, situated in the acid, was in most cases immersed last, at the utmost have only effected the first deflections, and then only when these were feeble. But in general the heterogeneity which originates for the positive plates from the contact with two fluids, is far stronger. At times I have also convinced myself that the results were essentially the same, whether the positive plates were immersed at the same time, or one after the other, before or after the negative plates.

alone, therefore, no measure for the difference of the electromotive forces here entering into action, the determination of which, however, is the sole object of the present inquiry. I have therefore, in the following experiments, chiefly directed my attention to the accurate determination of the direction of the current; its relative intensity, being dependent on too many circumstances, is only given approximately: the magnitude of the deflections of the needle were, however, always carefully noted.

M. Vorsselman de Heer, in his valuable memoir on Electric Telegraphy*, states of this numerator, after having observed that it depends on the nature of the metals, and not on their dimensions; that its value is not altered when salts, alkalies, or acids, which, like sulphuric acid, or nitric acid, are not electrolyzable, are added to the water; but that it undergoes a change when the body which is added is itself an electrolyte, for instance hydrochloric acid, in which case the numerator would be smaller.

According to Faraday, who, however, is not acquainted with Ohm's theory, this numerator would be greater, the stronger the affinity of the positive metal, the zinc, is for the oxygen, chlorine, or electro-negative constituent of the fluid in general; it must, however, be here observed, that we possess at present nothing more than approximate valuations from which we may judge of the energy of a chemical affinity.

The main result of my experiments has proved in the most positive manner *that the value of the numerator in Ohm's formula, or the magnitude of the electromotive force in general is altered, sometimes increased, sometimes diminished, by any substance added to the water, be it an electrolyte or not, and indeed (which should be well observed) increased for one metal combination, and diminished for another, by the same substance, added to the water in the same proportion.*

Nor have I been able to find that this force stands in direct ratio to the energy of the affinity between the positive metal and the negative constituent of the fluid. It is weak in cases where this energy must be considered as strong, and on the contrary strong where but a weak affinity can be admitted. Frequently, indeed, a current originates, and at times a powerful one, where, to judge from the affinity, not the slightest action should be expected.

Sufficient proofs of this will be found in the statements contained in the following Tables.

To render these Tables intelligible, it may be observed that

* Poggendorff's *Annalen*, vol. xlv. p. 516.

the signs of inequality express which of the two fluids (*a* and *b*, fig. 1, page 489) develops the strongest electromotive force when a pair of plates of the metals stated at the head of the column are immersed in them. The expression $s < w$, for instance, in the space common to the columns *zinc-silver*, and *hydrochloric acid water*, indicates that in contact with *zinc* and *silver* the *water* excites a greater electromotive force than the *hydrochloric acid*; or, in other words, that the current has a direction as if the zinc plate in the water were positive to the zinc plate immersed in the acid, the word positive being taken in the sense previously mentioned.

I might also have added to the direction of the current, or the direction of the magnetic deflections, their magnitudes, as I always noted them carefully. But I have omitted this, on the one hand, because the intensity of the current is no measure of the difference of the electromotive forces under consideration, and also because in the present case it is at the same time a very variable element. In general the intensity of the current in this kind of circuits is most powerful in the first moment, and then rapidly decreases; the amount and velocity of the decrease vary however considerably. This causes among other things that in two cases the difference of the two first deflections of the needle, to the right and to the left, may indeed be equal, but the magnitudes of the deflections differ considerably. In the one case these deflections amounted perhaps to 10° and 8° , in the other to 30° and 28° . In general the intensity of the current only descends to zero, but at times it also increases towards the opposite side. Sometimes this reversion of the current takes place so rapidly, that, for example, the first deflection of the needle, perhaps of 50° to the right, is immediately succeeded by one on the left of 70° . Now and then the intensity of the current is but feeble in the first moment, increases then for a time, and then gradually sinks to zero. Such variations render a numerical comparison of the intensity of the current in the different cases quite impossible, especially as in general, on the repetition of one and the same experiment, highly different values are obtained for the magnitudes of the deflections. I have therefore merely stated in general, in the notes to the Table, when the current was especially strong or feeble: when reversions of the current result, they are for the most part already indicated in the Table by the signs of inequality placed beneath one another.

DIFFERENCE OF THE ELECTROMOTIVE FORCES ON THE CONTACT OF TWO METALS WITH TWO FLUIDS.

No.	Fluids of the degrees of concentration mentioned.	Zinc. Platina.	Zinc. Silver.	Zinc. Copper.	Zinc. Tin.
1	Sulphuric acid (<i>s</i>)	$s > w$	$s < w$	$s < w$	$s < w$
	Water (<i>w</i>)	$s < w$			
2	Nitric acid (<i>s</i>)	$s > w$	$s < w$	$s < w$	$s = w$
	Water (<i>w</i>)				
3	Hydrochloric acid (<i>s</i>)	$s < w$	$s < w$	$s < w$	$s < w$
	Water (<i>w</i>)				
4	Solution of chlorine (<i>c</i>)	$c > w$	$c < w$	$c < w$	$c = w$
	Water (<i>w</i>)				
5	Solution of chlorine (<i>c</i>)	$c > s$	$c > s$	$c > s$	$c < s$
	Hydrochloric acid (<i>w</i>)				$c > s$
6	Caustic potash (<i>a</i>)	$a > w$	$a > w$	$a > w$	$a < w$
	Water (<i>w</i>)				
7	Ammonia (<i>a</i>)	$a > w$	$a > w$	$a < w$	$a > w$
	Water (<i>w</i>)				
8	Carbonate of soda (<i>n</i>)	$n < w$	$n < w$	$n < w$	$n < w$
	Water (<i>w</i>)				
9	Sulphuric acid (<i>s</i>)	$s > b$	$s > b$	$s > b$	$s > b$
	Borax (<i>b</i>)				
10	Sulphuric acid (<i>s</i>)	$s < i$	$s > i$	$s > i$	$s < i$
	Iodide of potassium (<i>i</i>)				
11	Hydrochloric acid (<i>s</i>)	$s < i$	$s > i$	$s > i$	$s < i$
	Iodide of potassium (<i>i</i>)				
12	Sulphuric acid (<i>s</i>)	$s < l$	$s < l$	$s > l$	$s < l$
	Sal-ammoniac (<i>l</i>)				

1. With *platina* $s > w$ merely weak, already diminishing on the second immersion, and then $s < w$ stronger than with silver, copper, tin; with *tin* $s < w$ increasing.

2. Results differing with *silver*, sometimes $s = w$, sometimes $s > w$, in most cases however decidedly $s < w$.

3. All action feeble; with *copper* however pretty strong.

4. With *platina* and *copper* very strong, somewhat weaker with *silver*. With *tin* an inclination to $c < w$.

5. With *platina* and *copper* very strong, with *silver* weaker, with *tin* only the first (strong) deflection $c < s$, immediately succeeded by strong $c > s$.

6. With *silver* $a > w$ strong, after which, though to a slighter extent, with *tin* the $a < w$.

7. Strongest effects with *silver* and *copper*.

8. Action feeble, strongest with *platina* and *copper*.

9. Action pretty strong, weakest with *silver*.

10. With *platina* in two experiments, first $s > i$ very strong, which however rapidly decreased and changed into $s < i$ quite as powerful. In all the following experiments, although the platina was always previously heated, the first effect was $s < i$ and generally very strong, sometimes moderate at the commencement, and on repeated immersion increasing.—With *silver* and *copper* action very strong, with *tin* likewise but opposite.

11. With non-heated *platina*, sometimes, but very feeble, $s > i$, passing into $s < i$. But with heated platina immediately and very strong $s < i$. With *silver* and *copper* $s > i$ very strong, with *tin* $s < i$ moderate.

12. With *copper* the $s > l$ strong, still stronger $s < l$ with *tin*. With a circuit of *tin* and *copper* likewise $s > l$.

No.	Fluids of the degrees of concentration mentioned.	Zinc. Platina.	Zinc. Silver.	Zinc. Copper.	Zinc. Tin.
13	Hydrochloric acid (<i>s</i>)	$s > l$	$s < l$	$s < l$	$s < l$
	Sal-ammoniac (<i>l</i>)	$s < l$			
14	Salt (<i>k</i>)	$k > w$	$k < w$	$k < w$	$k = w$
	Water (<i>w</i>)	$k < w$			
15	Salt (<i>k</i>)	$k < s$	$k > s$	$k > s$	$k > s$
	Hydrochloric acid (<i>s</i>)	$k > s$			
16	Salt (<i>k</i>)	$k < l$	$k < l$	$k < l$	$k < l$
	Sal-ammoniac (<i>l</i>)				
17	Sulphate of zinc (<i>z</i>)	$z > b$	$z > b$	$z > b$	$z > b$
	Borax (<i>b</i>)				
18	Sulphate of magnesia (<i>m</i>)	$m > b$	$m > b$	$m > b$	$m > b$
	Borax (<i>b</i>)				

No.	Fluids of the degrees of concentration mentioned.	Iron. Platina.	Iron. Silver.	Iron. Copper.	Iron. Tin.
19	Sulphuric acid (<i>s</i>)	$s > w$	$s = w$	$s < w$	$s < w$
	Water (<i>w</i>)	$s < w$			
20	Hydrochloric acid (<i>s</i>)	$s < w$	$s < w$	$s < w$	$s < w$
	Water (<i>w</i>)				
21	Caustic potash (<i>a</i>)	$a < w$	$a < w$	$a < w$	$a < w$
	Water (<i>w</i>)				
22	Ammonia (<i>a</i>)	$a < w$	$a < w$	$a < w$	$a < w$
	Water (<i>w</i>)				
23	Sulphuric acid (<i>s</i>)	$s > i$	$s > i$	$s > i$	$s < i$
	Iodide of potassium (<i>i</i>)				
24	Hydrochloric acid (<i>s</i>)	$s > i$	$s > i$	$s > i$	$s < i$
	Iodide of potassium (<i>i</i>)	$s < i$			
25	Sulphuric acid (<i>s</i>)	$s > b$	$s > b$	$s > b$	$s > b$
	Borax (<i>b</i>)				
26	Sulphuric acid (<i>s</i>)	$s < z$	$s < z$	$s < z$	$s < z$
	Sulphate of zinc (<i>z</i>)				

13. With *platina* at first $s > l$ slowly passing over into a stronger $s < l$. *Silver* weak, *copper* strong (although of opposite nature as with sulphuric acid), *tin* very strong.

14. *Platina* weak, *silver* and *tin* moderate, *copper* strong.

15. *Silver* weak, the other metals moderate.

16. *Copper* strong, *platina* almost nil.

17. *Platina* and *copper* pretty strong.

18. *Copper* strongest.

19. All actions feeble.

20. Likewise: with *copper* strongest.

21. *Platina* moderate, *silver* weak, *copper* pretty strong, *tin* very strong.

22. *Silver* weak, *tin* moderate, *platina* pretty strong, *copper* very strong.

23. *Platina* strong, *silver* and *copper* very strong, *tin* feeble.

24. With *platina* $s > i$ at first and slowly passing into $s < i$ only once observed; subsequently always immediately $s < i$. With *silver* and *copper* $s > i$ very strong.

25. With *copper* and *tin* strong.

26. With *platina* strong, after which *tin*.

No.	Fluids of the degrees of concentration mentioned.	Tin. Platina.	Tin. Silver.	Tin. Copper.
27	Sulphuric acid (<i>s</i>)	$s < w$	$s > w$	$s > w$
	Water (<i>w</i>)			
28	Nitric acid (<i>s</i>)	$s < w$	$s < w$	$s < w$
	Water (<i>w</i>)			
29	Hydrochloric acid (<i>s</i>)	$s < w$	$s < w$	$s < w$
	Water (<i>w</i>)			
30	Caustic potash (<i>a</i>)	$a = w$	$a > w$	$a > w$
	Water (<i>w</i>)	$a > w$		
31	Ammonia (<i>a</i>)	$a < w$	$a < w$	$a < w$
	Water (<i>w</i>)			
32	Sulphuric acid (<i>s</i>)	$s > i$	$s > i$	$s > i$
	Iodide of potassium (<i>i</i>)			
33	Hydrochloric acid (<i>s</i>)	$s > i$	$s > i$	$s > i$
	Iodide of potassium (<i>i</i>)	$s < i$		

No.	Fluids of the degrees of concentration mentioned.	Amalg. of Zinc. Platina.	Amalg. of Zinc. Silver.	Amalg. of Zinc. Copper.	Amalg. of Zinc. Tin.	Amalg. of Zinc. Iron.	Amalg. of Zinc. distilled Zinc.
34	Sulphuric acid (<i>s</i>)	$s > w$	$s < w$	$s < w$	$s < w$	$s > w$	$s < w$
	Water (<i>w</i>)	$s < w$				$s > w$	$s > w$
35	Hydrochloric acid (<i>s</i>)	$s > w$	$s < w$	$s < w$	$s > w$	$s > w$	$s < w$
	Water (<i>w</i>)	$s < w$				$s > w$	$s > w$
36	Caustic potash (<i>a</i>)	$a > w$	$a > w$	$a > w$	$a < w$	$a > w$	$a < w$
	Water (<i>w</i>)						$a > w$

27. With *silver* and *copper* the $s > w$ weak, often $s = w$.

28. All weak, yet with *platina* the first deflection $= 20^\circ$.

29. Actions weak, the least so with *copper*.

30. With *silver* and *copper* stronger than with *platina*.

31. With *silver* and *platina* weak, with *copper* pretty strong.

32. *Silver* and *copper* strong, *platina* very feeble.

33. With *platina* both the $s > i$ as also the subsequent $s < i$ weak. With *silver* and *copper* the $s > i$ very strong.

34. In all the experiments with amalgamated zinc plates these were constantly kept in the fluids, and the negative plates only were taken out and immersed both at the same time.—With *platina* only the first deflection of 10° to 20° in the direction $s > w$, succeeded by one of 90° in the direction of $s < w$. The effect far more energetic than with *silver*, *copper*, *tin*, and than with distilled, not amalgamated, zinc in similar experiments (although here as well as in No. 1 the *platina* had been heated to redness).—With *iron* immediately a very slow deflection of 20° to 30° , then a tranquil increase of the deflection to a permanent value of 40° and above.—With *zinc* immediately 70° in the direction $s < w$, rapidly decreasing and passing into $s > w$ to 20° .

35. In this case with *platina* the reversion evident, but making its appearance slowly; after repeated immersions only $s < w$.—*Tin* forming an exception from No. 3, 20, 29.—With *iron* no increasing effect as in No. 34, but giving a stronger one than there.—*Zinc* as in No. 34.

36. With *platina* weak, with *copper* somewhat stronger, but very strong with *silver* and *iron*. The $a < w$ with *tin* pretty powerful; both effects weak with *zinc*.

No.	Fluids of the degrees of concentration mentioned.	Amalg. of Zinc. Platina.	Amalg. of Zinc. Silver.	Amalg. of Zinc. Copper.	Amalg. of Zinc. Tin.	Amalg. of Zinc. Iron.	Amalg. of Zinc, distilled Zinc.
37	Ammonia (<i>a</i>)	$a > w?$	$a > w$	$a < w$	$a > w$	$a > w$	$a > w$
	Water (<i>w</i>)						
38	Sulphuric acid (<i>s</i>)	$s < i$	$s > i$	$s > i$	$s < i$	$s < i$	$s < i$
	Iodide of potassium (<i>i</i>)					$s > i$	$s > i$
39	Hydrochloric acid (<i>s</i>)	$s < i$	$s > i$	$s > i$	$s > i$	$s < i$	$s < i$
	Iodide of potassium (<i>i</i>)				$s < i$	$s > i$	$s > i$

37. With *platina*, *copper*, and *zinc* feeble, stronger with *silver*, still stronger with *tin*, and exceedingly strong with *iron*.

38. Even with *non-heated* platina the $s < i$ powerful. The $s > i$ also strong with *silver* and *copper*, less so the $s < i$ with *tin*. With *iron* the action exceedingly energetic, first a deflection of 40° in the direction $s < i$, immediately succeeded by one of 90° and oscillations between 90° and 80° in the direction $s > i$.—With *zinc* just the same, the first deflection in the direction $s < i = 90^\circ$, the second in the direction $s > i = 90^\circ$ followed by oscillations between $+90$ and $+75^\circ$.

39. Even with *non-heated* platina $s < i$ powerful, with it at least no reversion. With *silver* and *copper* action very strong.—With *tin* the $s > i$ weak, slowly, and especially after repeated immersion passing into $s < i$.—With *iron* only the first deflection of 60° to 70° in the direction $s < i$, the second immediately in the direction $s > i = 90^\circ$, then oscillations from $+90^\circ$ to $+80^\circ$.—With *zinc* precisely the same.

A careful glance at the facts contained in these Tables will justify the correctness of the positions above advanced. I will here draw attention to some distinct cases which will establish them more perfectly.

[To be continued.]

LXXV. On the moving the Knight over every Square of the Chess-board alternately. By GEORGE WALKER, Author of various Works on Chess; and Honorary Secretary of the St. George's Chess Club.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

YOUR Number for the present month has just come into my hands, and I find it contains an interesting paper upon the knight's move, by Dr. Roget, to which my attention is particularly drawn, from the circumstance of that essay having, it appears, been suggested by one of my chess articles in Fraser's Magazine (see Fraser of March 1840.). In that memoir, the subject of the knight's circuitous leap was merely touched upon *en*

passant; my object being only to give a method by which the cavalier could be guided mechanically through the labyrinth of the sixty-four squares, without the director's seeing the board, but ordering the move from memory alone; and the means furnished by me seem fully to meet the end in view. Dr. Roget's solution of the problem is highly ingenious, and it has given me much pleasure to follow his knight over the field. The subject having been once broached in your scientific Journal, it is my present aim to add in some degree to the materials already contributed by the learned secretary of the Royal Society.

Those who have not gone deeply into chess are hardly aware that a whole library has been written upon the knight's move, and ten thousand modes are printed in which the feat may be performed. Many of these methods are coupled with the most curious conditions, and must have taken long years to perfect. In addition to the authorities quoted by Dr. Roget, I append a list of works and writers, exclusively devoted to the problem in question. That so much time has been well spent, I am not prepared to admit; although the matter is as fair a hobby to ride as any other species of solitary calculation: but attached enthusiastically as I am to the practice of chess as a game, I cannot but regret the same energies have not been applied to illustrate points more immediately connected with the conduct of our scientific sport. Many parts of chess, particularly the openings and endings of the game, are capable of mathematical demonstration; and to name one, the celebrated study of what should be the legitimate termination of the strife by King, Rook, and Bishop, against King and Rook only, has been an *open question* from earliest time. Philidor, at the head of a strong body of chess scribes, considers the Rook and Bishop ought to win by force; while an equal number of writers entitled to authority dismiss this quantum of conflicting strength as a drawn game. To him who shall first give a printed solution of this highly difficult problem, the thanks of the European chess circle would be eagerly and gratefully tendered; and we should be proud to enrol him in the St. George's Chess Club, as the man who had achieved a task, compared with which the labours of Hercules were but as typical.

To lay down a general principle by which the march of the knight may be unhesitatingly guided over the 64 squares alternately is, however difficult, possible to be done; and this without burdening the memory with letters, words, figures, or other cumbersome elements of similar machinery. The following is the best, because the most simple, of the nume-

rous different plans of action which have come under my observation relatively to the subject. I take it from the most voluminous work extant upon the subject, being the German treatise of Warnsdorf.

Place the knight to commence on any square of the board you please, marking such square with a wafer or counter to show it has been used, and marking in a similar manner each square on to which the knight leaps in succession. The knight being at his post as agreed on, let his first move be to that square from which, in play, he would command the fewest points, observing that if on two or more squares his power would be equal, he may go indifferently to either of those squares; and that, as matter of course, a square once covered is not to be reckoned amongst those he commands, but must be dismissed altogether as done with. Continue moving him on this principle, and he will traverse the sixty-four squares in as many moves; reckoning the setting him down originally on the square chosen for the starting-point as the first move. To exemplify this, suppose the knight to start upon the king's bishop's second square. In this case his first move must be of necessity to the corner; since upon the rook's square he would then command but one point, viz. the knight's third; the king's bishop's second, on which he started, not to be again counted, but as a *caput mortuum*. Further explanation of matter so simple cannot be necessary.

Permit me, Gentlemen, to express my gratification at seeing chess at length take its legitimate place among the higher branches of science, and its mysteries allowed to develop themselves upon your pages. The king of games is, indeed, now fully appreciated as the only rational mental recreation,—the strongest auxiliary in the way of sedentary sport towards weaning the young from frivolous and exceptionable amusements, and furnishing their minds with healthy exercise. It has been recently enacted that chess may be played in the Royal Institution (Albemarle-street); and I cannot but hope the example will be followed, until chess-boards and chess-men will be found placed in the halls of meeting of every learned and scientific association throughout the kingdom.

I have the honour to be,

Gentlemen, your obedient servant,

1, Devonshire Place, Haverstock Hill,
April 1840.

GEORGE WALKER.

List of Works and Writers above referred to.

R. Willis. Attempt to analyse the Automaton Chess-player.
Lond. 1821. 8vo, pp. 40.

- Pratt's Philidor. Lond. 1825, 8vo. (I pass over many Magazine articles, and brief notices of the knight-problem, in numerous works on chess; as well in various collections of a general character, as Guyot's Recreations, &c.).
- Ciccolini. Del Cavallo degli Scacchi. Paris, 1836, 4to, pp. 70, and 20 large plates. (This volume treats not only on the problem of the knight's move over the common chess-board of 64 squares; but also on the larger field of 100 squares; as well as the circular board of 64.).
- La Corso del Cavallo per tutti gli Scacchi della Scacchiere. Bologna, 1766, 4to.
- Collini. Solution du problème du Cavalier, &c. Manheim, 1773, 8vo. pp. 62.
- Essai sur les problèmes de situation. Rouen, 1783, 8vo. pp. 74.
- Der Rösselsprung, &c. bei Eduard Billig. 1831, 24mo. pp. 64.
- Warnsdorf, H. C. von. Des Rösselsprungs, &c. Schmal-kalden. 1823. 4to, pp. 68.
- Lettre adressée aux auteurs du Journal Encyclopédique sur un Problème de l'Echiquier. Prague, 1773.
- Dollinger, 24 verschiedene Arten den Springer, &c. Wien, 1806, 8vo.
- Netto das Schachspiel:—And numerous other German writers.

LXXVI. *Memoir on the Law of Substitutions, and the Theory of Mechanical Types.* By M. DUMAS.

[Continued from p. 447.]

Mechanical Types.

AFTER having verified in a manner which satisfied my own conviction, the existence of certain chemical types, I tried the general application of this theory of types to all the known series produced by substitution, and last year at the School of Medicine I made this system of ideas the basis of my lectures.

But always constant to the experimental progress of the science, and wishing never to swerve from it, I asked myself whether it was necessary to class together bodies having the same formula, produced by substitution, but essentially different in their most prominent chemical properties?

I said, the bodies produced by substitution are divided into two different classes: some evidently belong to the same

genus, to the same chemical type: the others could not take their place there. What place can be properly assigned to these kinds of bodies?

We have not to wait long for a reply, and it carries the law of substitutions to a degree of generality and importance which it does not belong to me to develop here, but which the order of ideas obliges me to indicate.

The admirable work of M. Regnault on the æthers has indeed given an unexpected development to the theory of types. There is nothing more natural than to class in one genus bodies which approach so near as acetic acid and chloroacetic acid: but there must be good reasons for admitting that there is a true analogy between

Marsh gas	$C^4 H^2 H^6$
Methylic æther	$C^4 O H^6$
Formic acid	$C^4 H^2 O^3$
Chloroform.....	$C^4 H^2 Ch^6$
Bromoform.....	$C^4 H^2 Br^6$
Iodoform.....	$C^4 H^2 I^6$
Chloridated methylic æther	$\left\{ \begin{array}{l} C^4 O H^4 \\ Ch^2 \end{array} \right.$
Bichloridated methylic æther	$\left\{ \begin{array}{l} C^4 O H^2 \\ Ch^4 \end{array} \right.$
Perchloridated methylic æther	$C^4 O Ch^6$
Hydro-chlorate of methylene	$C^4 Ch^2 H^6$
Chloridated hydrochlorate of methylene	$\left\{ \begin{array}{l} C^4 Ch^2 H^4 \\ Ch^2 \end{array} \right.$
Bichloridated hydrochlorate of methylene	$\left\{ \begin{array}{l} C^4 Ch^2 H^2 \\ Ch^4 \end{array} \right.$
Chloride of carbon.....	$C^4 Ch^2 Ch^6$

Amongst these bodies, to which, without forcing anything, we might add prussic acid and ammonia, we meet with acids, bases, neutral bodies, and consequently substances the most unlike in an ordinary chemical view.

M. Regnault admits, and, he purposes to prove, that all the bodies so unlike, chemically speaking, which this series contains, that all those which can be united in analogous series, have this in common, that they belong to the same mechanical system.

I repeat that it is not for me to explain views which will be explained hereafter by their inventor, but I have to show in what these views differ from those which preceded them, to cause it to be felt how they complete what may now be called the general theory of substitutions. Moreover, it is

easy to class these ideas in the clearest manner, by the three following propositions :—

1. Experience proves that a body may lose one of its elements and take another in its place, equivalent for equivalent; this is the general fact of substitutions.

2. When a body is modified in this way, we may admit that its molecule has always remained intact, forming a group, a system in which one element has taken the place of another purely and simply.

In this point of view, which is altogether mechanical, and which is that of which M. Regnault pursues the study, all the bodies produced by substitution present the same grouping and may be referred to the same molecular type. I look upon them as constituting a *natural family*.

3. Amongst the bodies produced by substitution, there are a great number which evidently keep the same chemical character, acting the part of acid or of base in the same manner and in the same degree as they did before the modification they have undergone.

These are the bodies which I have considered as belonging to one chemical type, as making a part of the same genus, to speak the language of naturalists.

Thus is explained the law of substitutions, thus do we give an account of the circumstances in which it is not observed.

Every time that a body is modified without departing from its molecular type, it is modified according to the law of substitutions.

Every time that a body in becoming modified passes into another molecular type, the law of substitutions ceases to be observed in the action which ensues. Blue indigo is not a body of the same type as white indigo; the perchloride of carbon does not belong to the type of olefiant gas; aldehyd has departed from the type of alcohol; hydrated acetic acid does not belong to the type of aldehyd, &c.

The Academy will observe how, in this long series of researches, which has required six years of labour and the concurrence of the most skilful French chemists, we have risen from an obscure corner of the science, gradually and by the force of experience only, to the most general ideas of natural philosophy.

I admit then that through all the substitutions that a compound molecule can have undergone, when for all its elements others have been substituted successively, so long as the molecule remains intact the bodies obtained always belong to the same *natural family*.

When through the effect of a substitution, a body is trans-

formed into another which presents the same chemical actions, these two products belong to the *same genus*.

Alcohol, hydrated acetic acid, chloracetic acid, belong to the same natural family. Acetic acid and chloracetic acid belong to the same genus.

Such are the bases of the natural classification of organic substances, which I shall have soon an opportunity of developing before the Academy.

Before going further, it is just to notice here the labours of the chemists who have directed the science towards the point of view which now occupies us.

M. Regnault not only takes the first place in this respect, by the date of his observations, but by the importance of his researches and with respect to the ideas he has deduced from them, we must consider this young chemist as having advanced more than any one the state of the science on this point.

In my own name I can speak more freely than when I was commissioned to express the opinion of the Academy, and I think it my duty to declare here that the views of M. Regnault are connected with physical studies of the highest order, and that they give to the theory of substitutions a development as fortunate as it was unlooked for, in its application to the study of the most intimate physical properties of bodies.

At the same time with M. Regnault, two other chemists well known to the Academy, MM. Persoz and Laurent, were also occupied in researches concerning the theory of substitutions.

One of them indeed, M. Persoz, did not appear to occupy himself with the application of this theory; but the formula by the help of which he endeavoured to express the composition of a great number of mineral bodies, agreed perfectly with the developments which the theory of substitutions received by degrees from experience. The system of formulæ adopted by M. Persoz, and the views which they express in mineral chemistry, have then found a fortunate application in a great number of facts which the theory of substitutions has led to the discovery of in organic chemistry.

M. Laurent on his side has made a multitude of researches, and has published a great number of memoirs in support of the laws by which he sought to foresee and to explain all the phænomena of substitutions. As we saw above that the principal difficulty which is opposed to the approximation of acetic acid and of chloracetic acid consists in the similar function which we are compelled to attribute to chlorine and to

hydrogen, it is of importance to remark here, that M. Laurent insisted on the identity of the function of the chlorine with that of the hydrogen in bodies formed by substitution long before it had been positively established by experience.

It will not be my object now to write the history of the theory which occupies us; when experience shall have sounded all the parts in succession, it will be useful to go into the discussion of the *à priori* ideas which may have often predicted the results.

Thus putting aside every historical question, and stopping only at facts, at the experiments which have served as a basis to my own convictions, in a word, consulting only my personal impressions, I must say, that the first results in which I believed I could recognise in a decisive manner the elements of a view arrested by this subject, are those which organic chemistry owes to M. Malaguti. In fact, we know that this skilful observer has recognised, that æther, whether free or combined, may always lose two equivalents of hydrogen and gain two equivalents of chlorine, without any of its essential chemical characters undergoing alteration; for its power of combination remains exactly the same; chloridated æther then is still æther.

My conviction became complete, when I was able to recognise the precise nature of chloracetic acid, and when I saw chlorine take the place of all the hydrogen of the acetic acid, without modifying its capacity of saturation, without in any way altering what I term its *fundamental* properties; chloridated acetic acid then is still acetic acid.

It is by setting out from these two facts, it is by adding those which M. Regnault had himself observed in the action of chlorine on the *liqueur des Hollandais*, that I have tried to show that there exist, in organic chemistry, types capable of undergoing, without being destroyed, the most singular transformations as to the nature of their elements.

More recently M. Regnault, in the memoir on the æthers which I have already quoted, giving a still greater extension to those views, considered the bodies formed by substitution as belonging to one mechanical system. We may wait with confidence for the developments which he promises to give to these first views.

[To be continued.]

LXXXVII. *On an apparent Inversion of Perspective in viewing Objects with a Telescope.* By JAMES D. FORBES, Esq., F.R.S., Professor of Natural Philosophy in the University of Edinburgh.*

IN October last, Sir John Robison directed my attention to a curious anomaly in the apparent perspective of objects seen through a telescope which had been first mentioned to him by Mr. Whitwell.

It consists in a complete seeming inversion of the true inclination of two horizontal lines towards a vanishing point when seen through an ordinary telescope. The top and bottom lines of a row of windows, for instance, viewed obliquely, seem, within the limit of the field of view of the telescope to converge to a point on the opposite hand of the spectator from that indicated by the common rules of perspective, and by the experience of the naked eye. There is no better exemplification of the fact than by viewing the figure of a common sign-board, not far from the eye and considerably foreshortened, with a common pocket telescope. The letters appear gradually to increase from the nearer towards the more remote part of the inscription.

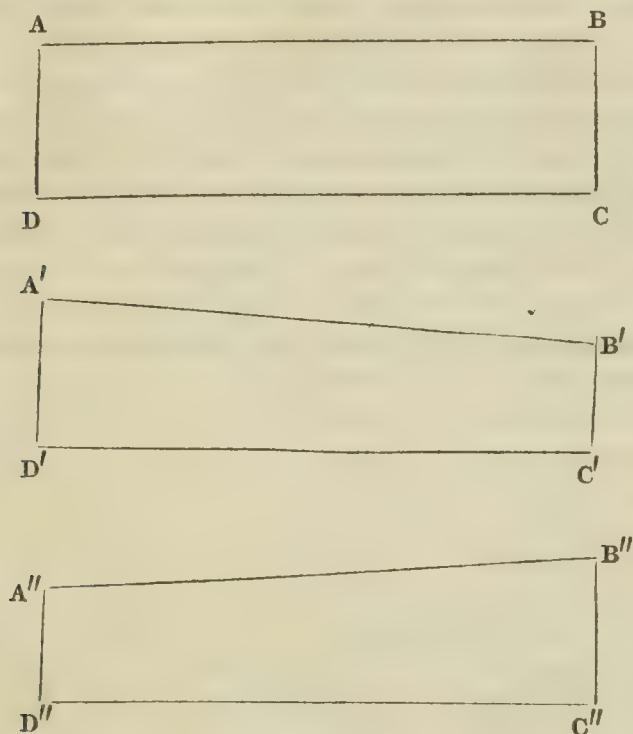
That the appearance is such as we describe no one will readily admit who does not make the experiment for himself; but once made, the fact appears so certain as to create surprise, that it does not always strike us, and that it has not (so far as I am aware) been mentioned in books on such subjects.

The first time I saw the anomalous appearance in company with Sir John Robison and Mr. W. A. Cadell, the explanation which I am about to state occurred to me as the true one. Not being particularly conversant with the subject of perspective, I contented myself with stating my opinion in writing at the time, and should probably have never recurred to the subject, had I not been lately requested to examine an ingenious paper, in which the *reality* of this distortion was admitted, and an attempt made to account for it by tracing the path of the rays through the telescopic lenses. Conceiving these investigations to be unsatisfactory, I made one or two simple experiments, which satisfied me completely of the accuracy of the view which I had previously taken of the matter, and which I now proceed to state.

The fact to be accounted for is, that a parallelogram, A B C D, or a word composed of letters of equal height, which by common perspective assumes to the naked eye the

* Communicated by the Author.

figure $A' B' C' D'$, when viewed through an erecting telescope has the inclination of the lines thrown the other way, or the surface then resembles the figures $A'' B'' C'' D''$; at all events no one will hesitate to affirm that the letters really the most distant seem to be larger than the nearer ones*.



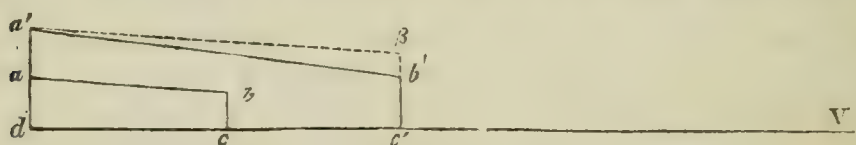
That this singular effect is a mere *optical illusion* I never doubted: and I recently ascertained the fact by measuring roughly with a micrometer the apparent angles under which $A'' D''$ and $B'' C''$ are respectively seen; the former was always found to exceed the latter, in other words the telescopic image is *really* convergent in the same direction with the unmagnified one, though the imagination in this case gives so completely the lie to the senses, that even when persuaded of the deception, and with the invariable standard of the micrometer divisions before our eyes, it is impossible to relinquish the preconceived idea.

The cause to which I assign this effect will appear a natural one to persons who are aware of the unperceived and momentary train of reasoning by which we arrive at conclusions apparently almost intuitive.

In every case in which the illusion now described is ob-

* It is scarcely necessary to observe that this anomalous appearance is wholly independent of the position of the image in the field of view, and is therefore independent of the common errors of aberration.

served, the spectator has previously determined by his eye the real position of the plane of the object towards which he directs his telescope: and when he views that object with a magnifying power of two, he believes himself to be looking at an object twice as large in the same plane as before; or else (what comes to the same thing) the same object as before brought twice as near to him, but moved parallel to itself. In either of these cases, the vanishing point ought to remain exactly the same for the enlarged as for the original object. If $a b c d$ be a board 4 feet long and 1 foot high, the eye expects to see through the telescope magnifying twice, a figure similar to that which a board 8 feet long and 2 high would present in the same situation, that is, a figure $a' b' c' d$ of which the upper and lower lines converge to the same point V as before; but the eye *really* sees through the instrument a merely magnified image of $a b c d$, namely $a' \beta c' d$, in which $a' \beta$ is



parallel to $a b$, and consequently the vanishing point V is thrown twice as far off. What must the mind, reasoning through the information lent by the eye, infer respecting this enlarged object? One of two things. *Either* that the sign-board so seen is really not a parallelogram, but has its further extremity $b' c'$ higher than the nearer one; *or else*, that the board is a true parallelogram, but that the plane in which it lies is more nearly perpendicular to the line joining the eye and the object, a plane in short which will give to horizontal lines a vanishing point as far beyond V as that is from d .

The former is the case when we look at an object to which we direct a telescope after having mentally formed an estimate of its position; the latter, or an erroneous estimate of a plane of the object, occurs when a person looks suddenly through a telescope previously pointed in an unknown direction.

I am not sufficiently conversant with works on perspective to be aware whether such a circumstance has before been noticed, but it was new to those whom I have had occasion to consult.

At all events it is very singular that it should have remained so long generally unknown that all objects (generally speaking) are seen through a telescope in *false perspective*.

The general principle may be thus stated in a single sentence. By common perspective, all parallel lines in a single

plane, or in any plane parallel to that, have a single vanishing point; but the act of magnifying increases the distance of the vanishing point in the same proportion as it does the apparent dimensions of the object; consequently the magnified object is not seen in true perspective for its own plane.

Edinburgh, March 13, 1840.

JAMES D. FORBES.

Postscript.—A casual circumstance brought to my recollection a few days ago an optical illusion mentioned (if indeed it was not shown) to me some years since by Sir David Brewster, the nature of which I could not perfectly recollect. Having applied to Sir David Brewster, he obligingly referred me to the *Edinburgh Journal of Science* * for a notice of it, when it proved, as I expected, to be referable to the principle I have just been applying.

“A field,” says Sir David, “may be so situated, that” (from the perspective of the furrows or drills upon its surface) “when seen through the telescope it appears like a perpendicular or *vertical wall of earth*. This phænomenon we have often seen in directing a telescope to a field above Melrose Abbey on the northern acclivity of the north-west Eildon Hill. This field is capable of being ploughed in the direction of its greatest declivity; but when it is viewed through a telescope, the slope is such that *the furrows do not appear to converge*, and the eye cannot readily perceive any difference between the breadth of the furrows at the remote end of the field and their breadth at the near end. The observer therefore immediately concludes that the field must be nearly in a vertical plane rising in front of him. This deception is a very remarkable one, and produces a singular effect on the mind when the field is covered with a crop, and when crows, &c. light upon it.”

A more perfect illustration of the *second form* of the optical illusion which I have described could not be desired. Every one knows how imperfectly the eye estimates the acclivity of a plane in full view. The parallelism of the ridges is tacitly assumed, and as their apparent convergence diminishes exactly in proportion as the magnifying power of the telescope increases, the mind is forced to the conclusion that the plane is more nearly perpendicular to the line joining the eye and any point of it, than it really is.

Hence it appears that Sir David Brewster noticed and published fourteen years ago one case of the curious observation of Mr. Whitwell.

March 17, 1840.

* First Series, iii. p. 88.

LXXVIII.—*On the Heat of Vapours and on Astronomical Refractions.* By JOHN WILLIAM LUBBOCK, Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.

[Continued from p. 441.]

ON THE PRESSURE OF STEAM.

THE most accurate and extensive experiments by which the accuracy of these relations can be tested are those which have been made upon the conditions of steam. The following are the experiments of Arago and Dulong, as recorded in tom. x. of the *Mémoires de l'Institut*, p. 231; together with the temperatures calculated by the best empirical formulæ*.

Nos. des observations.	Elasticité en mètres de mercure à 0°.	Elasticité en atmosph. de 0m.76.	Température observée.	Température calculée par la formule de Tredgold.	Température calculée par la formule de Roche coeff. moyen.	Température calculée par la formule de Coriolis.	Température calculée par la formule adoptée.
			Cent.	Cent.	Cent.	Cent.	Cent.
1	1.62916	2.14	123°·7	123°·54	123°·58	123°·45	122°·97
3	2.1816	2.8705	133·3	133·54	133·43	133·34	132·9
5	3.4759	4.5735	149·7	150·39	150·23	150·3	149·77
8	4.9383	6.4977	163·4	164·06	163·9	164·1	163·47
9	5.6054	7.3755	168·5	169·07	169·09	169·3	168·7
15	8.840	11.632	188·5	188·44	188·63	189·02	188·6
21	13.061	17.185	206·8	206·15	207·04	207·43	207·2
22	13.137	17.285	207·4	206·3	206·94	207·68	207·5
25	14.0634	18.504	210·5	209·55	210·3	211·06	210·8
28	16.3816	21.555	218·4	216·29	218·01	218·66	218·5
30	18.1894	23.934	224·15	222·09	223·4	224·0	224·02

There are reasons which make it probable that in inquiries of this nature the scale of temperature as indicated by the expansion of air is to be preferred, although the difference between the indications of a mercury thermometer with that of air is not considerable.

The following table is given by M. Pouillet (*Elémens de Physique*, vol. i. p. 259) for the centigrade scale :

Températures indiquées par le therm. à mercure, à enveloppe de verre.	Températures indiquées par un therm. à air, et corrigées de la dilatation du verre.	Volumes correspondans d'une même masse d'air.
— 36°	— 36°	0.8650
0	0	1.0000
100	100	1.3750
150	148.70	1.5576
200	197.05	1.7389
250	245.05	1.9189
300	292.70	2.0976
Ebull. du merc. 360	350.00	2.3125

[* Many of the results stated in the table of the French chemists are absolutely identical with those which had been published by Mr. Philip Taylor in 1822. See his Paper in the Philosophical Magazine, First Series, vol. ix. p. 452., and the accompanying engraved Table of his experimental results.—EDR.]

From the above I have deduced the following Table for Fahrenheit's scale :

Merc. therm.	Air therm.	Merc. therm.	Air therm.
212	212	482	478.1
302	299.7	572	558.9
392	386.7	680	662.0

I now proceed to determine for steam the constants γ and E by means of the observations of Dulong and Arago which I have quoted in the preceding page.

For the air thermometer on Fahrenheit's scale the experiments of Dulong and Arago (*Mém. de l'Institut*, vol. x.) give, θ being reckoned in Fahrenheit's scale and from the freezing point of water,

$$\begin{array}{lll}
 p = 1 & \theta = 180 & \frac{1}{\alpha} = 480^{\circ} \\
 p' = 11.632 & \theta' = 334.7 & \frac{1}{\alpha} + \theta' = 814.7 \\
 p'' = 23.934 & \theta'' = 396.4 & \frac{1}{\alpha} + \theta'' = 876.4.
 \end{array}$$

I find from these observations

$$\frac{p''^{1/\beta} - 1}{p'^{1/\beta} - 1} = [0.1140623],$$

the quantity within brackets being the logarithm of the corresponding number; and hence I find

$$\begin{array}{lll}
 \beta = .0134* & \gamma = .98677 & \frac{1}{\gamma} = 1.0134 \\
 E = 1.17602 & \log E = .0704184 & H = 6.6809.
 \end{array}$$

The pressure at the boiling point of water (212°) being unity,

$$\frac{1}{\alpha} + \theta = -\frac{[2.0651059]}{p^{.0134} - 1.17602};$$

so that if τ is the number of degrees on Fahrenheit's scale of the air thermometer, and the pressure p be reckoned in atmospheres,

* This value of β appears to me to be the only one which will satisfy the equation.

$$\tau = - \frac{[2.0651059]}{p^{.0134} - 1.17602} - 448^{\circ},$$

and if ϱ be the density of steam, the relative volume

$$\frac{\varrho}{\varrho'} = \frac{p \{p^{.0134} - 1.17602\}}{p' \{p'^{.0134} - 1.17602\}}.$$

In order to ascertain how far the new expression here given for τ represents the totality of the observations, I have calculated the temperatures corresponding to all the observed pressures in the observations of Arago and Dulong, and the results are exhibited in the following table.

Pressure in atmospheres.	Temperature.				Error of temperature calculated by Lubbock. Fahr.
	Observed.			Calculated.	
	Merc. therm. Cent.	Merc. therm. Fahr.	Air therm. Fahr.	Air therm. Fahr.	
2.1400	123.7	254.66	253.6	252.8	— .8
2.8705	133.3	271.94	270.4	270.1	— .3
4.5735	149.7	301.46	299.2	299.4	+ .2
6.4977	163.4	326.12	323.0	323.2	+ .2
7.3755	168.5	335.30	331.9	332.3	+ .4
11.6320	188.5	371.30	366.7*	366.7	0
17.1850	206.8	404.24	398.6	398.9	+ .3
17.2850	207.4	405.32	399.5	399.4	— .1
18.5040	210.5	410.90	404.9	405.3	+ .4
21.5550	218.4	425.12	418.5	418.8	+ .3
23.9340	224.15	435.47	428.4*	428.5	+ .1

The observations marked with an asterisk were employed in determining the constants.

The error of the temperature calculated by the formula adopted by Arago and Dulong corresponding to the first observation is — .73 cent. or — 1.3 of Fahr. I have no doubt that the observed temperature is in excess, and the agreement with the rest of the observations is so complete, that within this range of temperature the formula may, I think, be considered as exactly representing the phenomena. The errors of the temperatures, calculated by the various empirical expressions which have been hitherto proposed, are much greater, as may be seen in the table of Dulong and Arago. The following observations are those of Southern, given in p. 172, vol. ii., of Dr. Robison's Mechanical Philosophy.

Pressure.	Temperature.		Error of calculated temp.	Pressure.	Temperature.		Error of calculated temp.
	Observed.	Calculated.			Observed.	Calculated.	
Inch.				Inch.			
·52	62	59·5	−2·5	4·68	132	131·4	−·6
·73	72	69·3	−2·7	6·06	142	141·3	−·7
1·02	82	79·3	−2·7	7·85	152	151·6	−·4
1·42	92	89·9	−2·1	9·99	162	161·7	−·3
1·95	102	100·2	−1·8	12·64	172	171·8	−·2
2·65	112	110·7	−1·3	15·91	182	182·0	0
3·57	122	121·3	−·7	29·80	212	212·	

The formula deviates slightly from the observations at very low pressures. Dalton says that it is next to impossible to free any liquid entirely from air; of course if any air enter, it unites its force to that of the vapour.—*Manchester Memoirs*, vol. v. p. 570. It must be recollected that according to theory the constants γ and E are the same only as long as the chemical constitution of the vapour remains the same, and they vary for different substances.

With regard to the nature of the accurate expression which connects the pressure with the temperature, opinions have hitherto been various. According to Dr. Robison, Mr. Watt found that water would distil *in vacuo* when of the temperature of 70° , and that in this case the latent heat of the steam appeared to be about 100° ; and some other experiments made him suppose that the sum of the sensible and latent heats is a constant quantity. This, Dr. Robison says, is a curious and not improbable circumstance. Southern, on the contrary, concluded from experiments on the latent heat of steam at high temperatures that the *latent heat* is a constant quantity, instead of the latent heat + sensible heat being so. M. de Pambour, in speaking of Southern's view, says, "Cette opinion a paru plus rationnelle à quelques auteurs, mais le première nous semble mise hors de doute par les observations que nous allons rapporter." It appears to me by no means clear that Watt entertained the opinion here attributed to him, for in a note in the Appendix to Sir David Brewster's edition of Robison's *Mechanical Philosophy*, vol. ii. p. 167, he professes to agree in the opinion there delivered by Southern. In p. 166 Southern records three experiments, from which he obtained 1171° , 1212° , and 1245° , for the sums of the latent + sensible heat corresponding to the temperatures or sensible heat 229° , 270° , 295° . If we take the two extreme observations, we find a difference in the sum of the latent + sensible heat of 74 degrees, corresponding to a difference in the sensible heat of 66 degrees.

If the conditions under which Laplace obtained the equation

$$V = A + B \frac{p}{e} \frac{1}{\gamma}$$

are admitted, the value of E different from zero shows that the absolute heat is not constant; but the preceding theory does not appear to me to furnish the means of determining the value of D , and hence of deciding with certainty whether the latent heat is constant, and whether in augmentations of heat the sensible heat only varies. I think there can be little doubt that the conditions assumed by Laplace actually obtain, and that the hypothesis attributed to Watt* must be abandoned. The experiments recorded by Mr. Parkes in the 3rd volume of the Transactions of the Society of Civil Engineers, p. 71, which show that the quantity of fuel required to evaporate a given weight of water is nearly the same whatever be the pressure of the steam, do not seem to me to authorize a different conclusion. For this is precisely what would take place if the *latent* heat be constant, and if the quantity of fuel required to generate the *latent* greatly exceed that required to generate the concomitant *sensible* heat.

The quantity γ has never before been determined for steam† or for the vapour of any liquid, properly so called, as far as I am aware. It may excite surprise that the value of γ should come out less than unity. Both Poisson and Dulong assert that it is evident that γ must surpass unity, but the reason which they assign appears to me inconclusive.

[To be continued.]

LXXIX. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron.* By Dr. C. SCHAFHAEUTI, of Munich.

[Continued from p. 434.]

THE brown residuums of all white irons, when boiled with hydrochloric acid before ignition, parted with their iron with extreme difficulty. In one trial after boiling the mixture in a bottle whose neck was shut up with a capillary tube; first no apparent change took place, and only hydrochloric acid escaped; after boiling an hour the contents of the bottle began to become thickish, a disagreeably smelling gas escaped, which when ignited burned with a small but intensely blue-coloured flame.

* Mr. Sharpe has maintained the same opinion in the 2nd vol. of the Manchester Memoirs. See Dr. Thomson's Outline of Heat and Elasticity, p. 198.

† "Quant à la valeur de γ , elle nous est jusqu'à présent tout-à-fait inconnue."—Poisson, *Méc.*, tom. ii. p. 652.

After boiling for more than twenty-four hours, 20·4 grains left a dark-green residuum, weighing only 2·64 grains, and this residuum burnt in a dried stream of air in a horizontal glass tube, blackened with the first action of heat, and drops of water collected on the upper part of the tube. After the water was driven into its receiver, brown spots remained on the glass; the drops of water collected smelt as if impregnated with tobacco-smoke, and the gas which escaped from the apparatus for collecting carbonic acid had the same smell.

I collected carbonic acid 0·954: water 0·238: and there remained white silicon = 2·35.

For 0·954 carbonic acid is = 0·2637810 carbon; and 0·238 water is = 0·0263942 hydrogen. Sum of carbon and hydrogen 0·2901752. This last amount is only less 0·1998848 grain and to account for the loss of carbon, hydrogen and azote which passed through the bulb glass containing the caustic ley in the form of the before-mentioned nicotianic gas.

By boiling the residuum of white iron for a short time only in hydrochloric acid, very little iron is dissolved, and the remainder assumes a greyish colour, and becomes white or grey after the first ignition without first glowing like tinder.

By boiling it with caustic alkalis only traces of silica are extracted, whilst a species of brownish humus was dissolved, which I found never to exceed in amount 2 per cent.; the silica therefore must be contained in a chemical combination with carbon, azote and iron.

A short ignition of the residuum on the contrary is sufficient to make the greatest part of the iron soluble in acids. If it is only heated, till it begins of itself to glow, the iron is dissolved by acids, and the evolution of hydrogen shews that the iron must be contained in a metallic state in the residuum. When, on the contrary, this remainder is heated as long as it absorbs oxygen, and then treated with acids, no evolution of gas takes place, and a mixture of protoxide and peroxide of iron is dissolved.

As soon as this residuum begins to glow, carbonic acid, azote and a little hydrogen are invariably evolved, which shows the intimate connexion between the solubility of the iron and the carbon, azote and hydrogen in the residuums.

Not only the evolution of hydrogen gas, but the powerful action on the magnet, proves, that the iron in these residuums must be contained in the metallic state. Even were we to consider it as protoxide, the great increase of weight, that is, the absorption of oxygen, which takes place during ignition, could not be accounted for. But as the iron, notwithstanding its metallic state, is not soluble in acids before ignition, we must

assume that the metallic iron is in combination with carbon, azote and hydrogen, or perhaps in a state forming a sort of cyanet of iron.

The relation of the quantity of constituents of the brown residuum of the iron is so stable and so accurately in atomic proportions, that we may safely conclude the quantity of carbon from the amount of oxygen absorbed.

If we denote the increase of weight by letter (a), the oxygen of silicon as (s), the oxygen of iron by (f), and the carbon by (x), we obtain always the following simple formula, particularly when we consider that the iron is oxidized according

to the above-mentioned formula $\overset{.3}{\text{F}} \overset{..}{\text{F}}$:

$$x = s + f - a.$$

In some French irons I found the relations of carburets of iron to the siliciuret approach

$$\begin{array}{cc} \text{S} & \text{C}^4. \\ \text{F}^4 & \text{C}^4. \end{array}$$

If we further consider, that the white silica only remains when the iron residuum has been exposed to a number of ignitions, and that after only a few ignitions it remains always dark black, soluble neither in alkalis nor acids; we may find therein likewise a proof, that silicon is combined chemically with carbon. Further, the remainder of the residuum five times ignited had gained 0.028 grains, after the black-coloured scales had disappeared; this increase in weight can only be ascribed to the oxidation of those black scales, which must have been a metal whose oxide was white. Further, if we consider that the remainder of the residuum only once ignited, after having changed its deep-black colour to white, and after emitting a great quantity of smoke, had only lost 0.0511 grains; we may take all these circumstances as a certain proof that the silica obtained from these residuums must have been contained in the metallic state, and in combination with carbon, which is likewise to be suspected from its emitting bright red sparks when thrown into the flame of a candle.

Having now considered partially by induction the relative combination of the ingredients of the residuums of white irons, we proceed to ascertain by experiment the quantity of the volatile bodies in the residuums.

60 grains of the same Maesteg white iron, freed from all fine powder, and treated in a retort with 4 oz. of hydrochloric acid of sp. gr. 1.103, generated no spongy foam whatever; the liquid in the retort always remained transparent, and the action of the acid had ceased the next day.

13.711 grains of this residuum, corresponding with 35 grains

of iron, gave by combustion in Liebig's and Dumas's apparatus,

Carbon	1·22200
Hydrogen	0·03684
Azote	0·264

The residuum of the same iron, treated with hydrochloric acid, sp. gr. 1·14, left

Carbon	0·3862
Hydrogen	0·0297
Azote	0·2833

The actual quantity of azote and the other constituent calculated by direct chemical analysis, is shown in the following table:—

For 35 grains in per cent.

Silicon	0·353 =	1·00867
Aluminum . . .	0·030 =	0·08571
Antimony . . .	0·559 =	1·59710
Phosphorus . .	0·030 =	0·08553
Sulphur	0·112 =	0·32018
Azote	0·268 =	0·76371
Carbon	1·505 =	4·30000
Iron	32·033 =	91·52282
Loss	00·110 =	00·31428

35·000 = 100·00000

Iron of the works of Creuzot, departement de Saone et de Loire. Specific gravity = 7·536; the colour of the fracture *dead grey*. Treated as usual with hydrochloric acid of specific gravity 1·103. The evolution of hydrogen gas continued for fourteen days. The escaping hydrogen had an intolerable smell, somewhat resembling the smell of *selenium*. The remainder in the retort had retained the shape of the fragments of the iron previous to solution. When washed upon a filter, as soon as boiling water was poured upon the partially dry fragments, the air contained between the pores of the fragments escaped with the same hissing noise for a considerable time, as hydrogen escapes when hydrochloric acid is poured upon iron. In 35 grains of this iron 0·3213 grains of sulphur were contained, and the remainder was = 14·880 of a yellowish-brown colour. Concentrated hydrochloric acid when cold had no further action upon it; when boiled with the same acid for ten minutes it had lost 4·586 grains of iron. The remainder = 10·294, heated in a platinum crucible, did not glow or take fire spontaneously; but brought to a red heat, it at once attained a lively white appearance of ignition, which immediately subsided into its

original state, viz. a dull red heat. After cooling it had assumed a light-gray colour, arising from the intermixture of black with white spots, some of the white spots had aggregated into rather large reddish-white lumps. It gained in weight = 0.795 and was very powerfully attracted by the magnet. I boiled it again with concentrated hydrochloric acid and obtained a residuum = 4.217 grains. Heated on the lid of a platinum crucible it began to ignite spontaneously, and had afterwards lost 0.124 grains. Its colour now assumed a lighter gray appearance, and it looked more woolly and voluminous.

It was still attracted by the magnet. The black spots must therefore be a compound of carbon, iron and silicon, which could be destroyed neither by fire nor by acids.

I weighed again 14.880 grains of the above-mentioned residuum, and boiled it with hydrochloric acid for nearly thirty minutes. The remainder of the powder weighed now only 1.68 grains, was of a dirty yellowish-brown colour, and still retained the well-known smell of hydrogen gas. When heated it did not ignite spontaneously. It was apparently increasing in bulk, and began to assume a black colour, throwing up some dust on the sides of the crucible. After the crucible had acquired a white heat, the black colour rapidly disappeared, and a beautiful lemon-coloured powder remained, which after cooling, changed to a greenish yellow, and weighed 1.12 grains. It consisted of 0.687 silica, and oxide of chromium and iron = 0.526. I mention these two experiments, to show the great difference betwixt the residuums of the two remainders, boiled for a shorter or longer time. The remainder loses its spontaneous power of ignition the longer it is boiled in hydrochloric acid, and the more iron that is extracted from it, notwithstanding the powder to a certain degree always retains the property of spontaneously igniting on the application of a certain heat, but the degree of temperature sufficient to cause this ignition rises higher the longer it is boiled.

On being boiled for a short time only, and afterwards slowly ignited, the iron and perhaps silicon remain in a state of carburet, which is unalterable either by heat or acids, and is powerfully attracted by the magnet. I mention here the curious circumstance, that when, in the above-mentioned remainder, which we found = 1.68, we assume, instead of metallic iron, peroxide of iron, the loss is just equal to the quantity of carbon contained in 35 grains of iron.

Oxide of iron	. . .	0.533
Silicon	. . .	0.281
Loss	. . .	0.866
		<hr/> 1.680

I now proceed to show the results obtained when the above-mentioned residuum is treated, instead of acids, with caustic leys.

14·88 grains of this residuum, powdered and boiled with strong caustic potash ley for ten minutes, lost 0·261 grains. When the ley was poured over the powder it foamed like soap suds. A little hydrogen was disengaged, emitting (during the whole boiling,) an odour partly resembling that of heated asphaltum and partly that of fresh peat. Shortly after the boiling commenced, the powder appeared to be swimming in the clear liquid like distinct *black scales*, closely resembling so-called graphite scales, separated from gray cast iron; but when dried on the filter, those scales resumed their former yellowish-brown colour, and looked again like powder, without any trace of scales remaining; which again confirms my former assertion, that all *residuums of iron dissolved in hydrochloric acid*, are separated in scales.

14·619 grains of this boiled residuum, after being carefully heated, became dark-red brown, and had increased in weight 0·000

After the second ignition it became light red, and had increased in weight . . . 1·269

After the third ignition it had increased in weight 0·608

After the fourth ignition it had increased in weight 0·296

After the fifth ignition it had increased in weight 0·222

2·395

After being boiled in hydrochloric acid, only 0·6901 grains of a light gray colour remained. After a lively ignition it weighed only 0·4006 grains, which consisted of a distinct mixture of white and black grains.

Another residuum of 14·88 grains, boiled with caustic potash ley for more than thirty minutes, lost 0·325 grains. The remaining 14·555 grains, after being ignited had assumed a light red colour, and had increased . . . 1·871

Second time 0·486

Third time 0·249

Fourth time 0·000

0·606

During the process of boiling, the leys had extracted a mixture consisting of carbon, hydrogen, azote, and oxygen, with a very minute portion of silica. 35 grains of another specimen of Creuzot iron, whose specific gravity was = 7·305, treated

in the usual way with hydrochloric acid, left a residuum = 12·005. When heated carefully in a platinum crucible, it began to glow, and being quickly removed from the fire, after cooling it became black, and had gained . . . = 0·0000

After the second ignition it was observed to glow from within, and after cooling was found to be red within, covered with a black crust. It had gained 0·4500

After the third heating, a speck like a pin's head only began to glow, and the whole mass only became ignited after the sides of the crucible were red hot, and had gained 0·8460

After the fourth ignition, (and it had now become light red) 0·8108

After the fifth ignition 0·2100

„ sixth „ 0·1624

„ seventh „ 0·1440

„ eighth „ 0·0180

„ ninth „ 0·0540

„ tenth „ 0·0000

2·6949

After being boiled with hydrochloric acid, the usual light gray powder remained = 1·603. When ignited, it lost 0·3500, and a reddish-white powder was left amounting to 1·253 grains. The hydrochloric acid extracted from it peroxide of iron, contaminated with a little chromium = 0·5176, and left silica = 0·7353. The hydrochloric acid held in solution oxide of iron and chromium, and the whole remainder consisted therefore of

Iron combined with silica . 0·3589

Iron and manganese . . . 9·3703

Phosphorus 0·9450

Silicon 0·3532

Aluminum 0·0212

Azote. 0·2521

Carbon 0·6685

Loss 0·0358

12·0050

The hydrochloric acid in the retort had dissolved

Iron 21·9549

Chromium 0·4837

Phosphorus 0·1697

The sulphuret of lead yielded

Sulphur 0·3867

35·0000

Or to calculate it centesimally,

Silicon	1·0090
Aluminum.	0·0606
Manganese	0·2412
Arsenic	0·0000
Antimony	0·0000
Tin	0·0000
Chromium	1·3820
Phosphorus	3·1848
Sulphur	1·1050
Azote	0·7202
Carbon	1·9100
Iron	$\left\{ \begin{array}{l} 62·7282 \\ 27·5566 \end{array} \right.$
Loss	
	00·1023
	100·0000

I only state here briefly, that in order to separate chromium, iron, phosphorus and alumina, I proceeded in the usual way, —by melting the first-dissolved and evaporated quantity of iron to be analysed with carbonate of soda in a platinum crucible, and dissolving the fused mass in distilled water. The solution contained the phosphoric acid, the chromium, and a little alumina and silica; the remainder was oxide of iron, silica and alumina, and a little alkali.

The solution kept quiet for some time in a dish covered with paper, in order to allow the manganese to separate, was neutralized with nitric acid and evaporated to dryness. After having been moistened by nitric acid and dissolved again, the silica was left behind. The filtered liquor, neutralized with ammonia, let fall a basic phosphate of alumina. From the again filtered liquor, acidulated with acetic acid, acetate of lead precipitated phosphoric acid, sulphuric acid and chromium in combination with oxide of lead. Hydrochloric acid and alcohol separated the chromium from the precipitate, which consisted now only of phosphate of lead, sulphate of lead, and chloride of lead. From the solution, freed by means of sulphuretted hydrogen from all traces of lead, ammonia threw down the chromium in the state of oxide.

We here see a new feature in white iron; the carbon of white coke-iron is always combined with a constituent hitherto totally overlooked, that is, *azote*. No white coke-iron which I analysed was free from it, nor any gray iron free from aluminum.

The best *English cast steel* as well as *wootz* and *malleable iron*, possesses the same property of being combined with *azote*. Now as malleable iron produced from gray pig iron contains

nevertheless a quantity of azote, it must have been taken up during the process of puddling.

Silicon is also a necessary ingredient in all sorts of cast iron; *no carburation* of iron would take place without its presence, and the iron appears only to have dissolved the carburet of silicon. Silicon shares this property of carbonizing iron with manganese, and both are in some degree equivalent to each other, so that in certain cases the action of manganese is substituted for that of silicon.

Such iron, in which manganese is substituted partially for silica, has that peculiar property of producing, in a certain species of refining fire, *steel* instead of malleable iron; it is always obtained from decomposed spathic iron ores; its fracture is silver white, and possessing a highly large laminous crystalline structure, and is the well-known *spiegeleisen* of the Germans.

To give the reader an idea of the above-mentioned curious fact, I refer to the analysis of four sorts of white cast iron, made by the celebrated chemist Gay Lussac. The first specimen is white, very similar to the other three. It produces excellent malleable iron, but very inferior steel in the direct way.

Iron from Champagne.		De L'Iser.	Siegen in Germany.	Coblentz
Carbon	2.324	2.636	2.690	2.441
Silicon	0.840	0.260	0.230	0.230
Phosphorus ...	0.703	0.820	0.162	0.185
Manganese.....	traces	2.137	2.390	2.490
Iron	96.133	94.687	94.328	94.654

We see in the first specimen of the cast iron of Champagne, which produces excellent malleable iron, 0.840 of silicon with only a trace of manganese. In the second specimen, on the contrary, only 0.260 per cent. silicon are to be found, but, instead, manganese amounting to 2.137 per cent.

If we consider the quantity of silicon in the first example as a standard, the three other specimens contain only 0.26, and therefore 0.58 silicon less; now, 0.58 silicon wants 0.627 parts of oxygen to become oxidized to silica, but the quantity of oxygen which is necessary to convert the 2.137 of manganese into protoxide is 0.615; in fact, almost the same as for the silicon of the weight 0.58 before-mentioned.

This quantity of manganese gives to such iron the property of producing *steel* immediately in the refining fire, instead of malleable iron; and the explanation of this enigmatical property is given by the following facts, of which I shall treat more largely in another place.

The quantities of manganese and silicon are as 2·54 to 1; now, if we follow the process during the transition of this cast iron into steel, both those bodies have to burn away with the same quantity of carbon. By chemical analysis we find in the first two-thirds of the process, the quantity of manganese very rapidly diminishing in comparison with the carbon, leaving the silicon quite untouched. On the contrary, when manganese is replaced by silicon, this last is oxidized so very rapidly, as it is in relation to manganese only like 1 : 2·54, so that the greatest part of the silicon is oxidized when more carbon is present than is necessary to prevent the iron from fusion at a white heat. When the carbon is so far diminished, that the iron begins to become sufficiently solid for the hammer—all the silicon is oxidized, which gives, as we shall soon perceive, a hardness and tenacity to the steel.

Before we take leave of these three specimens of cast iron, I must return to the solution of acetate of lead of specimen A, in which the sulphuretted hydrogen had been collected.

The precipitated sulphuret of lead, as before mentioned, was not crystallized in scales; on the contrary, it had a viscous dark brown appearance; and after some days standing, I found it to contain rhomboidal columns of white crystals deposited on the bottom of the bottles in needle-like aggregations, and in a vertical position on the sides. On first inspection I considered them as crystallized acetate of lead, but their difficult solubility, however, in distilled water, induced me to examine them more closely; and by trying them with the blow-pipe, the rising copious white fumes covering the charcoal immediately showed the presence of antimony, and the well-known yellow ring around the crystals on the charcoal plainly signified the presence of lead. The crystals themselves, treated with concentrated sulphuric acid, disengaged acetic acid, and the crystals therefore consisted of antimony, lead, and acetic acid.

The above-mentioned treatment of 60 grains of white iron with hydrochloric acid of the specific gravity 1·103, left 2·92 grains of sulphuret of lead mixed with antimony and acetate of lead, and the precipitate contained 0·833 antimony.

It is remarkable, that not only almost all the antimony escaped with the hydrogen, but that the antimony was likewise deposited in the acetate of lead, being also oxidized and converted into an acetate, and that no trace of antimony followed the hydrogen after it had passed the second bottle.

LXXX.—*Notices respecting New Books.*

Solutions of the principal Questions of Dr. Hutton's Course of Mathematics ; forming a general Key to that work, designed for the use of Tutors and private Students. By THOMAS STEPHENS DAVIES, F.R.S., Lond. and Edinb. Royal Military Academy, Woolwich.

THE work of Dr. Hutton is too well known to require any detailed description in our pages. Drawn up half a century ago for the use of the gentlemen cadets of the Royal Military Academy, its object was to remove the inconvenience which had been felt to arise from the use of detached parts of a multitude of works on different branches of mathematical science. Compiled, too, for the use of boys who entered the institution at the age of fourteen, and whose periods of study varied from two to four years,—for boys who beside mathematics were instructed in their applications to mechanical and physical science,—the course was necessarily rendered a brief one.

Those of our readers who have paid attention to the history of mathematical science in this country, do not need to be told that, at the time of its first publication, it was by far the best treatise on the subject existing in our language. During the time which has elapsed, many improvements have been introduced into it by Dr. Gregory, to keep pace with the progress of science; but in the last edition the alterations were much more extensive and important than any which had preceded them. The number of new questions which were introduced into that edition have rendered it one of the most valuable books of examples in any language; and many subjects were treated there which for the first time found their way into an elementary book,—amongst which may be particularly specified Horner's general method of solving algebraical equations with numerical coefficients.

The work before us contains either the entire or indicated methods of solution of all the questions in this edition where it appeared likely that there could arise the least difficulty. We have always thought that such works were useful to teachers, and to private students of honest purpose; but we saw a drawback to this utility in the probability of their falling into the hands of pupils themselves. In the present case, however, we do not anticipate this evil; for the work cannot be used for the purpose of deception when any master exercises common vigilance, inasmuch as no student can proceed through the writing and especially filling the occasional blank steps without at least *understanding* the solution, and seeing its application to collateral problems. To a mere boy of ordinary capacity this is the utmost that can be accomplished even by the most diligent teacher: and hence, in this case, the labour of the master will be diminished and the acquirements of the pupil still secured.

To both we consider this work invaluable. We were much struck with the general elegance of the solutions, and much pleased with the systematic working formulæ adapted to numerical application. Such examples are calculated to improve the mathematical taste of

teachers themselves, whilst they will form in the *bond fide* student a regard for good order, a judicious discrimination between different methods of operation, that cannot fail to be of the utmost future use to him. The almost universal use of Hutton's Course both in private schools and by private students, therefore, will be rendered still more conducive to the general diffusion of good mathematical taste, by the publication of the present appendage to it.

We have not space to enter into an analysis of the contents of the 551 pages contained in Mr. Davies's work—filled, as it is, with original views on every branch of elementary mathematics. The author has even touched in various places on the matter of scientific history, and we value this innovation as much as any. How preposterous is it to despise the labours of our predecessors in whatever field of literature or science we are labouring ! As this portion of Mr. Davies's work is more immediately open to criticism than any other, perhaps it will not be considered irrelevant if we devote a few lines to its consideration.

The author's observations on the middle-age abacus are sensible and valuable. The change between the manual abacus and the membranaceous tablets is, indeed, easily conceived ; and in all probability was transferred in that manner to the Arabian system of computation. " Though this mode of notation," as Mr. Davies observes, " may never have been necessary, and very rarely employed by chroniclers and other persons merely literary, it would be of extreme value in the *performance of computations*. Amongst these expeditions would be of great consequence, and this would often be facilitated by writing the symbols in ruled columns, instead of placing the dice upon the abacus. This, again, would lead to running the several component letters of any one number into a single and continuous figure, which would represent the number." We must, however, observe, in all this, that we prefer *facts* to wholesale conjecture, however pretty and ingenious this last may be.

The derivations of the signs $+$ and $-$ as given at pp. 11, 13, are, we think, very improbable, and show that the late Professor Rigaud and Mr. Davies, who have worked together on this subject, are not well versed in ancient handwriting. On a point of this nature, where the subjects in dispute were introduced five centuries ago, it is necessary, we humbly submit, to take into consideration the mode of writing at the period,—if, indeed, the symbols are not altogether arbitrary,—and not reason on the *et* and the *P* of the present day, as if our rough-writing were the same with that of the Italians in the fifteenth century. We are sorry to speak disparagingly of what we infer to be a favourite theory with our author, but its very improbability is quite sufficient to bring with it a condemnation, and perhaps might eventually have done so from less merciful critics.

We beg our readers to study attentively the various remarks tending to the completion of the new method of solving numerical equations. The method of synthetic division, which forms the basis of that process, is here developed at length, and traced to the com-

mon method as its source (pp. 93–96) ; whereas Mr. Horner's investigation, though perfectly valid, rested on considerations too elevated for the supposed progress of the student. We were also much struck with the very simple and perfectly general method of investigating the criteria of DeGua and Budon for the reality of the roots in any given interval.

The new and extremely elegant solution of the singular problem, known as Colonel Titus's problem, given by Mr. Davies, well deserves attention. A singular fact with respect to the history of this problem was discovered by Mr. Halliwell in a MS. in the British Museum*. It appears that Harriot was the originator of it, and that it was proposed as early as the year 1649 by William Brereton, who was one of Harriot's pupils.

Here we close our brief memoranda on a work which is certainly one of the most valuable in its kind that has ever appeared. The style in which it is written, and its entire conception, bespeaks no ordinary mind ; and though, from the author's reputation, we should have expected a work superior to those generally published with the same professed objects, yet our expectations did not reach to the possibility of so varied a mass of information being couched under such a form as that to which the author was tied down by the nature of his undertaking. We only regret that his limits prevented him from continuing his original plan of giving varied solutions and entering into the history of the different branches of elementary mathematics. We venture to hope that at some future period this latter part of the plan may be accomplished in another form.

LXXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

May 7, **A** PAPER was read, entitled " Researches in Embryology, 1840. **A** Third Series: a Contribution to the Physiology of Cells." By Martin Barry, M.D., F.R.S., F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh.

In the second series of these researches, the author had traced certain changes in the mammiferous ovum consequent on fecundation. The object of his present communication is to describe their further appearances obtained by the application of higher magnifying powers ; and to make known a remarkable process of development thus discovered. In order to obtain more exact results, his observations were still made on the same animal as before, namely, the rabbit, in the expectation that, if his labours were successful, it would be comparatively easy to trace the changes in other mammals. By pursuing the method of obtaining and preserving ova from the Fallopian tube which he recommended in his last paper, he has been enabled to find and examine 137 more of

* Life of Sir Samuel Morland, p. 28.

these delicate objects ; and has thus had ample opportunity of confirming the principal facts therein stated. He has now procured in all 230 ova from the Fallopian tube. But being aware that repeated observations alone do not suffice in researches of this nature, unless extended to the very earliest stages, he again specially directed his attention to the ovum while it is still within the ovary, with a view to discover its state at the moment of fecundation, as well as immediately before and after that event.

The almost universal supposition, that the Purkinjian or germinal vesicle is the essential portion of the ovum, has been realized in these investigations ; but in a manner not anticipated by any of the numerous conjectures which have been published. The germinal vesicle becomes filled with cells, and these again become filled with the foundations of other cells ; so that the vesicle is thus rendered almost opaque. The mode in which this change takes place is the following, and it is one which, if confirmed by future observation, must modify the views recently advanced on the mode of origin, the nature, the properties, and the destination of the nucleus in the physiology of cells. It is known that the germinal spot presents, in some instances, a dark point in its centre. The author finds that such a point is invariably present at a certain period ; that it enlarges, and is then found to contain a cavity filled with fluid, which is exceedingly pellucid. The outer portion of the spot resolves itself into cells ; and the foundations of other cells come into view in its interior, arranged in layers around the central cavity ; the outer layers being pushed forth by the continual origin of new cells in the interior. The latter commence as dark globules in the pellucid fluid of the central cavity. Every other nucleus met with in these researches has seemed to be the seat of changes essentially the same. The appearance of the central portion of the nucleus is, from the above process, continually varying ; and the author believes that the nature of the nucleolus of Schleiden is to be thus explained. The germinal vesicle, enlarged and flattened, becomes filled with the objects arising from the changes in its spot ; and the interior of each of the objects filling it, into which the eye can penetrate, presents a repetition of the process above described. The central portion of the altered spot, with its pellucid cavity, remains at that part of the germinal vesicle which is directed towards the surface of the ovum, and towards the surface of the ovary. At the corresponding part, the thick transparent membrane of the ovum in some instances appears to have become attenuated, in others also cleft. Subsequently, the central portion of the altered spot passes to the centre of the germinal vesicle ; the germinal vesicle, regaining its spherical form, returns to the centre of the ovum, and a fissure in the thick transparent membrane is no longer seen. From these successive changes it may be inferred that fecundation has taken place ; and this by the introduction of some substance into the germinal vesicle from the exterior of the ovary. It may also be inferred, that the central portion of the altered germinal spot is the point of fecundation. In further proof that such really is the case,

there arise at this part two cells, which constitute the foundation of the new being. These two cells enlarge, and imbibe the fluid of those around them, which are at first pushed further out by the two central cells, and subsequently disappear by liquefaction. The contents of the germinal vesicle thus enter into the formation of two cells. The membrane of the germinal vesicle then disappears by liquefaction.

Each of the succeeding twin cells presents a nucleus, which, having first passed to the centre of its cell, resolves itself into cells in the manner above described. By this means the twin cells, in their turn, become filled with other cells. Only two of these in each twin cell being destined to continue, the others, as well as the membrane of each parent-cell, disappear by liquefaction, when four cells remain. These four produce eight, and so on, until the germ consists of a mulberry-like object, the cells of which do not admit of being counted. Nor does the mode of propagation continue the same with reference to number only. The process inherited from the germinal vesicle by its twin offspring, reappears in the progeny of these. Every cell, whatever its minuteness, if its interior can be discerned, is found filled with the foundations of new cells, into which its nucleus has been resolved. Together with a doubling of the number of the cells, there occurs also a diminution of their size. The cells are at first elliptical, and become globular.

The above mode of augmentation, namely the origin of cells in cells, appears by no means to be limited to the period in question. Thus it is very common to meet with several varieties of epithelium-cells in the oviduct, including those which carry cilia, filled with cells; but the whole embryo at a subsequent period is composed of cells filled with the foundations of other cells.

In the second series of these researches, it was shown that the mulberry-like object above mentioned, is found to contain a cell larger than the rest, elliptical in form, and having in its centre a thick-walled hollow sphere, which is the nucleus of this cell. It was further shown that this nucleus is the rudimental embryo. From what has been just stated, it appears, that the same process, by which a nucleus in one instance transforms itself into the embryo, is in operation in another instance, where the product does not extend beyond the interior of a minute and transitory cell. Making allowance, indeed, for a difference in form and size, the description given of the one might be applied to the other. It was shown in the second series, that in the production of the embryo out of a nucleus, layer after layer of cells come into view in the interior, while layers previously formed are pushed further out; each of the layers being so distinctly circumscribed as to appear almost membranous at its surface. The same membranous appearance presents itself at the surface of the several layers of a nucleus in many situations. Further, in the formation of the embryo, a pellucid centre is the point around which new layers of cells continually come into view; a centre corresponding to that giving origin to similar appearances in every nucleus described in the present memoir. It was

shown that in the embryo this mysterious centre is present until it has assumed the form of the cavity, including the sinus rhomboidalis, in the central portion of the nervous system.

The process above described as giving origin to the new being in the mammiferous ovum, is no doubt universal. The author thinks that there is evidence of its occurrence in the ova of batrachian Reptiles, some osseous Fishes, and certain of the Mollusca; though the explanation given of these has been of a very different character. It has hitherto been usual to regard the round white spot, or cicatricula, on the yolk of the bird's laid egg, as an altered state of the discus vitellinus in the unfecundated ovarian ovum. So far from thinking that such is the case, the author believes the whole substance of the cicatricula in the laid egg to have its origin within the germinal vesicle, in the same manner as in the ovum of Mammalia.

There is no fixed relation between the degree of developement of ova, and their size, locality, or age. The variation with regard to size is referable chiefly to a difference in the quantity of fluid imbibed in different instances by the incipient chorion. Vesicles filled with transparent fluid are frequently met with in the Fallopian tube, very much resembling the thick transparent membrane of the ovarian ovum. These vesicles are probably unimpregnated ova, in the course of being absorbed. The so-called "yolk" in the more or less mature ovarian ovum, consists of nuclei in the transition state and exhibiting the compound structure above described. The mass of these becomes circumscribed by a proper membrane. They and their membrane subsequently disappear by liquefaction, and are succeeded by a new set, arising in the interior, and likewise becoming circumscribed by a proper membrane, and so on. This explains why some observers have never seen a membrane in this situation. After the fecundation of the ovum, the cells of the tunica granulosa, that is, part of the so-called "disc," are found to have become club-shaped, greatly elongated, filled in some instances with cells, and connected with the thick transparent membrane by their pointed extremities alone.

That the thin membrane described by the author in his second series as rising from the thick transparent membrane in the Fallopian tube, and imbibing fluid, is really the incipient chorion, was then shown by tracing it from stage to stage, up to the period when villi form upon it. There remained, however, two questions undecided; viz., whether the chorion is formed of cells, and if so, whether the cells are those of the so-called "disc," brought by the ovum from the ovary. The author now states that the chorion is formed of cells, which gradually collect around the thick transparent membrane, and coalesce; and that the cells in question are *not* those of the "disc" brought with the ovum from the ovary. The cells which give origin to the chorion are intended to be more particularly described in a future paper.

The existing view, namely, that a nucleus, when it leaves the membrane of its cell, simply disappears by liquefaction, is inapplicable to any nucleus observed in the course of these investigations.

The nucleus resolves itself into incipient cells in the manner above described. In tracing this process, it appears that the nucleus, and especially its central pellucid cavity, is the seat of changes which were not to have been expected from the recently advanced doctrine, that the disappearing nucleus has performed its entire office by giving origin at its surface to the membrane of a single cell. It is the mysterious centre of a nucleus which is the point of fecundation; and the place of origin of two cells constituting the foundation of the new being. The germinal vesicle, as already stated, is the parent cell, which, having given origin to two cells, disappears, each of its successors giving origin to other two, and so on. Perpetuation, however, at this period, consists, not merely in the origin of cells in cells, but in the origin of cells in the pellucid central part of what had been the nucleus of cells.

The author shows that neither the germinal vesicle, nor the pellucid object in the epithelium-cell, is a cytoblast. He suggests, that the cells into which, according to his observations, the nucleus becomes resolved, may enter into the formation of secondary deposits—for instance, spiral fibres; and that they may contribute to the thickening which takes place, in some instances, in the cell-membrane.

The germ of certain plants passes through states so much resembling those occurring in the germ of mammiferous animals, that it is not easy to consider them as resulting either from a different fundamental form, or from a process of developement which even in its details is not the same as what has been above described; the fundamental form in question in Mammalia—and therefore it may be presumed of Man himself—being that which is permanent in the simplest plants,—the single isolated cell.

LXXXII. *Intelligence and Miscellaneous Articles.*

ON THE PRODUCTION OF ELECTROTYPES. BY ALFRED SMEE,
ESQ., SURGEON.

[Illustrated by Plate VII.]

THE mode of taking copies of medals by the galvanic current* is deservedly occupying much of the public attention, and each is striving to add his mite to the perfection of this elegant and useful process. There are two or three points to which I am desirous of drawing the attention of your readers, as they appear to open a new and important field for investigation for which I have not the time at present. With regard to the precipitation of the copper, I beg leave to submit a modification of a plan first proposed by Mr. Mason, in a paper read before the Electrical Society, but I believe also contemporaneously used by other persons, that of making copper form the oxygen side of the battery, which being dissolved is again thrown

* We learn from the foreign journals, that Prof. Steinheil, of Munich, is applying this process for making a cast in copper, from a composition by the celebrated sculptor Schwanthauler, representing the labours of Hercules, and containing 140 figures.—*EDIT.*

down at the platina or hydrogen end upon the medal or mould placed for its reception.

The mode which I adopt is, first to obtain a long dish or trough, and then to place a wire in the inside along its bottom, which is connected to the zinc of one of the cells of my battery along the opposite side of the vessel; a large piece of copper is placed in connexion with the silver of the battery, and a solution of sulphate of copper is then added. By this arrangement the current is generated at the zinc, passes to the medal, reduces the copper whilst the oxygen and acid are transferred to the refuse copper, and dissolves a corresponding quantity of copper, and by this means the solution is always kept saturated with the metal.

When medals are to be copied, they are singly placed in contact with the wire in connexion with the zinc of the battery, and in this way many may be done in the same vessel, and any may be taken out and examined without the slightest interruption to the others. The rapidity of the process may be increased without detriment by the use of two to six or even more cells of the battery, as the copper will still be extremely tough. It will be found that my battery will require not the slightest alteration, except once a day, when the liquid should be changed. I have tried other solutions of copper, such as the nitrate; but although the process is hastened, the metal is apt to be brittle, or to have other imperfections.

When engraved plates are to be copied, the first copy is in *basso relievo*, and therefore a second is required to be made which is in "*intaglio*," and then ready for printing. Copies may even be taken of non-conducting substances, as wood-cuts, &c., by brushing them over with black-lead, taking care that the copper wire is in good contact with the plumbago.

The great advantages of this mode of proceeding above all others are, first, the quality of the copper is far better than when reduced in the usual way as described by Messrs. Spencer and Solly; this advantage is owing to the use of the copper at the oxygen end as suggested by Mr. Mason; secondly, all the plates or medals, for there is no limit to the number, are in the same vessel; thirdly, the process may be hurried or retarded, accordingly as the number of plates of the battery are increased or diminished; fourthly, the plates will not require to be interfered with till the precipitation is completely finished; and there are many other more trifling advantages which it would be tedious to enumerate.

The mode of proceeding here detailed differs but little from others which have been described; but these trifling differences are so important in practice, that this mode will probably supersede every other. In fact, I have had the pleasure of seeing many most valuable copper-plates subjected to this process, and the specimen which accompanies this paper is I believe the first which has ever undergone the ordeal of having the large number of impressions, required for any publication, printed from it. Of course it is a perfect facsimile, and therefore this method would be of the greatest importance to bankers for their notes, and is far superior to Mr. Perkins's process for the multiplication of plates, because in that case they

almost invariably require to be touched up afterwards, and therefore absolute identity is destroyed. The cost of their manufacture would be trifling, being merely the value of the zinc* dissolved in the battery, and a pound of zinc of the value of sixpence would produce a copper-plate weighing about two pounds; and I trust that copper will again, from its beauty, take the place of steel engravings.

So much for the precipitation of the copper; and the next thing to which I have to direct your attention, is a mode of making a copper-plate engraving without an engraving in the first instance. This is done by drawing upon a smooth piece of copper (such as a plate used for engraving) with any thick varnish or pigment insoluble in water, and then exposing the plate in the usual way to the influence of the current, when first copper will be thrown down upon the uncovered parts and will gradually grow over the drawing, and the electrotype when removed will be ready for printing. A practical difficulty, however, arises in the application of this in the arts, for unless very thick oil paint is used, sufficient depth is not obtained to hold the ink. However, judging from the sharpness of the edges of the lines, I have but little doubt that this difficulty may be overcome by those who are accustomed to drawing; and it possesses, as an additional advantage to its cheapness, the valuable property of not requiring the artist to reverse the design. An opposite effect to this may be produced by placing a piece of copper similarly drawn upon at the oxygen end of the battery, when the metal will be acted upon, leaving a drawing in basso relievo.

Bank of England, April 21, 1840.

ON THE REDUCTION OF CHROMATE OF LEAD. BY R. F. MAR-
CHAND.

The employment of chromate of lead, instead of oxide of copper, in organic analysis, is in many cases recommended by Richardson. It is preferable in the examination of substances containing chlorine, iodine, bromine, and sulphur, but particularly in the examination of the two latter. Erdman and I have frequently employed it with Hess's apparatus, and observed that the reduced chromate of lead will again absorb oxygen and might be then again employed. This circumstance induced me to make various experiments upon the reduction of this salt.

The chromate of lead used in these experiments was prepared by the precipitation of a solution of nitrate of lead with an excess of bichromate of potash, and afterwards carefully washing it: the salt was heated to dryness; it became of a dark red colour approaching cinnabar red; on cooling it returned to its former yellow colour provided it was not fused. If fused it turned to a dark brown colour, which on reducing to powder was of a brownish yellow colour. When the fused salt is quickly cooled by throwing it into cold water it becomes of a permanent red colour, giving also a red powder.

* The zinc in the fluid might be precipitated as a carbonate, for which there is great demand in the arts, and thereby the expense of the electrotype would be further diminished.

Many persons imagine that in organic analysis with chromate of lead it is necessary to use a very strong heat in order to perfect the operation. This is a mistake, for carbon as well as hydrogen very easily reduces the chromate. If however it is required to liberate oxygen, then the temperature must be very great, and the salt must be fused. This circumstance, as may be easily conceived, is inconvenient and liable to introduce error.

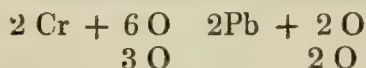
When chromate of lead is heated in a current of hydrogen gas, it commences to glow at a heat far below redness, and a quantity of water is formed. The yellow colour of the salt disappears; it becomes black, and very small metallic globules are disseminated through the mass.

3.049 grammes lost 0.307 gram. or 10.07 per cent. of oxygen; this loss may be increased by a continued and strong heat.

1.91 grammes lost in another experiment 0.224 gram., or 11.8 per cent. oxygen. At the commencement of the reduction of this portion the temperature was kept moderate, by which it lost 0.2045 gram., or 10.7 per cent. Oxygen gas was then passed over it while in a heated state. At a low degree of heat, the mass burnt with great brilliancy, turned brown, at least partly so, which was very evident on cooling. It absorbed 0.133 gram. oxygen, which calculated for the original quantity (1.91) amounted to 7 per cent.; a small quantity of water was formed during the operation, amounting, however, to but a few milligrammes. It would therefore appear that hydrogen was condensed in the pores of the reduced mass, but in a small quantity; heating in a stream of carbonic acid gas would have entirely driven it out. The oxidated quantity 1.839 gram. which had lost from the first 3.7 per cent. of oxygen, was again reduced in hydrogen gas, by which it lost 0.152 gram., this upon the whole quantity is equal to 8 per cent. Upon heating in oxygen gas the same appearances again took place, and the mass absorbed 0.128 gram., therefore, a similar quantity as before. A subsequent reduction at a very high temperature occasioned a loss of 0.128 gram. while a repeated oxidation only gave an increase of 0.119. This was again driven out by hydrogen, but without any further decrease of oxygen.

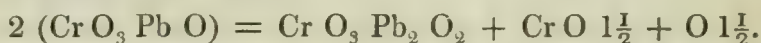
If we examine these experiments, keeping in view the results, we shall find that 1.91 gram. lost 0.224 gram. equal to 11.8 per cent. while the last oxidation was only 1.19 gram. or 6.2 per cent. Chromate of lead contains 19.54 per cent. of oxygen, equal to four

atoms, $\overset{..}{\text{Cr}} \overset{..}{\text{Pb}}$. 11.8 per cent. is equal to 2.4 atoms or nearly $\frac{5}{8}$ ths of the whole amount of oxygen. This would be 12.2 per cent. The reduction, if complete, would convert all the oxide of lead into the metallic state and the chromic acid into the state of oxide of chrome.



By oxidation about half of the oxygen is recombined; this takes place the more readily when the metallic lead is in a finely divided

state and not melted by too strong a heat. It is not the lead alone, but also the oxide of chrome, which absorbs oxygen. If oxide of chrome alone be heated in oxygen gas, it is not converted into chromic acid; but this takes place, as is well known, if an alkali be present. I therefore consider that oxide of lead has the same effect in this respect as an alkali. In order to obtain an intimate mixture of oxide of chrome and oxide of lead, I endeavoured by means of heat so to decompose the chromate of lead, that all the chromic acid should be converted into oxide of chrome. The temperature must be very high for this purpose, and it requires a long time before any considerable quantity of oxygen can be driven out of this salt. 1.409 gramme was fused in a very thin platina crucible by the strongest heat of a spirit-lamp before any appreciable loss took place. 0.057 grm. were then given off equal to 4 per cent. which is nearly $\frac{3}{16}$ ths of the whole quantity contained in it; this would be 3.9 per cent. It is therefore very probable that at first the chromate of lead is so decomposed that basic chromate of lead and oxide of chrome are formed.

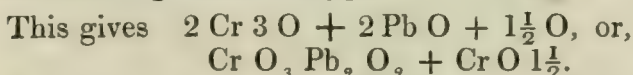


The compound obtained in this manner I considered very favourable for the conversion of chromic oxide into chromic acid. I heated it, and passed a stream of oxygen through it. However, to my great surprise, not the least alteration took place, and I found no increase in weight.

In the oxidation of the reduced salt, oxygen must have combined with the oxide of chrome, as 6.2 per cent. was in all absorbed, while the whole of the oxygen of the oxide of lead in the salt amounts only to 4.89 per cent.; and it is not at all likely that even the whole of this 4.89 per cent. was absorbed by the reduced lead, as it was for the most part fused into small globules, which must have very much prevented the action of the oxygen upon it.

I again fused 2.057 grammes of chromate of lead in an æther lamp supplied with oxygen: after a long-continued heat it lost 0.091 grm. or 4.4 per cent. The reduction was in this case carried on rather further than the conversion into basic chromate of lead and oxide of chrome. When, however, oxygen gas was passed over this compound very little of it was absorbed.

I at last prepared an intimate mixture of oxide of chrome and superoxide of lead; this was heated to redness and oxygen gas passed through it. This compound contained 0.445 grm. oxide of chrome. When the absorption had terminated, the increase of weight was found to be 0.066 grms. The colour was changed from green to brown. 0.455 grm. oxide of chrome contain 0.132 oxygen, therefore just double the quantity it had taken up. Therefore one atom of oxide of chrome upon heating with oxide of lead had combined with $1\frac{1}{2}$ atom of oxygen.



It is therefore the same compound which is formed when chromate of lead is fused by itself.

It follows from the foregoing experiments:

That chromate of lead is very easily converted by means of carbon and hydrogen into a mixture of oxide of chrome and metallic lead.

This mixture by heating is in a state to combine with oxygen; and this combination takes place not only with the metallic lead, but also with the oxide of chrome.

By heat alone it is very difficult to deprive chromate of lead of oxygen. It is at first converted into a mixture of basic chromate of lead and oxide of chrome: in order to reduce all the chromic acid into oxide of chrome, an uncommonly highly temperature is required.

When a mixture of oxide of chrome and oxide of lead is heated, it is also converted into basic chromate of lead and oxide of lead.

It is therefore chromate of lead, which is often employed in organic analysis, from which this latter mixture is derived.—*Journal für Praktische Chemie*, No. 2. 1840.

PORTRAITS IN DAGUERREOTYPE.

Professor Draper, of the University of New York, informs us in a note dated March 31st, that he has succeeded during the winter in procuring portraits by the Daguerreotype, and that they have all the beauty and softness of the most finished mezzotint engraving, and only require from 20 to 45 seconds for execution.

METEOROLOGICAL OBSERVATIONS FOR APRIL, 1840.

Chiswick.—April 1. Slight rain: cloudy. 2. Hazy: very fine. 3. Cold dry haze: frosty at night. 4—6. Very fine. 7. Fine: stormy showers at night. 8. Slight showers. 9. Cloudy and cold. 10—12. Very fine. 13—17. Fine but very dry. 18. Clear, hot and dry. 19. Hazy: very fine. 20. Very fine. 21—23. Cloudy and fine. 24. Very fine. 25. Very hot, nearly cloudless, and excessively dry. 26, 27. Hot and dry. 28. Excessively hot for the period of the season, thermometer 81° in the shade. 29, 30. Very fine: hot and dry. This month is remarkable for the limited quantity of rain and for a high temperature; the latter being the consequence chiefly of a powerful direct solar heat, which overcame likewise the counteracting effects of north and north-east winds, for they were in fact more prevalent than those from the opposite direction.

Boston.—April 1. Cloudy: rain P.M. 2. Rain. 3—5. Fine. 6. Cloudy. 7. Cloudy: stormy with rain P.M. 8. Cloudy: hail and rain P.M. 9—11. Fine. 12. Rain: rain early A.M. 13—19. Fine. 20—24. Cloudy. 25—29. Fine. 30. Cloudy.

Applegarth Manse, Dumfries-shire.—April 1. Mild day with a shower. 2. Keen and cold but dry. 3, 4. Dry and more temperate. 5. Fine day after a very slight shower. 6. Stormy day with showers, though slight. 7. Keen cold day. 8. More moderate. 9. Fine mild day. 10. The same: slightly moist and cloudy. 11. Drizzling all day, but very lightly. 12. Fine though cold: slight rain P.M. 13. Fine soft slight rain. 14. Charming spring day. 15. The same: with frost rime A.M. 16, 17. Fine but coldish: frost rime again. 18. Very fine warm day. 19. The same: white rime A.M. 20. The same: slight showers P.M. 21. The same: gentle shower. 22. The same: moisture. 23. Dry but threatening. 24. The same: cleared up. 25—28. Beautiful day. 29. The same, but cloudy. 30. The same: very warm.

Sun shone out 29 days. Rain, very slight, fell 6 days. Frost, rime 4 days.

Wind north 1 day. North-east $\frac{1}{2}$ day. East-north-east 2 days. East 3 days. East-south-east 1 day. South-east $\frac{1}{2}$ day. South-south-east 2 days. South 7 days. South-south-west 1 day. South-west $7\frac{1}{2}$ days. West-south-west 1 day. West $2\frac{1}{2}$ days. North-west 1 day.

Calm 15 days. Moderate 8 days. Brisk 3 days. Strong breeze 3 days. Boisterous 1 day.

Days of Month. 1840. April.	Barometer.			Thermometer.				Wind.			Rain.			Dew point. Lond.: Roy. Soc. 9 a.m.		
	London : Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London : Roy. Soc. Self-register.		Chiswick. 1 p.m.	Bost.	Dum- fries- shire.	London: Roy. Soc. 9 a.m.	Chiswick.		Boston.	Dumfries- shire.
		Max.	Min.		Max.	Min.	Max.	Min.								
1.	29.664	29.685	29.638	29.27	29.50	29.55	44.4	47.0 43.9	54	53	42	48½ 37	.02	42
2.	29.720	29.922	29.714	29.35	29.73	29.93	46.3	47.2 39.3	60	38	46	46½ 37½14	...	40
3.	30.024	30.063	30.012	29.65	30.02	29.94	42.3	50.0 41.0	53	27	44	51 3502	...	42
4.	29.960	29.980	29.918	29.52	29.85	29.90	39.8	49.0 34.4	51	32	43½	51½ 34	37
5.	30.044	30.062	30.012	29.58	29.92	29.76	45.2	45.8 39.6	59	35	45	52½ 39	39
6.	29.798	29.814	29.480	29.22	29.43	29.49	49.3	55.2 38.8	58	30	49	51 40½	0.05	40
7.	29.558	29.845	29.560	29.15	29.65	30.04	43.4	44.4 38.9	52	36	46	46 35	.02	41
8.	29.998	30.175	30.026	29.64	30.11	30.11	41.5	42.2 37.8	54	29	41½	45 33	.01	.07	...	38
9.	30.264	30.416	30.277	29.83	30.22	30.25	42.3	43.3 35.3	50	25	46	53 37	.01	.05	...	37
10.	30.408	30.409	30.373	29.89	30.24	30.12	44.7	45.4 36.4	56	25	46	50½ 44	34
11.	30.258	30.283	30.126	29.79	29.90	29.90	46.8	47.7 35.4	62	33	47½	48 44	36
12.	30.058	30.075	29.994	29.55	29.95	29.88	47.2	48.0 43.8	60	26	49	48½ 41	43
13.	29.918	29.950	29.860	29.49	29.80	29.78	45.0	46.0 39.7	68	26	51	55 42½14	...	38
14.	29.808	29.868	29.808	29.33	29.75	29.78	46.9	48.7 41.6	69	30	50	57½ 3912	0.20	40
15.	29.942	30.050	29.936	29.40	29.83	29.94	55.7	56.3 44.7	70	29	52½	62 40½	40
16.	30.108	30.185	30.095	29.48	30.06	30.23	55.7	56.4 44.6	70	37	54	54 40½	42
17.	30.188	30.204	30.105	29.65	30.27	30.16	50.5	51.6 40.9	62	32	46	54½ 36½	44
18.	30.040	30.164	29.992	29.65	30.05	29.93	47.0	47.7 38.2	64	33	47	58 34	43
19.	29.922	29.927	29.884	29.45	29.89	29.82	42.8	43.5 39.0	64	29	48½	60½ 33	40
20.	29.898	29.974	29.886	29.36	29.73	29.75	51.2	51.6 42.0	69	41	52	57½ 39	0.03	41
21.	30.074	30.142	30.060	29.44	29.78	29.78	53.8	54.8 47.0	62	46	54	56 44	44
22.	30.234	30.337	30.212	29.56	30.08	30.08	58.2	58.8 49.3	66	42	57	54 38	49
23.	30.348	30.337	30.305	29.62	30.10	30.09	58.4	59.2 50.0	69	49	58	63 49	50
24.	30.298	30.277	30.126	29.58	30.08	30.05	58.3	58.8 50.4	75	41	57	58½ 46½	51
25.	30.174	30.223	30.112	29.51	30.03	30.00	61.8	66.4 50.7	80	40	62	67 40½	53
26.	30.312	30.369	30.280	29.64	30.14	30.25	60.5	66.2 51.5	77	42	61	64½ 47½	54
27.	30.356	30.340	30.297	29.80	30.28	30.20	62.5	66.8 51.2	76	42	61	67½ 41½	0.00	55
28.	30.300	30.292	30.257	29.75	30.11	30.04	59.7	67.0 48.7	81	40	62	70½ 43	52
29.	30.298	30.323	30.268	29.70	30.14	30.18	67.0	67.0 54.0	79	39	61	62½ 45	52
30.	30.324	30.338	30.238	29.75	30.20	30.22	59.2	72.2 52.0	74	46	58½	67½ 50	51
Mean.	30.077	30.133	30.028	29.55	29.094	29.071	50.9	53.5 43.3	64.80	34.86	51.2	56.40.2	.06	.54	0.28	Mean. 49.6

THE
LONDON AND EDINBURGH
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE,
SUPPLEMENT TO VOL. XVI. THIRD SERIES.

LXXXIII. *On Galvanic Circuits composed of two Fluids, and of two Metals not in contact.* By Professor J. C. POGGENDORFF.*

[Continued from p. 498, and concluded.]

WITH all combinations in which zinc, iron, or tin forms the positive metal, even with some in which amalgamated zinc occupies that place, the water has the ascendancy over the (dilute) hydrochloric acid, as already observed by Fechner with the combination zinc-copper in a somewhat more complex experiment, his *experimentum crucis*†.

But water also exhibits this ascendancy, in the greater number of cases, over sulphuric acid‡; with the combinations zinc-platina, zinc-tin even more strongly than over hydrochloric acid, exceedingly strong, especially with amalgamated zinc and recently heated platina. And yet sulphuric acid is no electrolyte.

Nor is nitric acid an electrolyte, and nevertheless it so alters the electromotive force that with zinc-platina it has the ascendancy over water, but in the other combinations with zinc and tin as positive member it succumbs to it.

Similarly circumstanced is ammonia, which is likewise no electrolyte. In all combinations with iron and tin as positive member, it always succumbs to water; in those with *zinc*, amalgamated or pure, it has on the contrary (copper excepted) generally the ascendancy.

Solution of chlorine affords a similar example. The difference of its action from water and from hydrochloric acid is sufficiently evident from the table; and yet it can hardly be classed among the electrolytes.

The assertion of Vorrsselman de Heer that only electrolytes are capable of changing the electromotive force, is therefore certainly incorrect.

* From Poggendorff's *Annalen*, vol. xlix. January, 1840: translated by Mr. W. Francis.

† Lond. and Edinb. Phil. Mag., vol. xiii. p. 374.

‡ As I have already shown, although less distinctly, in another way for the zinc-copper circuit. *Annalen*, vol. xlv. p. 405.

Just as little, in my opinion, do the facts speak for Faraday's theory. A stronger affinity must undoubtedly be attributed to the *chlorine* for *zinc*, *iron*, and *tin* than to the oxygen; and nevertheless, in circuits containing one of these metals as positive member, *hydrochloric acid* does not act more *strongly*, but more *weakly*, than water*.

In the dilute sulphuric and nitric acids, according to Faraday, only the affinity of the oxygen of the water acts on the positive metal. Consequently these acids, when water acts opposed to them, must either produce no current, or, if the above-mentioned affinity is increased by their presence, developé, as is otherwise generally admitted, a *more powerful* electromotive force than pure water. Nevertheless the two acids act, in the greater number of cases examined, more *weakly* than water†.

The same observation may be applied to *caustic potash* and *ammonia*. In Faraday's "*Experimental Researches*" we find it stated, § 919, "Thus, when zinc, platina, and dilute sulphuric acid are used, it is the union of the zinc with the oxygen of the water which determines the current;" further, § 932, "The similarity in the action of either dilute sulphuric acid or potassa goes indeed far beyond this, even to the proof of identity," and § 933, "But all the effects in these experiments prove, I think, that it is the oxidation of the metal necessarily dependent upon, and associated as it is with, the electrolyzation of the water, that produces the current; and that the acid or alkali merely act as solvents by removing the oxidized zinc."

But in truth the caustic alkalies always act differently from *water* and differently from acids‡, and that immediately, in the first moment of immersion, when the zinc is still in possession of its full metallic lustre. There is, indeed, a certain relation to the attackability of the positive metal apparent, as is evident for

* It also acts more weakly than sulphuric acid, at least with amalgamated zinc and platina or silver. (See Note, p. 423.)

† Becquerel observed years ago (*Ann. de Chim. et de Phys.*, vol. xli. p. 17) that when zinc and copper are placed in two cells separated by membrane and filled with a solution of the sulphate of zinc, and then some nitric acid is poured into one of them, this addition in the copper cell heightens the current, but in the zinc cell weakens it. This experiment, certainly, when correctly apprehended, as already observed by Berzelius (*Jahresbericht*, No. x. p. 23), is no argument in favour of the chemical theory. Similar facts, have moreover been noticed by Fechner (*Annalen*, vol. xliii. p. 433).

‡ This indeed has not escaped Faraday. In § 941, he says, "The alkali, in fact, is superior to the acid in bringing a metal into what is called the positive state;"—but why? it may be asked; and how does this agree with § 921? in which it is stated, "Oxidation or other direct action upon the metal itself is the source of the current, but it is of the *utmost importance* to observe that the oxygen or other body must be in the state of combination, and indeed in such a state of combination as to constitute an electrolyte."

instance from the comparison of the *iron* circuits with *zinc* and *tin* circuits; but why *both alkalies* with the iron circuits, and the *ammonia* with tin circuits have in general a *weaker* action than *water*,—why, even with the combinations *zinc-platina*, *zinc-silver*, the alkalies have the ascendancy over the water, is according to that view not conceivable.

It is the same in all those cases in which two oxy-combinations are opposed to one another: *water* and *carbonate of soda*, *sulphuric acid* and *borax*, *sulphate of zinc* and *borax*, *sulphate of magnesia* and *borax*.

Perhaps here, from the carbonate of soda and borax being easily decomposable salts, and their solutions in the cases examined always succumbing to that of the second oxy-combination, a position from the *Experimental Researches* might be brought forward (§ 549) according to which substances are said to produce a more powerful current the more difficultly decomposable they are, and *vice versâ*. Thus might also be explained the general weaker action of dilute acids in comparison to water, since acidulated water is more easily decomposable than pure. However, this position appears hardly in unison with the fundamental principle of the chemical theory, that the affinity of the oxygen, chlorine, &c., for the zinc, is that which excites the current; for it might rather be supposed that this affinity can the less enter into activity the more strongly the oxygen, chlorine, &c., are retained by the positive element from which they are to be separated. Moreover, it cannot be applied to the alkalies, which in most cases increase the electromotive force and yet at the same time render water more decomposable than it is of itself. A further example, among many others, is afforded by *salt* and *sal-ammoniac*, two chlorides, the solution of the latter of which develops a stronger electromotive force than that of the first. It can hardly be admitted that *sal-ammoniac* is of more difficult decomposition than *salt*!

The solution of chlorine likewise, if I correctly understand Faraday's view, could not, according to it, act differently from pure water; for the chlorine is merely dissolved in the water, not combined with a body to form an electrolyte. Nevertheless it acts with *zinc-platina*, for instance, much more strongly than water. It acts, moreover, instantaneously on immersion, so that the action can scarcely proceed from formed chloride of zinc, which is also evident from the fact, that solution of chlorine, which has been frequently employed, containing therefore a considerable quantity of chloride of zinc, does not act in the least more powerfully than when pure.

If, on the other hand, the action is derived from the chlorine, then again, according to this view, it cannot be conceived why

the *solution of chlorine* acts more powerfully than *hydrochloric acid*, in which, nevertheless, the chlorine is electrolytically combined with hydrogen, and without doubt is more strongly retained by the *hydrogen* than in the solution of chlorine by the *water*.

It might perhaps be replied, that the solution of chlorine is not a mere solution of the chlorine, but a mixture of hydrochloric acid and the oxide of chlorine; but setting aside that this supposition is not proved, is not even very probable, and is accompanied by a second supposition not more probable, that this mixture or the oxide of chlorine is of more difficult decomposition than hydrochloric acid, we need only call to mind the other cases where *non-electrolytes* exert a sensible influence on the development of the electromotive force,—the actions of *sulphuric acid*, of *nitric acid*, *ammonia*, *water* impregnated with *oxygen*, of the *peroxide of hydrogen**, of the *quinto-sulphuret of potassium* ($K S_5$)—to perceive that such a supposition is neither required nor can be generalized.

The position, that those bodies which, brought between the metallic plates of a voltaic pile, render it active, are all electrolytes (*Exp. Res.* § 858, 921), must therefore be thus altered, that the fluids between the metallic plates must, it is true, be electrolytes, *i. e.* decomposable bodies, since, at least with aqueous fluids and with a certain intensity of current, no conduction can take place without decomposition; but that the electromotive force which is developed on the contact of these fluids with the metals is not in any necessary connexion with the conductivity or decomposability, and can be increased or diminished by bodies which are not electrolytes, *i. e.* not directly decomposable†. I shall hereafter communicate another proof on this point.

Not less difficult for the disputed theory would be the cases in which *hydrochloric acid* and *sal-ammoniac*, *hydrochloric acid* and *salt*, or *salt* and *sal-ammoniac* are opposed to each other. In each of them, according to it, only the chlorine could act on both sides, and consequently no current arise. But I will pass over the discussion of these cases and turn to the *iodide of potassium*, from which Faraday has derived the new argument in favour of the chemical theory.

In the preceding tables I have enumerated the action of the iodide of potassium in 17 metallic combinations, both towards

* Becquerel, *Ann. de Chim. et de Phys.*, vol. xxviii. p. 19.

† De la Rive and other supporters of the oxidation-theory, admit also, as is well known, that it is the oxidation of the zinc alone, no matter whether it be caused by the oxygen of the atmosphere absorbed by the water or any other action, which determines the current.

sulphuric acid and towards *hydrochloric acid*; in the whole, therefore, 34 cases. In 18 cases the acid had the preponderance; in 10 the iodide of potassium. In 6 the result was ambiguous, as during the experiment a decided reversion of the current ensued; and probably some of the first belong to this category. If we merely look at the *number* of favourable cases, the affinity-theory would indeed still retain some probability; but this must entirely disappear if at the same time we take into consideration the *weight* of the unfavourable cases.

Among the latter, viz. the unfavourable cases, those with platina more especially surprised me, and of these that with the combination *zinc-platina* and *hydrochloric acid*. It is in fact the same experiment which Faraday mentions in support of his view, which he himself had the kindness to show me in London (in this experiment *hydrochloric acid* was employed), and which I also had previously more than once performed with the same positive result. Whence now the opposite result? An error it could not be; I had repeated it so often and under the most varied circumstances! After some experiments I was so fortunate as to find the solution of this enigma; and with this, at the same time, the untenability of the explanation of the experiment, as also of the whole of this argument in favour of the chemical theory of Galvanism, is, in my opinion, clearly demonstrated.

The result of the experiment, in fact, depends solely on the *concentration* of the acid*. If we employ, as was the case in all the experiments enumerated in the table, a hydrochloric acid of 1.138 spec. gr., diluted with 6 times its volume of water, the iodide of potassium has the preponderance; but if, on the contrary, the same acid be employed but not diluted, the superiority is then on the side of the acid; and the current has such a direction, that on the decomposition of the iodide of potassium, the iodine passes to the platina. Between these two degrees of concentration of the acid, there will evidently be one with which not the least current will occur†.

* Probably also on the concentration of the solution of the iodide of potassium; but I have not varied this.—On the other hand, I have assured myself, that the purity of this solution has little influence. A solution which from frequent use was mixed with sulphuric acid, had thus become brown and acid, and contained therefore free hydriodic acid with iodine, and dissolved zinc with effervescence,—acted just the same as pure iodide of potassium, and evinced with platina the superiority over hydrochloric acid more strongly than the latter.

† Likewise with *silver*, *copper*, and *tin*, combined with *amalgamated zinc*, the concentrated *hydrochloric acid* (of 1.138 spec. gr.) has instantaneously and strongly the ascendancy over the iodide of potassium; even the dilute acid with *silver* and *copper* acts in the same way, as shown in the table; with *tin*, however, it succumbs to the iodide of potassium. The action of *tin* is therefore quite analogous to that of *platina*.

It is not to be supposed that the acid, thus diluted, is too weak to act on the zinc; on the contrary, it attacks it very violently, so energetically indeed that it becomes sensibly warm. The plea, that with the dilute acid the affinity of the chlorine for the zinc has become from the presence of the water weaker than that of the iodine for the same metal, is therefore inadmissible.

Moreover, the same acid has with *silver* and with *copper*, no matter whether combined with *zinc* (common or amalgamated) *iron* or *tin*, in a high degree the ascendancy over the solution of the iodide of potassium,—a phenomenon which, compared with the opposite one on employing *zinc-platina*, at the same time clearly proves what essential part the *two* metals of the circuit take in the production of the current, and how contrary to nature, therefore, that view is, according to which, in reference to the voltaic current, the positive metal is termed the *generating* and the negative the *conducting* metal*.

Similar phenomena, not favourable to the theory, are evinced by the circuits of *sulphuric acid*, *iodide of potassium*, *platina* and a positive metal, by which, moreover, when the latter consists of *zinc*, especially non-amalgamated, those remarkable reversions in the direction of the current occur which have already been noticed in the table under No. 10.

An experiment performed specially for this purpose, and continued longer than usual, made me better acquainted with them. Bright filed plates of distilled zinc and non-heated plates of platina were employed for this experiment. The former remained during the whole time in the fluid; the latter were simultaneously immersed and taken out. During the first immersion, the current proceeded constantly in the direction $s > i$.

* It is not very intelligible how this notion can have gained ground, since well-proved facts have long ago shown quite the contrary. If the negative metal in the circuit had merely to act as it were a passive part, to perform merely the function of *conducting*, then evidently the best conductor must produce the strongest current, or, rather, the greatest electromotive force. Copper conducts decidedly better than platina; but yet the latter, in combination with a positive metal, gives rise to a far greater electromotive force than the former. How essential the negative metal of the circuit is in *generating* the current, is most decidedly evident from the position first established by Fechner (Schweigger's *Journ.* vol. lx. 1830, p. 17, and Poggendorff's *Annalen*, vol. xliii. p. 433), that, as soon as the fluids do not act very alteringly on the metals, the voltaic law of the tensions is also valid for the electromotive forces of the circuit, that therefore, for instance, the electromotive force of a *zinc-platina* circuit is equal to the sum of the electromotive forces of a *zinc-copper* and of a *copper-platina* circuit. The law naturally cannot be applied to the intensity of the currents. The current from *copper-platina* is, as I have convinced myself, by far weaker than the difference of the currents from *zinc-platina* and *zinc-copper*,—easily imaginable from the inequality of the resistance of transition.

The first oscillation of the needle had the amplitude $80^\circ - 45^\circ$ (*i. e.* went from 80° on the one side of the meridian to 45° on the other). The deflexions, however, decreased rapidly, and when they had sunk to $12^\circ - 0^\circ$ the platina was removed. This washed, dried, and again inserted, produced a current in the direction $s < i$. The first deflexions were $90^\circ - 85^\circ$; the following, $85^\circ - 80^\circ$; $80^\circ - 75^\circ$, &c., till at last $6^\circ - 3^\circ$, when they were again taken out. The third, fourth, fifth, &c., immersion gave all of them currents in the direction $s < i$, only commencing with slighter deflexions.

At the same time two remarkable circumstances occurred here. At the commencement, namely, the (distilled) zinc, as is always the case when its surfaces are very bright, was but very slightly attacked by the dilute sulphuric acid, which also in this case consisted of 1 vol. concentrated acid, and 9 vol. water; but the longer it remained in it the stronger the action became, so that at last the disengagement of gas was very lively. At the commencement, the vibrations of the needle, although great, were nevertheless quite regular; but in proportion as the evolution of gas increased, sudden convulsions were evident in the vibrations, which became greater and greater, and at last passed into actual starts of 30, 40, 50, 60, 70, &c. degrees, and thus far exceeded the commencing deflexions which even on the fourth immersion did not amount to more than 10° . All these starts took place in the direction $s < i$. Their progressive increase evidently pointed to an increasing ascendancy of the iodide of potassium over the acid.—The second notable circumstance was, that with each taking out of the platina, although both plates were removed at the same time, a strong deflexion (of 90°) likewise in the direction $s < i$ resulted. This phenomenon was especially surprising at the close of the first immersions, as the needle then made but very small and quite regular vibrations*.

I now repeated the same experiment (zinc filed bright, platina not heated) with a *stronger sulphuric acid* (1 vol. concen-

* I frequently observed similar starts, and especially when, on employing *ordinary zinc*, I endeavoured to find out whether sulphuric acid or hydrochloric acid (both of the degrees of dilution mentioned at p. 489.) would develop the greater electromotive force. With silver, as negative metal, the convulsions and starts of the needle were so powerful that I could not decide as to the direction of the current. With *platina* these disturbances did not occur, because it was first examined, and the acids did not then act so violently on the zinc. With *copper* and *tin* they vanished in comparison to the force of the main effect; for with *copper*, the *sulphuric acid*, and with *tin*, the *hydrochloric acid*, had in the highest degree the superiority. Subsequent experiments, partly with bright filed distilled zinc, partly with ordinary, but amalgamated, showed me that also with *platina* and *silver* the sulphuric acid has the ascendancy, although in a far less degree.

trated acid with 4 vol. water, or in weight 1 and 2 parts). In this case there was no indication of $s > i$; the first deflexion occurred even with the first immersion of the platina in favour of $s < i$, and indeed very violently $= 90^\circ$. All the succeeding effects were likewise powerful, and in the same direction. The deflexion was, with slight vibrations, 80° , 70° , 60° , and so on, till at last about 20° , from whence, remarkably enough, it again increased. The amplitude of the last observed vibration went from $70^\circ + 30^\circ$.

The violent action of the acid on the zinc obliged me to terminate the experiment; but it was immediately recommenced with *amalgamated zinc* and the *same acid*. At present the current was nearly zero; only a slight deflexion of about 4° betrayed an inclination to $s < i$.

I now heated the platina, and indeed only that plate which was to stand in the acid. On contemporaneous immersion of both plates (of which the one heated had naturally, as in all similar experiments, perfectly cooled) a deflexion of 90° in the direction $s > i$ first resulted, immediately succeeded by one likewise of 90° in the direction $s < i$, and the needle now vibrated on the same side of the meridian, successively about the points 85° , 80° , 75° , 70° , 60° , 50° , 40° , till at last, after some minutes, it merely indicated a permanent deflexion of 2° , but still always in the direction $s < i$.

The heating of the platina was at present performed on the other plate, which was to be placed in the iodide of potassium. The result on immersion was of the same kind, but considerably less in strength than the previous one. The first deflexion in favour of $s > i$ amounted only to 5° , and the one immediately succeeding, in the direction $s < i$, to 22° only, upon which the needle then soon came to rest. A second heating of the plate to be placed in the acid, produced again the same result in its whole force. The first deflexion, in the direction $s > i$, was 90° ; the second, in the direction $s < i$, likewise 90° , and the current now retained this direction with great energy, which diminished but very slowly*.

* This phenomenon also appeared when dilute hydrochloric acid was employed, and to a far greater extent. If, after the effect has decreased to zero, the plate standing in the acid be taken out, washed, heated, and, after cooling, re-immersed, a movement of 90° , in the direction $s < i$, is immediately obtained, succeeded by a highly permanent deflexion in the same direction. If after the deflexion has again descended to nearly zero, we perform the same operation with the plate inserted in the iodide of potassium, this has but a very weak, in most cases, no result. Heating of both plates acted as when this was performed solely with the first. I likewise observed on this occasion that heated plates of platina almost wholly lose their remarkable effect by being suspended for some hours in the atmosphere.

I have communicated these details in order to show that the phænomena in circuits of the kind described are by no means always so simple as it seems they should be according to the affinity-theory. That, moreover, they do not speak for it is undoubtedly evident enough. Only at first and transitorily has the sulphuric acid the ascendancy over the iodide of potassium; subsequently this acid, although it incontrovertibly attacks the zinc more violently than the iodide of potassium (even with energy increasing during the course of the action), is always overpowered by it; nay, what is remarkable, the less dilute acid (with 4 times its vol. of water) succumbs to the iodide of potassium to a greater extent than the acid diluted with 9 times its vol. of water. How is all this to be explained from the relative affinity of the oxygen and iodine to the zinc?

The above facts appear to stand in contradiction to the experiment of Faraday, mentioned at p. 487, in which a current was obtained with sulphuric acid, strongly overpowering the iodide of potassium. However, this contradiction is merely apparent; for, what was not there observed, the sulphuric acid was not pure, but purposely mixed with some nitric acid. A sulphuric acid, containing nitric acid, has in fact, (of which I have convinced myself,) in a high degree the superiority over the iodide of potassium; a powerful and permanent deflexion in favour of $s > i$ is immediately obtained, and at the same time there may be distinctly observed, from the yellow coloration of the solution of the iodide of potassium around the platina plate, the separation of the iodine*.

If, nevertheless, this fact is still employed, after so many proofs against the affinity-theory, as an argument in favour of it, it may then with justice be asked, Why then does the sul-

* A dilute sulphuric acid, consisting of 1 vol. acid of 1.827 spec. gr., and 4 vol. water, to which was added an eighth of its volume of nitric acid of 1.321 spec. gr., was used for this experiment. A more dilute mixture, consisting of 12 parts in weight of dilute sulphuric acid (1 vol. concent. acid, and 9 vol. water), and one part in weight of the above nitric acid, gave by far weaker results. *Platina*, combined with *zinc*, produced, it is true, a current in the direction $s > i$; but even after it had been heated, it did not equal the current from *silver-zinc* or *copper-zinc*. With the combination *tin-zinc* the current had the direction $s < i$, as with pure sulphuric acid.

Dilute nitric acid alone (p. 489) likewise gives rise to similar effects. *Copper*, *silver*, *heated platina*, combined with *zinc*, immediately produced a powerful current in the direction $s > i$. With *non-heated platina* the direction of the deflexion was the same, but the intensity only slight, merely 5° ; it increased, however, perceptibly, and without any oscillation the needle slowly rose to 45° , where it remained. With *tin* the direction of the current was the reverse, *i. e.* the iodide of potassium had the superiority, and indeed strongly. This is the more remarkable, as I convinced myself that the tin in the same acid is highly negative to the zinc.

phuric acid, without this addition of nitric acid, afford in most cases the opposite result? Want of chemical action on the zinc it certainly is not! And then, how, even according to the affinity-theory, is the action of the nitric acid to be explained?

A long discussion might here be opened; I will, however, merely touch upon one point. Faraday states that the addition of nitric acid to the sulphuric acid increases the *intensity of the chemical action*; and, after communicating some facts from which he draws the conclusion that this acid does not increase the *quantity* of the electricity, he adds: "This mode of increasing the intensity of the electric current, as it excludes the effect dependent upon many pairs of plates, or even the effect of making any one acid stronger or weaker, is at once referable to the conditions and force of the chemical affinities which are brought into action, and may, both in principle and practice, be considered as perfectly distinct from any other mode*."

Here we may with justice put the question, What measure then do we possess for the intensity of a chemical action? When the question is, as to the attack of an acid on a metal, we have, I believe, no other measure than the quantity of the metal which is dissolved from the unity of surface in the unity of time. But with this, certainly the most natural, view, there exists no reason why the nitric acid should enjoy any single advantage over the sulphuric acid, when these acids are taken of such a degree of concentration that they both dissolve just the same quantity of a like zinc surface in the same time. An advantage, according to Faraday's theory, is the less to be expected, as both acids are non-electrolytes, and their effect therefore could only be of like nature, and merely consist in increasing the affinity of the oxygen of the water for the zinc†. But

* Faraday's Experimental Researches, § 908. It may be observed *en passant* that what the Faradayan theory terms the increase of the *quantity* of the electricity is the same as heightening the force of the current by diminishing the resistance; for instance, enlarging the surfaces, increasing the concentration of the fluids, therefore precisely the same as diminishing the denominator of Ohm's formula. By *electrolytic intensity*, or *intensity* of the electricity, this theory on the other hand understands, at least with the simple circuit, the electromotive force, or the numerator of this formula. But both expressions are sometimes used in a different sense, of which I have already given an example in the *Annalen*, vol. xlvii. p. 128^a, and of which the explanation of the difference between the current of the pile, and that of the simple circuit (Exp. Res. § 99†),—an explanation so perfectly simple, according to Ohm's theory,—gives a further proof.

† However, with the nitric acid, whether employed alone, or mixed with sulphuric acid, the process is not so simple, even with the above-mentioned

[^a A Translation of the Memoir of Ohm has just appeared in Part VII. of the Scientific Memoirs.—EDIT.]

since, nevertheless, a specific distinction remains between the effects of the two acids, the one, added to the water, developing a slighter, and the other a greater electromotive force than the iodide of potassium, we should be forced to admit that the *quality* of the chemical action produces a specific difference in the excited electricity, and should thus again be brought back to the position maintained by De la Rive, but hitherto not proved, of the variety of electricities. I know not whether this is the opinion of the English philosopher; but the above-mentioned position, and another in which he expresses as a conjecture, "The same quantity of electricity may pass in the same time, in at the same surface, into the same decomposing body in the same state, and yet, differing in intensity, will decompose in one case, and in the other not,"*—would admit of such a construction.

But be this as it may, so much is certain, that there is no need of the hypothesis of an increase of the intensity of the chemical action, in order to explain the experiment in question. I have in fact convinced myself in the most positive manner *that the result of the addition of the nitric acid does decidedly not arise from the chemical attack of this acid on the zinc, but solely from an action of it on the platina.*

Instead of placing the *zinc* and *platina* in common in the stronger mixture of acids mentioned at p. 545, I separated the two acids by animal membrane (bladder), inserted the zinc (amalgamated) in the sulphuric acid (1 vol. concentrated acid, and 4 vol. water), and the platina in the nitric acid (1 vol. concent. acid, and 6 vol. water), while the two other plates, zinc and platina, stood in the solution of iodide of potassium. Now although in this case the zinc underwent no other attack in quantity and quality than in the experiment mentioned at p. 543, in which the iodide of potassium had the ascendancy over the sulphuric acid, yet the direction of the current was the reverse; the iodide of potassium succumbed to the acid. The current also possessed a very considerable intensity, and, if not quite so powerful as in the case in which the zinc stood in the acid mixture, this, evidently, merely arose from collateral circumstances, partly from the separated acids having perhaps a weaker power of conduction than the mixed, partly, and without doubt chiefly, from the metals in the present arrangement being in a somewhat disadvantageous position, the communication between the two being made only by the membranous

moderate degrees of concentration; for it is at least in part decomposed, which is indicated by the altered development of gas at the zinc, and more decidedly evident from the ammonia, the existence of which in the solution of zinc may be distinctly demonstrated by the addition of an excess of caustic potassa.

* Faraday, Experim. Res. § 988.

bottom of a cylinder which contained the platina and nitric acid, and was surrounded by a wider one which received the zinc and sulphuric acid.

To be perfectly certain that the mixed acids did not develop greater electromotive force than the separated, I caused them to oppose one another by substituting in the apparatus just described an acid mixture for the iodide of potassium. The experiment was in other respects similar to the previous one, only that the acids had a somewhat different degree of concentration. Both diluted acids consisted of 1 part by weight of concentrated acid, and 3 parts by weight of water, and equal parts by weight of them were mixed on the one side of the circuit with each other, and separated on the other side by bladder. The experiment was made both with amalgamated and non-amalgamated zinc, and previously heated platina.

In both cases the result was, *that the separated acids not only excite an electromotive force quite as great as the mixed, but have indeed a slight superiority over these!* The latter fact is the more remarkable, as the zinc plate (even amalgamated), immersed in the sulphuric acid containing nitric acid, is evidently more strongly attacked than that in the pure acid, and yet, after washing both in water, is in this fluid negative towards the latter plate.

I consider these facts, indeed, as more demonstrative than those already mentioned at p. 541, with the hydrochloric acid; nay, as so decisive, that I regard the proofs against the tenability of the argument, derived from Faraday's experiment, in favour of the chemical theory of galvanism, as perfectly destroyed by them*.

However I cannot refrain from drawing attention to the cir-

* The above fact is certainly decisive against the Faradayan theory, which merely admits the chemical attack on *metals* as the cause of the voltaic electricity. On the other hand, according to the theory of Becquerel or De la Rive, one might deduce this near equality in the action of the separated and mixed acid from an accidental compensation with the current originating from the contact of both acids. Now fluids excite, it is true, an electric current by their reciprocal contact, as was first *actually proved* by Fechner (Poggendorff's *Annalen*, xlviii. pp. 1. and 225.). A portion of the action may therefore in effect have originated from this cause; but since the currents, which truly originate from the reciprocal contact of the fluids, are always weak only, it is not probable that this portion was considerable, and exercised any great influence on the main result. As already mentioned, the separated acids have the superiority over the mixed when the *platina* is inserted in the nitric acid; the reverse happens when the *zinc* is immersed in the nitric acid. But in both cases the superiority is only slight. This appears to me to prove, that the current from the fluids which in both cases must possess opposite direction, has no considerable part in the main action.

cuits composed of *acid, iodide of potassium, amalgamated and non-amalgamated zinc.*

As evident from the Table, the current has in these circuits, on employing *pure sulphuric acid* or *hydrochloric acid* in the diluted state, after a first deflexion in the direction $s < i$, the direction $s > i$ with great energy, or the acid the ascendancy over the iodide of potassium. The same is the case, and indeed without the first deflexion $s < i$, when *sulphuric acid containing nitric acid* or *pure concentrated hydrochloric acid* (spec. gr. 1.138) is employed.—In all these cases, therefore, the *non-amalgamated zinc* acts as a negative metal, for instance like silver, towards the *amalgamated*; and yet it is always attacked far more energetically than the latter; by the concentrated hydrochloric acid, indeed, with a truly stormy violence. How can this be explained in a satisfactory manner according to the chemical theory?

I say in a satisfactory manner; for the explanation which Faraday has given of the cause of the positiveness or greater activity of the amalgamated zinc in comparison with the unamalgamated,—namely, that the latter, being directly attacked by the acids, neutralizes them by the oxide it produces, and thus retards the progress of oxidation, whilst at the surface of the amalgamated zinc the oxide formed is instantly removed by the free acid present, and the clean metallic surface is always ready to act with full energy upon the water*,—can scarcely be termed satisfactory, as it is in open contradiction to experience, which shows that under like circumstances by far more of the unamalgamated than of the amalgamated zinc is dissolved.

Just as little can the doctrine of *local* and *circulating* chemical forces, and the assumption that the latter are produced in greater energy or quantity by the amalgamated zinc than by the unamalgamated†, be admitted here, or generally, as valid. This doctrine possesses, it is true, such pliancy, that by it all the numerous cases, where, as in the experiment of Berzelius (p. 486.), the negative metal is more violently attacked than the positive, may be set aside, with the explanation that it is effected by a local action which adds nothing to the current; but, on a closer view, it is nothing more than a gratuitous hypothesis, which the chemical theory finds itself compelled to adopt in order not most palpably to fall into a contradiction with the fact, that the energy of the electromotive force no ways corresponds to the violence of the attack on the zinc or positive metal. Where is there any proof of this? It is as much in want of one, as

* Exp. Res. § 1005.

† Exp. Res. §§ 947, 996, 1120.

the hypothesis imagined by De la Rive to get rid of the same difficulty, viz., that the electricities separated by the chemical process find a partial reunion at the very place where they are developed, and consequently the intensity of the current need not necessarily stand in direct proportion to the energy of this process,—an hypothesis which has already been dissected by Fechner*, and, in my opinion, founders even upon this ground alone, that, *cæteris paribus*, the current is the more intense the better the fluid conducts, *i. e.* the easier this reunion can take place in it.

It is certainly an advantage of the contact-theory that it needs neither the one nor the other hypothesis, but is perfectly satisfied with the simple view, that the so-called local action, that which happens even previously to the closing of the circuit, is a pure chemical process not at all appertaining to the circuit: but the advantage were but slight, if it could merely enumerate in its favour the simplicity of this view; its real superiority over the chemical theories it acquires from its being a view well founded on facts. All cases more accurately examined, whether it be in the present Memoir or in previous ones by Fechner† and others, *prove* in the most evident manner that the energy of the direct chemical attack of the fluid on the positive metal does in no way stand in any connexion with the intensity of the excited electromotive force. And on the other hand it is *not proved* that the local action is ever converted into circulating, or weakened by it‡. What has been advanced as such, is evidently founded on error. The decrease of the hydrogen at the zinc which results on the closing of the circuit, does not happen from a *transfer* of *this* hydrogen to the negative metal, but simply from the oxygen being carried by the current to the zinc, and there combining with the hydrogen. I hope shortly to be able to confirm this by facts.

The following note has been communicated by M. Poggendorff to the Translator, with a request that it should be added:

As I did not foresee on penning this memoir that it would have the honour of being translated into English, it may be necessary to observe that I had not the intention of bringing forward a complete refutation of the chemical theory of galvanism, but merely to show that two facts recently brought forward in favour of this theory do not prove what it is intended they should. For the same reason many things have been passed over, or but briefly noticed, which appeared unnecessary for Germany, but which needed a more detailed exposition for the English readers, who in general are unacquainted with the researches of Ohm and Fechner. Among others, might here be enumerated the distinction between *electromotive force* and *intensity of current*, the not taking which into considera-

* Poggendorff's *Annalen*, vol. xlv. p. 232.

† For instance, Poggendorff's *Annalen*, vol. xliii. p. 433.

‡ Exp. Researches, § 996.

tion has already given rise to so many errors; further, the resistance of *opposition*, an element hitherto little attended to in England*, which is not merely in itself of importance, but to which especial regard must be had in the discussion of the two views respecting the origin of *voltaic* electricity, as it is considerably affected by the chemical action. Very generally the heightening of the intensity of the current by direct chemical attack on the one or other metal of the circuit is merely due to the diminution of the resistance of transition, and is not a result of the increase of the electromotive force. But it must be proved with respect to this force, that it is in direct ratio to the energy of the chemical action on one of the metals (or to the difference of the actions on both metals) of the circuit, if the chemical theory is to be regarded as founded. It would, however, then be requisite to continue to separate and quantitatively to determine the individual elements which have any influence on the intensity of the current, in the same way as it has been done by Fechner in his work, "*Maas bestimmungen über die Galvanische Kette* (Leipzig, 1831, 260 pages in quarto). I am convinced that the English physicists, to whose zeal and ability we are already indebted for so many interesting facts in the field of galvanism, would add considerably to the extension of our scientific knowledge of this branch of physics, and would consider Fechner's memoir on the Contact-theory† in a different point of view were they more intimately acquainted with that work, based on Ohm's theory, and illustrating and extending it.

POGGENDORFF.

* By which, among other things, the recent, important, and interesting discovery of Mr. Roberts (See L. & E. Phil. Mag. for Feb.) finds its explanation.

† Besides the article on the Contact-theory, translated in the Phil. Mag., might be mentioned two others in Poggendorff's *Annalen*, vol. xlv. p. 432, and vol. xlix. p. 433. What Schœnbein has brought forward against it does not appear to me to have any weight, and indicates his non-acquaintance with Ohm's theory.

LXXXIV. *Contributions to Electricity and Magnetism*. No. III.
On Electro-dynamic Induction. By JOSEPH HENRY, LL.D.,
Prof. of Natural Philosophy in the College of New Jersey,
Princeton.

[Continued from p. 265, and concluded.]

SECTION VI.—*The production of induced Currents of the different Orders from ordinary Electricity.*

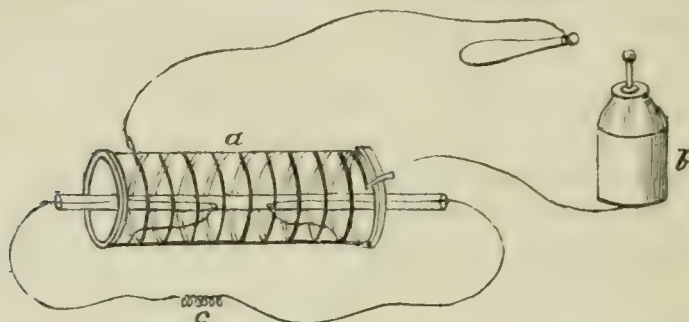
98. **D**R. FARADAY, in the Ninth Series of his Researches, remarks, that "the effect produced at the commencement and the end of a current (which are separated by an interval of time when that current is supplied from a voltaic apparatus) must occur at the same moment when a common electrical discharge is passed through a long wire. Whether if it happen accurately at the same moment they would entirely neutralize each other, or whether they would not still give some definite peculiarity to the discharge, is a matter remaining to be examined."

99. The discovery of the fact, that the secondary current, which exists but for a moment, could induce another current

of considerable energy, gave some indication that similar effects might be produced by a discharge of ordinary electricity, provided a sufficiently perfect insulation could be obtained.

100. To test this, a hollow glass cylinder, fig. 11, of about six inches in diameter, was prepared with a narrow riband of

Fig. 11.



a glass cylinder, *b* Leyden jar, *c* magnetizing spiral.

tin-foil, about thirty feet long, pasted spirally around the outside, and a similar riband of the same length, pasted on the inside; so that the corresponding spires of the two were directly opposite each other. The ends of the inner spiral passed out of the cylinder through a glass tube, to prevent all direct communication between the two. When the ends of the inner riband were joined by the magnetizing spiral (11.), containing a needle, and a discharge from a half-gallon jar sent through the outer riband, the needle was strongly magnetized in such a manner as to indicate *an induced current through the inner riband in the same direction as that of the current of the jar*. This experiment was repeated many times, and always with the same result.

101. When the ends of one of the ribands were placed very nearly in contact, a small spark was perceived at the opening, the moment the discharge took place through the other riband.

102. When the ends of the same riband were separated to a considerable distance, a larger spark than the last could be drawn from each end by presenting a ball, or the knuckle.

103. Also if the ends of the outer riband were united, so as to form a perfect metallic circuit, a spark could be drawn from any point of the same, when a discharge was sent through the inner riband.

104. The sparks in the two last experiments are evidently due to the action known in ordinary electricity by the name of the lateral discharge. To render this clear, it is perhaps

necessary to recall the well-known fact, that when the knob of a jar is electrified positively, and the outer coating in connexion with the earth, then the jar contains a small excess of positive electricity beyond what is necessary to perfectly neutralize the negative surface. If the knob be put in communication with the earth, the extra quantity, or the free electricity, as it is sometimes called, will be on the negative side. When the discharge took place in the above experiments, the inner riband became for an instant charged with this free electricity, and consequently threw off from the outer riband, by ordinary induction, the sparks described. It therefore became a question of importance to determine, whether the induced current described in paragraph 100 was not also a result of the lateral discharge, instead of being a true case of a secondary current analogous to those produced from galvanism. For this purpose the jar was charged, first with the outer coating in connexion with the earth, and again with the knob in connexion with the same, so that the extra quantity might be in the one case *plus* and in the other *minus*; but the direction of the induced current was not affected by these changes; it was always the same, namely, from the positive to the negative side of the jar.

105. When, however, the quantity of free electricity was increased, by connecting the knob of the jar with a globe about a foot in diameter, the intensity of magnetism appeared to be somewhat diminished, if the extra quantity was on the negative side; and this might be expected, since the free electricity, in its escape to the earth through the riband, in this case would tend to induce a feeble current in the opposite direction to that of the jar.

106. The spark from an insulated conductor may be considered as consisting almost entirely of this free or extra electricity, and it was found that this was also capable of producing an induced current, precisely the same as that from the jar. In the experiment which gave this result, one end of the outer riband of the cylinder (100) was connected with the earth, and the other caused to receive a spark from a conductor fourteen feet long, and nearly a foot in diameter. The direction of the induced current was the same as that of the spark from the conductor.

107. From these experiments it appears evident that the discharge from the Leyden jar possesses the property of inducing a secondary current precisely the same as the galvanic apparatus, and also that this induction is only so far connected with the phænomenon of the lateral discharge as this latter partakes of the nature of an ordinary electrical current.

Phil. Mag. S. 3. Vol. 16. No. 106. *Suppl.* July 1840. 2 P

108. Experiments were next made in reference to the production of currents of the different orders by ordinary electricity. For this purpose a second cylinder was prepared with ribands of tinfoil, in a similar manner to the one before described. The two were then so connected that the secondary current from the first would circulate around the second. When a discharge was passed through the outer riband of the first cylinder, a tertiary current was induced in the inner riband of the second. This was rendered manifest by the magnetizing of a needle in a spiral joining the ends of the last-mentioned riband.

109. Also by the addition, in the same way, of a third cylinder, a current of the fourth order was developed. The same result was likewise obtained by using the arrangement of the coils and helices shown in fig. 9. For these experiments, however, the coils were furnished with a double coating of silk, and the contiguous conductors separated by a large plate of glass.

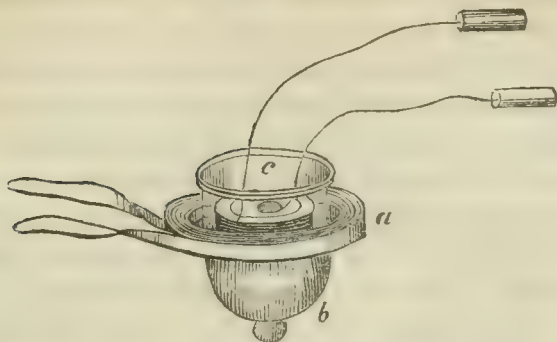
110. Screening effects precisely the same as those exhibited in the action of galvanism were produced by interposing a plate of metal between the conductors of different orders, figures 8 and 9. The precaution was taken to place the plate between two frames of glass, in order to be assured that the effect was not due to a want of perfect insulation.

111. Also analogous results were found when the experiments were made with coils interposed instead of plates, as described in paragraph 68. When the ends of the interposed coils were separated, no screening was observed; but when joined, the effect was produced. The existence of the induced current, in all these experiments, was determined by the magnetism of a needle in a spiral attached to one of the coils.

112. Likewise shocks were obtained from the secondary current by an arrangement shown in fig. 12. Helices No. 2 and No. 3 united are put within a glass jar, and coil No. 2 is placed around the same. When the handles are grasped, a shock is felt at the moment of the discharge, through the outer coil. The shocks, however, were very different in intensity with different discharges from the jar. In some cases no shock was received, when again, with a less charge, a severe one was obtained. But these irregularities find an explanation in a subsequent part of the investigation.

113. In all these experiments, the results with ordinary and galvanic electricity are similar. But at this stage of the investigation there appeared what at first was considered a remarkable difference in the action of the two. I allude to

Fig. 12.



a coil No. 2, *b* an inverted bell glass, *c* helices No. 2 and 3.

the direction of the currents of the different orders. These, in the experiments with the glass cylinders, instead of exhibiting the alternations of the galvanic currents (92), were all in the same direction as the discharge from the jar, or, in other words, they were all *plus*.

114. To discover, if possible, the cause of this difference, a series of experiments was instituted; but the first fact developed, instead of affording any new light, seemed to render the obscurity more profound. When the directions of the currents were taken in the arrangement of the coils (fig. 9) the discrepancy vanished. *Alternations were found the same as in the case of galvanism.* This result was so extraordinary that the experiments were many times repeated, first with the glass cylinders, and then with the coils; the results, however, were always the same. The cylinders gave currents all in one direction; the coils in alternate directions.

115. After various hypotheses had been formed, and in succession disproved by experiment, the idea occurred to me that the direction of the currents might depend on the distance of the conductors, and this appeared to be the only difference existing in the arrangement of the experiments with the coils and the cylinders*. In the former the distance between the ribands was nearly one inch and a half, while in the latter it was only the thickness of the glass, or about $\frac{1}{20}$ th of an inch.

116. In order to test this idea, two narrow slips of tinfoil, about twelve feet long, were stretched parallel to each other, and separated by thin plates of mica to the distance of about $\frac{1}{50}$ th of an inch. When a discharge from the half-gallon jar was passed through one of these, an induced current in the

* This idea was not immediately adopted, because I had previously experimented on the direction of the secondary current from galvanism, and found no change in reference to distance.

same direction was obtained from the other. The ribands were then separated, by plates of glass, to the distance of $\frac{1}{20}$ th of an inch; the current was still in the same direction, or *plus*. When the distance was increased to about $\frac{1}{8}$ th of an inch, no induced current could be obtained; and when they were still further separated the current again appeared, but was now *found to have a different direction, or to be minus*. No other change was observed in the direction of the current; the intensity of the induction decreased as the ribands were separated. The existence and direction of the current, in this experiment, were determined by the polarity of the needle in the spiral attached to the ends of one of the ribands.

117. The question at this time arose, whether the direction of the current, as indicated by the polarity of the needle, was the true one, since the magnetizing spiral might itself, in some cases, induce an opposite current. To satisfy myself on this point, a series of charges, of various intensity and quantity, from a single spark of the large conductor to the full charge of nine jars, were passed through the small spiral, which had been used in all the experiments, but they all gave the same polarity. The interior of this spiral is so small, that the needle is throughout in contact with the wire.

118. The fact of a change in the direction of the induced current by a change in the distance of the conductors, being thus established, a great number and variety of experiments were made to determine the other conditions on which the change depends. These were sought for in a variation of the intensity and quantity of the primary discharge, in the length and thickness of the wire, and in the form of the circuit. The results were, however, in many cases, anomalous, and are not sufficiently definite to be placed in detail before the Society. I hope to resume the investigation at another time, and will therefore at present briefly state only those general facts which appear well established.

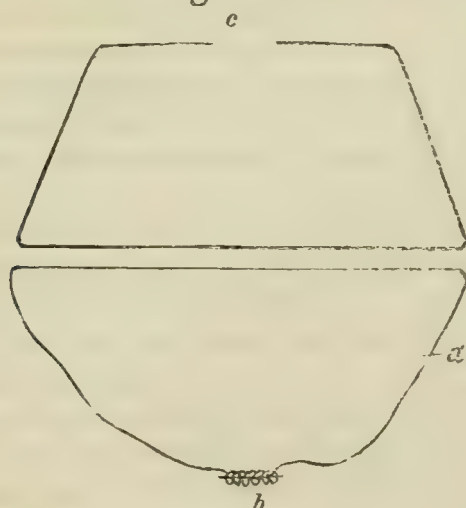
119. With a single half-gallon jar, and the conductors separated to a distance less than $\frac{1}{20}$ th of an inch, the induced current is always in the same direction as the primary. But when the conductors are gradually separated, there is always found a distance at which the current begins to change its direction. This distance depends certainly on the amount of the discharge, and probably on the intensity; and also on the length and thickness of the conductors. With a battery of eight half-gallon jars, and parallel wires of about ten feet long, the change in the direction did not take place at a less distance than from twelve to fifteen inches, and with a still

larger battery and longer conductors, no change was found, although the induction was produced at the distance of several feet.

120. The facts given in the last paragraph relate to the inductive action of the primary current; but it appears from the results detailed in paragraphs 110 and 114, that the currents of all the other orders also change the direction of the inductive influence with a change of the distance. In these cases, however, the change always takes place at a very small distance from the conducting wire; and in this respect the result is similar to the effect of a *primary current* from the discharge of a small jar.

121. The most important experiments, in reference to distance, were made in the lecture-room of my respected friend Dr. Hare of Philadelphia, with the splendid electrical apparatus described in the fifth volume (new series) of the Transactions of this Society. The battery consists of thirty-two jars, each of the capacity of a gallon. A thick copper wire of about $\frac{1}{10}$ th of an inch in diameter and eighty feet in length, was stretched across the lecture-room, and its ends brought to the battery, so as to form a trapezium, the longer side of which was about thirty-five feet. Along this side a wire was stretched of the ordinary bell size, and the extreme ends of this joined by a spiral, similar to the arrangement shown in fig. 13. The two wires were at first placed within the di-

Fig. 13.



c place of the battery, *b* spiral.

stance of about an inch, and afterwards constantly separated after each discharge of the whole battery through the thick wire. When a break was made in the second wire at *a*, no magnetism was developed in a needle in the spiral at *b*; but when the circuit was complete, the needle at each discharge

indicated a current in the same direction as that of the battery. When the distance of the two wires was increased to sixteen inches, and the ends of the second wire placed in two glasses of mercury and a finger of each hand plunged into the metal, a shock was received. The direction of the current was still the same, but the magnetism not as strong as at a less distance.

122. The second wire was next arranged around the other, so as to inclose it. The magnetism by this arrangement appeared stronger than with the last ; the direction of the current was still the same, and continued thus, until the two wires were at every point separated to the distance of twelve feet, except in one place where they were obliged to be crossed at the distance of seven feet, but here the wires were made to form a right angle with each other, and the effect of the approximation was therefore (46) considered as nothing. The needle at this surprising distance was tolerably strongly magnetized, as was shown by the quantity of filings which would adhere to it. The direction of the current was still the same as that of the battery. The form of the room did not permit the two wires to be separated to a greater distance. The whole length of the circuit of the interior large wire was about eighty feet ; that of the exterior one hundred and twenty. The two were not in the same plane, and a part of the outer passed through a small adjoining room.

123. The results exhibited in this experiment are such as could scarcely have been anticipated by our previous knowledge of the electrical discharge. They evince a remarkable inductive energy, which has not before been distinctly recognised, but which must perform an important part in the discharge of electricity from the clouds. Some effects which have been observed during thunder storms, appear to be due to an action of this kind.

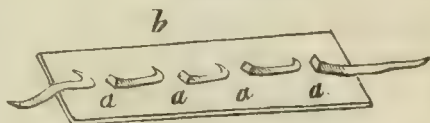
124. Since a discharge of ordinary electricity produces a secondary current in an adjoining wire, it should also produce an analogous effect in its own wire ; and to this cause may be now referred the peculiar action of a long conductor. It is well known that the spark from a very long wire, although quite short, is remarkably pungent. I was so fortunate as to witness a very interesting exhibition of this action during some experiments on atmospheric electricity made by a committee of the Franklin Institute in 1836. Two kites were attached, one above the other, and raised with a small iron wire in place of a string. On the occasion at which I was present, the wire was extended by the kites to the length of about one mile. The day was perfectly clear, yet the sparks from the wire had so much projectile force (to use a con-

venient expression of Dr. Hare) that fifteen persons joining hands and standing on the ground, received the shock at once, when the first person of the series touched the wire. A Leyden jar being grasped in the hand by the outer coating, and the knob presented to the wire, a severe shock was received, as if by a perforation of the glass, but which was found to be the result of the sudden and intense induction.

125. These effects were evidently not due to the accumulated intensity at the extremities of the wire, on the principles of ordinary electrical distribution, since the knuckle required to be brought within about a quarter of an inch before the spark could be received. It was not alone the quantity, since the experiments of Wilson prove that the same effect is not produced with an equal amount of electricity on the surface of a large conductor. It appears evidently therefore a case of the induction of an electrical current on itself. The wire is charged with a considerable quantity of feeble electricity, which passes off in the form of a current along its whole length, and thus the induction takes place at the end of the discharge, as in the case of a long wire transmitting a current of galvanism.

126. It is well known that the discharge from an electrical battery possesses great divellent powers; that it entirely separates, in many instances, the particles of the body through which it passes. This force acts, in part, at least, in the direction of the line of the discharge, and appears to be analogous to the repulsive action discovered by Ampère, in the consecutive parts of the same galvanic current. To illustrate this, paste on a piece of glass a narrow slip of tinfoil, cut it through at several points, and loosen the ends from the glass at the places so cut. Pass a discharge through the tinfoil from about nine half-gallon jars; the ends, at each separation, will be thrown up, and sometimes bent entirely back, as if by the action of a strong repulsive force, between them. This will be understood by a reference to fig. 14; the ends are shown bent back at *a, a, a, a*. In the popular experiment of the pierced card, the bur on each side appears to be due to an action of the same kind.

Fig. 14.



b glass plate; *a, a, a, a*, openings in tinfoil.

127. It now appears probable, from the facts given in paragraphs 119 and 120, that the table in paragraph 92 is only an approximation to the truth, and that each current from

galvanism, as well as from electricity, first produces an inductive action in the direction of itself, and that the inverse influence takes place at a little distance from the wire.

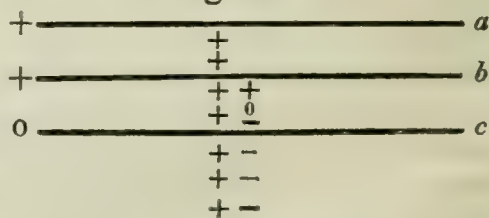
128. To test this, the compound helix was placed on coil No. 1, to receive the induction, and its ends joined to those of the outer riband of tinfoil of the glass cylinder, while the magnetizing spiral was attached to the ends of the inner riband. A feeble tertiary current was produced by this arrangement, which in two cases gave a polarity to the needle indicating a direction the same as that of the primary current. In other cases the magnetism was either imperceptible or *minus*. With an arrangement of two coils of wires around two glass cylinders, one within the other, the same effect was produced. The magnetism was less when the distance of the two sets of spires was smaller, indicating, as it would appear, an approximation to a position of neutrality. These results are rather of a negative kind, yet they appear to indicate the same change with distance in the case of the galvanic currents, as in that of the discharge of ordinary electricity. The distance however at which the change takes place would seem to be less in the former than in the latter.

129. There is a perfect analogy between the inductive action of the primary current from the galvanic apparatus and of that from the larger electrical battery. The point of change, in each, appears to be at a great distance.

130. The neutralizing effect described in Section IV. may now be more definitely explained by saying, that when a third conductor is acted on at the same time by a primary and secondary current (unless it be very near the second wire) it will fall into the region of the *plus* influence of the former, and into that of the *minus* influence of the latter; and hence no induction will be produced.

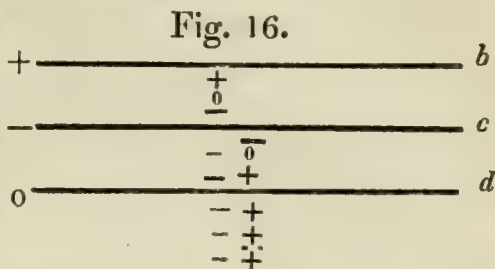
131. This will be rendered perfectly clear by fig. 15, in which *a* represents the conductor of the primary current, *b* that of the secondary, and *c* the third conductor. The characters $+++$, &c., beginning at the middle of the first conductor and extending downwards, represent the constant *plus* influence of the primary current, and those $+0--$, &c., beginning at the second conductor, indicate its inductive influence as changing with the distance. The third conductor, as is shown by the

Fig. 15.



figure, falls in the *plus* region of the primary current, and in the *minus* region of the secondary, and hence the two actions neutralize each other, and no apparent result is produced.

132. Fig. 16 indicates the method in which the neutralizing effect is produced in the case of the secondary and tertiary currents. The wire conducting the secondary current is represented by *b*, that conducting the tertiary by *c*, and the other wire, to receive the induction from these, by *d*. The direction



of the influence, as before, is indicated by + 0 — —, &c., and the third wire is again seen to be in the *plus* region of the one current, and in the *minus* of the other. If, however, *d* is placed sufficiently near *c*, then neutralization will not take place, but the two currents will conspire to produce in it an induction in the same direction. A similar effect would also be produced, were the wire *c*, in fig. 15, placed sufficiently near the conductor *b*.

133. Currents of the several orders were likewise produced from the excitation of the magneto-electrical machine. The same neutralizing effects were observed between these as in the case of the currents from the galvanic battery, and hence we may infer that also the same alternations take place in the direction of the several currents.

134. In conclusion, I may perhaps be allowed to state, that the facts here presented have been deduced from a laborious series of experiments, and are considered as forming some addition to our knowledge of electricity, independently of any theoretical considerations. They appear to be intimately connected with various phænomena, which have been known for some years, but which have not been referred to any general law of action. Of this class are the discoveries of Savary, on the alternate magnetism of steel needles, placed at different distances from the line of a discharge of ordinary electricity*, and also the magnetic, screening influence of all metals, discovered by Dr. Snow Harris of Plymouth †. A comparative study of the phænomena observed by these distinguished savants, and those given in this paper, would probably lead to some new and important developments. Indeed every part of the subject of electro-dynamic induction

* *Annales de Chimie et de Physique*, 1827.

† Philosophical Transactions, 1831, [noticed in Phil. Mag. and Annals, N. S. Vol. x. p. 297-298.]

appears to open a field for discovery, which experimental industry cannot fail to cultivate with immediate success.

Note.—On the evening of the meeting at which my investigations were presented to the Society, my friend, Dr. Bache of the Girard College, gave an account of the investigations of Professor Ettingshausen of Vienna, in reference to the improvement of the magneto-electric machine, some of the results of which he had witnessed at the University of Vienna about a year since. No published account of these experiments has yet reached this country, but it appears that Professor Ettingshausen had been led to suspect the development of a current in the metal of the keeper of the magneto-electric machine, which diminished the effect of the current in the coil about the keeper, and hence to separate the coil from the keeper by a ring of wood of some thickness, and afterwards, to prevent entirely the circulation of currents in the keeper, by dividing it into segments, and separating them by a non-conducting material. I am not aware of the result of this last device, nor whether the mechanical difficulties in its execution were fully overcome. It gives me pleasure to learn that the improvements, which I have merely suggested as deductions from the principles of the interference of induced currents (76), should be in accordance with the experimental conclusions of the above-named philosopher.

LXXXV.—*On the Heat of Vapours and on Astronomical Refractions.* By JOHN WILLIAM LUBBOCK, Esq., Treas. R.S. F.R.A.S. and F.L.S., Vice-Chancellor of the University of London, &c.

[Continued from p. 514.]

ON THE STEAM-ENGINE.

THE law which connects the pressure and the temperature of steam having been unknown, various empirical rules have been given. As, however, the expressions which arise are not in a convenient form for the calculations which are required in order to ascertain the *duty* which steam-engines are capable of performing, or to solve other problems of the same nature, M. de Pambour*, in his work on that subject, has employed another expression, viz.

$$\mu = \frac{1}{g} = \frac{1}{n + q p},$$

in which g is the density of steam, p the pressure, and n and q constants. According to my expression

* *Théorie de la Machine à Vapeur*, p. 111.

$$\frac{1}{e} = \frac{(1 - E)}{\frac{1}{p^\gamma - E p}}.$$

The pressure being reckoned in atmospheres, and the density of steam corresponding to the pressure of one atmosphere (or 14.706 lbs. per square inch) being unity. If we take the density of water for unity, then as the volume of steam at the pressure of one atmosphere is 1700 times greater than that of the same weight of water,

$$\mu = - \frac{K}{\frac{1}{p^\gamma - E p}} \quad K = 1700 (E - 1)$$

$$\log K = 2.4765041 \quad \frac{1}{\gamma} = 1.0134 \quad E = 1.17602.$$

If we suppose that a certain volume of water represented by S be transformed into vapour at the pressure p , and that M is the absolute volume of vapour which results, we shall have

$$\frac{M}{S} = \mu = \frac{[0.4109002]}{p} \left(\frac{1}{\alpha} + \theta \right).$$

If afterwards the same volume of water is transformed into vapour at the pressure p' , and that the absolute volume which the resulting vapour occupies be called M' , we shall have

$$\frac{M'}{S} = \mu'$$

$$\frac{M}{M'} = \frac{p'^{\frac{1}{\gamma}} - E p}{\frac{1}{p^\gamma - E p}}$$

“ Soit* P la pression *totale* de la vapeur dans la chaudière, et p' la pression qu’aura cette vapeur à son arrivée dans le cylindre, pression qui sera toujours moindre que P , excepté dans un cas particulier que nous traiterons plus loin. La vapeur pénétrera donc dans le cylindre à la pression p' , et elle continuera d’affluer avec cette pression et de produire un effet correspondant, jusqu’à ce que la communication entre la chaudière et le cylindre soit interceptée. Alors il cessera d’arriver de la vapeur nouvelle dans le cylindre, mais celle qui y est déjà parvenue, commencera à se dilater pendant le reste de la course du piston, en produisant par sa détente une certaine quantité de travail, qui s’ajoutera à celle déjà produite pendant la période d’admission de la vapeur.

* The reasoning here is taken from M. de Pambour’s work.

“ P étant la pression de la vapeur dans la chaudière, et p' la pression qu'elle prendra à son arrivée dans le cylindre avant la détente, soit π la pression de cette vapeur en un point quelconque de la détente. Soit en même temps l la longueur totale de la course du piston, l' la portion parcourue au moment où a commencé la détente, et λ celle qui correspond au point où la vapeur a acquis la pression π . Enfin, soit encore a l'aire du piston, et c la liberté du cylindre, c'est-à-dire l'espace libre qui existe à chaque bout du cylindre, au-delà de la portion parcourue par le piston, et qui se remplit nécessairement de vapeur à chaque course; cet espace, y compris les passages aboutissants, étant représenté par une longueur équivalente du cylindre.

“Si l'on prend le piston au moment où la longueur de course parcourue est λ , et la pression π , on verra que si le piston parcourt, en outre, un espace élémentaire $d\lambda$, le travail élémentaire produit dans ce mouvement sera $\pi a d\lambda$. Mais en même temps, le volume $a(l' + c)$ occupé par le vapeur avant la détente sera devenu $a(\lambda + c)$.” Hence,

$$\frac{M}{M'} = \frac{p'^{\frac{1}{\gamma}} - E p'}{\pi^{\frac{1}{\gamma}} - E \pi} = \frac{\lambda + c}{l' + c}.$$

$$\lambda + c = (l' + c) \frac{(p'^{\frac{1}{\gamma}} - E p')}{(\pi^{\frac{1}{\gamma}} - E \pi)}$$

The elementary work produced $= \pi a d\lambda$.

$$\begin{aligned} \int \pi a d\lambda &= \pi a (\lambda + c) - \int a (\lambda + c) d\pi \\ &= \pi a (\lambda + c) - \int a (l' + c) (p'^{\frac{1}{\gamma}} - E p') \frac{d\pi}{(\pi^{\frac{1}{\gamma}} - E \pi)} \\ &= \pi a (\lambda + c) - a (l' + c) (p'^{\frac{1}{\gamma}} - E p') \int \frac{d\pi}{\pi^{\frac{1}{\gamma}} - E \pi} \\ &= \pi a (\lambda + c) \\ &\quad + a (l' + c) \frac{(p'^{\frac{1}{\gamma}} - E p')^{\gamma}}{E (\gamma - 1)} \log (1 - E \pi^{\frac{\gamma-1}{\gamma}}) + \text{const.} \end{aligned}$$

This integral is to be taken from $\lambda = l'$ to $\lambda = l$, let $\pi = p$ when $\lambda = l$, when $\lambda = l'$, $\pi = p'$.

$$\int \pi a \, d\lambda = p a (l + c) - p' a (l' + c) \\ + a (l' + c) \frac{(p'^{\frac{1}{\gamma}} - E p') \gamma}{E(\gamma - 1)} \log \left\{ \frac{1 - E p^{\frac{\gamma-1}{\gamma}}}{1 - E p'^{\frac{\gamma-1}{\gamma}}} \right\}$$

for the values of the constants E, γ . See p. 511.

To this must be added the work effected during the course of the piston through l' , which is $p' a l$, and if R is the total pressure exerted upon unity of surface of the piston

$$p a (l + c) - p' a c \\ + a (l' + c) (p'^{\frac{1}{\gamma}} - E p') \frac{\gamma}{E(\gamma - 1)} \log \left\{ \frac{1 - E p^{\frac{\gamma-1}{\gamma}}}{1 - E p'^{\frac{\gamma-1}{\gamma}}} \right\} \\ = a R l \quad (A)^*$$

$R = (1 + \delta) r + p'' + f$, f is the friction of the machine not loaded, δ the increase of this friction due to unity of the charge r , p'' the pressure on the surface of the piston, representing the atmospheric pressure when the machine works without condensation, and otherwise the pressure of condensation in the cylinder.

If S denote the volume of water converted into vapour by the boiler in unity of time, this volume in the cylinder becomes

$$- \frac{S K}{p'^{\frac{1}{\gamma}} - E p'}$$

K being the same constant as in p. 563.

It is evident, according to the reasoning of M. de Pambour, in p. 125. of his work, that if v denote the velocity of the piston

$$\frac{S K}{p'^{\frac{1}{\gamma}} - E p'} = - v a \frac{l' + c}{l} \quad (B.)$$

* This equation is equivalent to the equation (A) of M. de Pambour, p. 123, which may be put into the form

$$p a (l + c) - p' a c + \frac{l S}{q v} \text{ Nap. log } \left\{ \frac{l + c}{l' + c} \right\} = a R l.$$

$$\text{or} \quad p a (l + c) - p' a c + [6.9505960] \frac{l S}{v} \log \left\{ \frac{l + c}{l' + c} \right\} = a R l,$$

the pressure being reckoned in lbs. per square inch.

$$p'^{\frac{1}{\gamma}} - E p' = - \frac{l S K}{a v (l' + c)} = \frac{K}{\mu'},$$

Similarly,

$$p^{\frac{1}{\gamma}} - E p = - \frac{l S K}{a v (l + c)} = \frac{K}{\mu}$$

$$\frac{1}{\mu} = \frac{l S}{a v (l + c)} \qquad \frac{1}{\mu'} = \frac{l S}{a v (l' + c)}$$

$$p a (l + c) - p' a c - \frac{l S K \gamma}{v E (1 - \gamma)} \text{ Nap. log } \left\{ \frac{1 - E p'^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} \right\} = a R l$$

$$p a (l + c) - p' a c - (p'' + f) a l - [4.6411966] \frac{l S}{v} \log^* \left\{ \frac{1 - E p'^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} \right\} = a l (1 + \delta) r$$

$$\log \left\{ \frac{1 - E p'^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} \right\} = \log \left\{ \frac{l + c}{l' + c} \right\} - \frac{1}{\gamma} \log \left(\frac{p'}{p} \right).$$

If the machine work without expansion,

$$p = p', \qquad \log \left\{ \frac{1 - E p'^{\frac{\gamma-1}{\gamma}}}{1 - E p^{\frac{\gamma-1}{\gamma}}} \right\} = 0,$$

$$r = \frac{p - (p'' + f)}{1 + \delta}$$

The data upon questions relating to the steam-engine are the quantities a , l , l' , S , and v , and it is evident that from these quantities the quantities μ and μ' may at once be found by an easy arithmetical operation; from these the following table will give the corresponding pressures p and p' , and these pressures being introduced into equation A, the value of $a r$ may be easily found.

* Log. of Briggs, the pressure being reckoned in atmospheres, the log. of the constant is [7.9670537], the pressure being reckoned in lbs. per square foot.

Table showing the volume (compared with that of water at 212°),
and the temperature of steam.

Pressure in lbs. per square inch. $14.706 \times p.$	Temperature. Fahrenheit. τ		Volume. μ^\dagger	Pressure in lbs. per square inch. $14.706 \times p.$	Temperature. Fahrenheit. τ		Volume. μ
	Air*. Therm.	Merc. Therm.			Air. Therm.	Merc. Therm.	
1	101.5	101.5	20816	56	287.6	289.5	498
2	126.0	126.0	10871	57	288.7	290.7	490
3	141.4	141.4	7442	58	289.8	291.8	482
4	153.0	153.0	5691	59	290.9	293.0	474
5	162.2	162.2	4622	60	292.0	294.1	467
6	170.1	170.1	3902	61	293.0	295.1	460
7	176.8	176.8	3381	62	294.0	296.2	453
8	182.8	182.8	2986	63	295.0	297.2	447
9	188.3	188.3	2678	64	296.0	298.2	440
10	193.2	193.2	2429	65	297.0	299.2	434
11	197.8	197.8	2223	66	298.0	300.3	428
12	202.0	202.0	2051	67	299.0	301.3	422
13	205.9	205.9	1905	68	300.0	302.3	417
14	209.5	209.5	1778	69	301.0	303.3	411
14.706	212.0	212.0	1700	70	301.9	304.2	406
15	212.9	212.9	1669	71	302.8	305.2	401
16	216.3	216.4	1572	72	303.7	306.1	396
17	219.3	219.5	1487	73	304.6	307.0	391
18	222.3	222.6	1410	74	305.5	308.0	386
19	225.1	225.5	1342	75	306.4	308.9	381
20	227.9	228.3	1280	76	307.3	309.8	377
21	230.4	230.9	1224	77	308.2	310.7	372
22	232.9	233.5	1172	78	309.1	311.6	368
23	235.2	235.8	1125	79	310.0	312.6	364
24	237.6	238.3	1082	80	310.9	313.5	359
25	239.8	240.6	1042	81	311.8	314.5	355
26	242.0	242.8	1005	82	312.7	315.4	351
27	244.0	244.9	971	83	313.5	316.3	348
28	246.1	247.0	939	84	314.3	317.1	344
29	248.0	249.0	909	85	315.1	317.9	340
30	250.0	251.0	881	86	315.9	318.7	337
31	251.8	252.8	855	87	316.7	319.6	333
32	253.7	254.8	831	88	317.5	320.4	330
33	255.4	256.5	808	89	318.3	321.2	326
34	257.2	258.3	786	90	319.1	322.0	323
35	258.9	260.1	765	91	319.9	322.9	320
36	260.6	261.8	746	92	320.7	323.7	317
37	262.2	263.5	727	93	321.5	324.5	313
38	263.8	265.1	709	94	322.3	325.3	310
39	265.3	266.6	693	95	323.0	326.0	307
40	266.8	268.2	677	96	323.7	326.8	305
41	268.2	269.6	662	97	324.4	327.5	302
42	269.7	271.2	647	98	325.1	328.3	299
43	271.1	272.6	633	99	325.8	329.0	296
44	272.5	274.1	620	100	326.5	329.7	293
45	273.8	275.4	608	105	330.0	333.3	281
46	275.2	276.8	596	120	339.7	343.4	249
47	276.5	278.2	584	135	348.4	352.4	223
48	277.8	279.5	573	150	356.5	360.8	203
49	279.1	280.8	562	165	363.9	368.4	186
50	280.4	282.1	552	180	370.7	375.5	172
51	281.7	283.5	542	195	377.0	382.0	160
52	282.9	284.7	532	210	383.3	388.4	150
53	284.1	286.0	523	225	389.0	394.4	141
54	285.2	287.1	514	240	394.5	400.1	133
55	286.4	288.3	506				

$$* \tau = - \frac{[2.0651059]}{p^{.0134} - 1.17602} - 448^{\circ} \quad \dagger \mu = \frac{[0.4109002] \left(\frac{1}{\alpha} + \theta \right)}{.}$$

The following example will serve to show in what manner the table was calculated.

Ex.—Calculation of the temperature and volume of steam for the pressure of 180 lbs. per square inch.

$$\log 180 = 2.2552725$$

$$\log 14.706 = 1.1674946$$

$$\log p = 1.0877779 \times .0134 = .01457622386 = \log 1.03413$$

$$1.17602$$

$$1.03413$$

$$2.0651059$$

$$\log .14189 = 9.1519518$$

$$2.9131541 = \log 818.7$$

$$448.0$$

$$\text{Temp. Fahr. Air Therm.} = 370.7$$

$$4.8$$

$$\text{Temp. Fahr. Merc. Therm.} = 375.5$$

$$0.4109002$$

$$\log \frac{1}{p} = 8.9122221$$

$$2.9131541$$

$$2.2362764 = \log 172 \quad \mu = 172$$

The following data are taken from M. de Pambour's work on the Steam Engine, p. 238.

$$l' = .25 \, l \quad v = 250 \quad f = .5 \text{ (lb. per square inch)} \quad \delta = .14$$

$$l = 10 \text{ ft.} \quad a = 12.566 \text{ sq. ft.} \quad S = .927 \text{ cub. ft.}$$

$$p'' = 4 \text{ lbs. per square inch.} \quad c = .05 \, l$$

$$\log v = 2.3979400$$

$$\log v = 2.3979400$$

$$\log a = 1.0991971$$

$$\log a = 1.0991971$$

$$\log .30 = 9.4771213$$

$$\log 1.05 = 0.0211893$$

$$2.9742584$$

$$3.5183264$$

$$\log S = 9.9670797$$

$$\log S = 9.9670797$$

$$3.0071787 = \log 1016.6$$

$$3.5512467 = \log 3558.3$$

$$\mu' = 1016.6$$

$$\mu = 3558.3$$

Hence by the table $p' = 25.686$, $p = 6.627$ reckoned in lbs. per square inch.

$$p' = 1.7466, \quad p = .4506, \quad p'' + f = .3060 \text{ in atmospheres.}$$

$$\log 1.7466 = .2422019, \quad \log .4506 = 9.6538224$$

$$.2422019 \times .0134 = .00324550546$$

$$9.9967545$$

$$\log E = 0.0704184$$

$$\hline 0.0671729 = \log 1.16727$$

$$0.3461776 \times .0134 = .00463877984$$

$$.0704184$$

$$\hline .0750571 = \log 1.18865$$

$$\log .18865 = 9.2756568$$

$$\log .16727 = 9.2234181$$

$$\hline 0.0522387$$

$$\log p = 9.6538224$$

$$\log a = 1.0991971$$

$$\log (l + c) = 1.0211893$$

$$\hline 1.7742088$$

$$59.457$$

$$\log p' = 0.2422019$$

$$\log a = 1.0991971$$

$$\log c = 9.6989700$$

$$\hline 1.0403690$$

$$10.974$$

$$38.451$$

$$\hline 49.425$$

$$\hline$$

$$4.6411966$$

$$\log l = 1.0000000$$

$$\log S = 9.9671554$$

$$8.7179923$$

$$\hline 4.3263443$$

$$\log v = 2.3979400$$

$$\hline 1.9284043$$

$$84.802$$

$$59.457$$

$$\hline 144.259$$

$$49.425$$

$$\hline 94.834$$

$$\log 94.834 = 1.9769641$$

$$\log 14.706 = 1.1674946$$

$$\log 144 = 2.1583625$$

$$\hline 5.3028212$$

$$\log l (1 + \delta) = 1.0569049$$

$$\hline 4.2459163$$

$$ar = 17616$$

$ar = 17616$ expressed in lbs., M. de Pambour finds $ar = 17337$.

[To be continued.]

LXXXVI. *On the Combinations of Carbon with Silicon and Iron, and other Metals, forming the different Species of Cast Iron, Steel, and Malleable Iron.* By Dr. C. SCHAFHAEUTL, of Munich.

[Continued from p. 523, and concluded.]

IN scarcely any analytical proceedings has the presence of electro-negative metals been more overlooked, than in the analyses of cast iron, steel, and wrought iron: the best irons of Sweden contain, as we shall soon show, a considerable quantity of arsenic, and the celebrated English *Low-Moor* iron contains still more. By forging the best English cast steel, arsenic is volatilized and may be very easily detected by the smell; and the blacksmiths who forge *Low-Moor* iron, frequently complain of the unpleasant smell which escapes (by them termed a sulphureous smell), causing them often swelled lips. It is for this reason that the *Low-Moor* iron surpasses in hardness and tenacity all other English iron. The same iron is known for its capability of being converted into bar-steel for *coach-springs*, although not bearing a higher degree of conversion.

It is known that Wootz, or Indian steel, as well as cast steel made from Dannemora iron, is particularly adapted for cutting instruments which require an extremely sharp fine edge; but for purposes in which great tenacity is required, without a particularly fine grain, where the steel is required to be welded at an elevated degree of heat and in large masses, the celebrated Russian CCND iron is far preferable, containing besides a large quantity of silicon and manganese, also a large quantity of phosphorus.

To the presence of sulphur as well as arsenic is generally ascribed the property of the iron being red-short, and the sulphur particularly has in this respect a bad reputation. Karsten declares that even the presence of 0.03375 part of sulphur is capable of making the iron totally unfit for use in a red heat, because he caused sulphate of lime or gypsum to be melted down with iron ore in a blast furnace, and found the iron prepared from it perfectly red-short, containing only the small quantity of sulphur just mentioned. But he did not investigate the other contents of the red-short cast iron, which in such case contains always calcium or sulphuret of calcium in its composition as well as sulphuret of silicon. If such a small quantity of sulphur as Karsten mentions, would make iron red-short, no malleable iron at all could be produced by means of pit-coal, as even the softest and best English iron invariably

contains more sulphur than the quantity mentioned by him. Charcoal itself imparts a portion of sulphur to the iron.

As a further proof of what has just been asserted, we now refer to some specimens of *French cast iron*, the produce of the furnaces near *Alais*, département du Gard, at the foot of the Cevennes.

These specimens were produced from hydrated oxides of iron, which cover the summits of several hills of limestone and carboniferous sandstone extending over a long district, and have undoubtedly been deposited in this state by water. The greatest part of this ore has a perfectly ochry appearance, intermixed with masses of red oxide of iron, which is so similar to oxide of iron, precipitated from its solution in acids by caustic ammonia, and dried on a filter, that it is impossible to distinguish between the two specimens when placed together, which is the artificial and which the natural. As it is the mode in France to assay the iron ores only in the *dry way* in a small crucible, lined with charcoal, nothing more is obtained than the quantity of metallic iron which is contained in the ore; and as those ores were found to be very rich and in great abundance, a large iron-work has in consequence been erected; but actual trial of the ore in the blast furnaces very soon convinced the proprietors, that in the selection of ore for *practical* purposes more research is required than the mere melting down of three grammes of iron ore in the crucible in the laboratory.

The iron obtained from this ore had invariably the bad quality of emitting a great quantity of fumes during heating, and not welding except in a state of half-fusion. But when this welding was accomplished, the iron naturally had lost its quality, and was found to be burnt. This ore is invariably mixed with a subarseniate of iron, which contains in 100 parts 7 of arsenic acid and 13.68 of water, and is mechanically mixed with galena, bournonite, and similar minerals. The small quantity of arsenic in this case is easily overlooked even with the blow-pipe, and generally no other means will detect the presence of arsenic except a current of sulphuretted hydrogen.

The manner of conducting the blast furnaces with pit coal in France is, notwithstanding the great difference in the ores, exactly the same as in England; and English workmen are down to the present time generally employed, not only in working at the puddling furnaces, but also at the blast furnaces.

It will be apparent, that the above-mentioned ore is extremely fusible, and requires therefore a very careful arrangement of the fluxes, in order to obtain a tolerably good quality of cast iron, particularly as in this hot southern climate the rarefied and dry air has a peculiar influence on the produc-

tion of the blast furnaces under some circumstances, which I shall further explain; so that I witnessed for months a different working of the blast furnaces, even at different periods of the day.

Notwithstanding the profusion of rich ores throughout France, it is infinitely more difficult to produce iron of good quality from them, than from the clay ironstone in England; and excepting where iron is melted down with charcoal, the iron produced is of very inferior quality compared with that of England.

As I think it will be very instructive to examine the chemical properties of several specimens of such cast iron, obtained from the same ore and in the same furnace, I shall briefly describe five specimens of iron from the furnace of Alais.

I call the first (*a*); it has a dead gray appearance, but is intersected by somewhat whitish shining rays having a distant resemblance to the lamellar crystallization of white crystallized charcoal iron. It was rather hard and brittle, and its specific gravity was 7.442. The second specimen (*b*) was obtained under peculiar circumstances. During one cast, in particular, the iron ran from the hearth into the moulds in the sand, and the rapid contraction of the exterior of those pigs forced out the still liquid interior through the face of the pigs like a fountain. The iron thus forced out is the specimen (*b*); it had a silvery white appearance, broke with large crystalline planes, approaching somewhat to a cubical fracture, and had a specific gravity of 7.33.

The specimen (*c*) was also perfectly silver-white, consisting of an extremely large pearl-like granulation, easily to be broken, and its specific gravity was 7.582.

Specimen (*d*) was extremely difficult of fusion, scarcely to be melted down in the fineries, and not at all available in the puddling furnaces; its specific gravity was 7.61.

Specimen (*e*) is a malleable iron produced from gray cast iron, obtained only from a few casts during the beginning of the working of the blast furnace. Its qualities we shall afterwards describe.

On treating the specimen (*a*) with hydrochloric acid in the way before described, I observed, that during the last washing of the sulphuret of lead with boiling-hot distilled water, acidulated with hydrochloric acid, as soon as this water dropt beneath into the solution of nitrate of lead, the surface of the liquid assumed a beautiful bright vermilion-red colour during the formation of chloride of lead. As soon as a considerable quantity of the coloured fluid was collected, I decanted it into another glass and found next day that the colouring

matter was deposited, traversed by beautiful red needle-like crystals; these crystals washed with water during solution lost their colour, were not soluble in alcohol, and heated in a glass tube emitted a very pungent smell somewhat resembling cyanogen.

The liquid solution of nitrate of lead smelt very strongly of hydrogen developed from cast iron, and I am convinced, that this compound of hydrogen, carbon, azote, and sulphur forms a salt with the oxide of lead, which is more distinct, when the acid, in which the cast iron is dissolved, is so weak, that little or no sulphuretted hydrogen is disengaged. When acetate of lead is used, and the lead carefully precipitated by sulphuric acid, this compound is separated from the lead and will be found to swim on the surface of the liquid. It is unfortunately too little to be collected, and the only way to glean some idea as to its composition is, to detonate the hydrogen with oxygen*.

Another remarkable circumstance is, that in determining the quantity of arsenic contained in the iron by boiling it in aqua-regia, dropping the neutralized filtered liquid into hydro-sulphuret of ammonia, and decomposing it carefully with acid, the precipitated sulphur, during its solution in aqua-regia, deposited invariably a black scaly heavy substance, darker than graphite. From 12·89 grains of this sulphur, I separated 0·36 grains of these black scales. By heating them in a glass tube, sulphur was disengaged, and black dull scales remained equal to 0·1438 grains. These black scales were not soluble in any acid, and appeared through the microscope mixed with white transparent grains. A part of these scales heated on a platinum foil began to sparkle, glowed in a very lively manner, which after some time ceased, and a white powder remained, mixed with those transparent grains. The powder before the blowpipe appeared like silica, and the scales consisted therefore probably of sulphuret of silicon, or a mixture of sulphuret of silicon with metallic silicon; or it might perhaps be considered as a compound of sulphur, carbon, and silicon. If we consider it as a simple sulphuret of silicon, we

6

might obtain the not very probable formula Si S .

* A series of experiments, which I have made recently, and since the writing of the above paper (which only contains the experiments made while travelling through England and France), confirm entirely this opinion. I shall elucidate this subject further in a paper on the gases developed by treating iron with acids, and describe at the same time a new portable apparatus for analysing the compound radicals (of organic chemistry), by means of which the oxygen, hydrogen, carbon, and azote of the compound are easily determined in one operation only.

The remainder of the solution of this iron in hydrochloric acid, heated in the crucible, *showed no disposition to glow* like the specimens before-mentioned; it lay still dark on the bottom, after the crucible had become for a considerable time red-hot, and after four ignitions its appearance was but very slightly altered. It increased after the first ignition = 0·064

„	second	„	= 0·209
„	third	„	= 0·096
„	fourth	„	= 0·016

0·385

35 grains of the specimen (a) boiled in aqua-regia for five minutes, left a residuum only = 3·7625. It was this solution in which the before-mentioned black scales were generated.

The same quantity of iron treated in a retort with diluted nitric acid, left only 2·30 grains of residuum, without the development of any carbonic acid, which shows that during the solution a new compound is formed, of which azote and hydrogen form a prominent part.

By treating the same quantity of iron with still more dilute nitric acid, carbonic acid gas was evolved, and the residuum was found to be 3·144 grains.

From this residuum, boiled in a platinum dish with nitric acid, sulphur was very soon separated, which was removed after renewing the acid; and boiling it again till decomposition was entirely completed, perfectly white silica was separated, which in the course of drying on the filter, became interspersed with beautiful blue spots, having a resemblance to phosphate of iron.

The 2·3 grains residuum of the iron, treated in the second experiment in the retort, was, after the action of the acid had ceased, poured out with the acid into a china dish, and remained untouched for six weeks.

After the lapse of this time, a brown sediment was as usual found on the bottom, and in the middle of it a white mass, composed of gelatinous granulations, interspersed with brick-red or vermilion points, like vanadic acid. A great part of this gelatinous residuum retained the perfect form of the fragments of cast iron, the *skeleton* of which they formed.

These gelatinous fragments, when divided, showed the interior to consist of gelatinous layers, which from the second layer down to the centre were of a beautiful blue colour. They imparted to diluted hydrochloric acid when poured over them a green colour, which was destroyed by ammonia, and again revived by acids. Reagents as well as the blowpipe discovered nothing in the solution except protoxide of iron, carbon and

azote, and the blue tints appeared to me therefore to form a cyanuret of iron (?).

The gelatinous mass, viewed through the microscope, consisted of an aggregation of gelatinous grains of silica, interspersed with grains at least ten times smaller of a vermilion colour. The blue layers had the same composition, with the exception, that the silica was penetrated by the before-mentioned blue colour.

From these experiments we can perceive very plainly the mechanical structure of the white cast iron, in which the close connexion of the silicon with carbon and azote is beautifully developed.

The disappearance of the blue colour from the layers on the outside during the contact of the acid and the air, shows the progressive formation and decomposition—as well as the interspersed brick-red grains resembling vanadic acid, and explains the mechanical arrangement of the different constituents of cast iron.

The specimen (*b*) shows other remarkable qualities. Treated with concentrated hydrochloric or nitric acid, the yellow powder, consisting of sulphur and silicon, as already mentioned in a foregoing part of this treatise, was invariably separated: 1.80 grains of this yellowish-gray powder, which swam as a viscid mass on the surface of the liquid, heated on platinum foil, lost 0.80 grains of sulphur. The remaining 1.00 of this yellowish-gray powder, ignited with carbonate of soda, was found to be contracted into a yellowish-brown cake, adhering very strongly to the crucible. Two grass-green drops of manganate of soda adhered to the lid; silica was separated amounting to 0.1611; oxide of iron = 0.855; alumina = 0.028.

If we consider the sulphur united with the iron, the formula resembles a bisulphuret of iron, and we have

Silicon	0.077
Sulphuret of iron . . .	0.945
Sulphur	0.800

1.822

or, if we take the ingredients as they appear,

Silicon	0.07739
Aluminum	0.01342
Iron and manganese .	0.85500
Sulphur	0.80000

1.75081

This yellow powder separated was soluble in aqua-regia, as well as in caustic ammonia.

Further, I dissolved 20 grains of the present specimen of cast iron in aqua-regia; it was quickly and violently attacked, leaving a black coaly residuum. Nevertheless 1.11 grains of the iron remained undissolved, which I carefully washed and separated. The solution was then evaporated to dryness, mixed with five times its weight of soda, and exposed to a white heat in a charcoal fire. The mass was found afterwards to be of a yellowish-brown colour, intermixed with black lumps, and several white drops adhered to the lid. Acidulated water was poured over it, and the whole digested for some time. A light gray muddy liquid was the result; but the mass on the bottom was not much attacked. After changing the acid, its action was very soon stopped again. By adding more acid the action began again very violently; the whole mass in the crucible was converted into a black viscid mass, filling up the whole crucible, while disengaging carbonic acid with a particularly sharp hissing noise. I poured the whole into a china dish. In the green liquid I found swimming a black flocculent mass in considerable quantity. After some time hydrogen gas was developed, and the black mass began to diminish gradually in bulk till all traces of it were lost, and the colour of the liquid, green before, became changed into yellow.

The evolution of hydrogen proved that the black mass must have been a reduced metallic body, either silicon or iron. It is very curious that the alkali shewed such a reducing power, which seems to be derived from the carbonaceous matter combined with the oxide of iron; a case which never occurred to me at any other time.

The silica separated weighed = 0.5148

The oxide of iron ,, 8.9041

9.4189

The chemical constituents of these examples, determined in the usual manner, are shown in the following table, to which I have added the preceding analysis for comparison:

Ingredients.	Gray French Iron.	White Welsh Iron.	Creuzot Iron.	Iron (a).	Iron (b).	Iron (c).	Specimen (d).	Steel.
Silicon ...	4.86430	1.00867	1.0090	1.860	2.006	0.4828	2.9784	0.5204
Aluminum	1.00738	0.08571	0.0606	0.108	0.098	0.0134	0.0876	0.000
Carbon ...	3.38000	4.30000	1.9100	5.800	4.750	2.7500	4.269	1.42800
Azote.....	0.00000	0.76371	0.7205	0.874	0.565	1.0360	0.6386	0.18310
Sulphur...	0.17740	0.32018	1.1050	0.645	0.800	0.3800	0.433	1.00200
Arsenic ...	0.00000	0.00000	0.0000	4.050	2.560	4.0800	3.840	0.93400
Antimony.	0.00000	1.59710	0.0000	0.000	0.000	0.000	0.000	0.12100
Chrome ...	0.00000	0.00000	1.38200	0.000	0.000	0.000	0.000	0.00000

For 35 grains of those irons.

Ingredients.	Gray French Iron.	White Welsh Iron.	Creuzot Iron.	Iron (a).	Iron (b).	Iron (c).	Specimen (d).	Steel.
Residuum in acids. .	5.53	6.7700	12.005	10.890	16.625	9.45	10.045	1.995
Increase of weight after ignition	0.000	0.9084	2.697	0.385	1.880	1.82	0.880	0.059
Flocculent powder after boiling with hydrochloric acid.	(lost.)							
	4.762 gray.	0.6270 gray.	1.603 gray.	2.316 gray.	2.036 black.	0.485 black.	2.044 black.	0.381 gray.

Table inclusive of the increase of weight in the different states of ignition.

	Gray French Iron.	White Welsh Iron.	Creuzot Iron.	(a).	(b).	(c). Loss.	Steel.
1st	0.000	0.173	0.000	0.064	0.1454	1.14	0.059
2nd.....	0.000	0.584	0.450	0.209	0.8000	1.16	0.000
3rd	0.000	0.043	0.846	0.096	0.560	0.80	0.000
4th	0.000	0.000	0.810	0.016	0.218	0.00	0.000
5th	0.000	0.000	0.210	0.000	0.000	0.00	0.000
6th	0.000	0.000	0.162	0.000	0.000	0.00	0.000
7th	0.000	0.000	0.144	0.000	0.000	0.00	0.000
8th	0.000	0.000	0.018	0.000	0.000	0.00	0.000
9th	0.000	0.000	0.054	0.000	0.000	0.00	0.000
10th	0.000	0.000	0.000	0.000	0.000	0.00	0.000

If we treat powdered iron with a current of dry chlorine, a quantity of silicon is always retained by the remaining carbon and azote; and this is likewise a proof that a certain quantity of silicon is chemically combined with the carbon, as no alkali has the power to extract it*.

* In specimens in which the silicon is combined with the iron, the silicon is left after the solution of the iron in acids, in the form of a white and somewhat gelatinous granulation, as we saw in the gray iron from Vienne. On the contrary, where the carbon is combined with the silicon in not too large proportions, even a white heat is insufficient to burn this carbon, as we had examples in the black second remainders of the iron, (b), (c), and (d).

Ingredients.	Gray French Iron.	White Welsh Iron.	Iron from Creuzot.	Iron (a).	b.	c.	d.	Steel.
Carbon and Azote	3.412	5.3920	2.086	6.5000	5.2696	4.02	3.5190	1.7265
Silicon	0.138	*1.008	0.702	0.4804	0.4804	0.23	0.4804	0.2740

Last residuums after ignition and extraction by acids.

Carbon, Hydrogen and Oxygen.....	3.700	3.276	(3.580)	4.7600	3.811	0.912	2.9754	0.3230
Silicon	4.812	1.009	1.000	1.8500	2.006	0.473	2.8671	0.5191
	gray.	gray.	gray.					

We here see the combination of carbon with silicon, the quantity of which is almost always constant. If we further consider the different relations of the numbers in the above tables, we find that the white irons (*a*) (*b*) (*c*) (*d*) are characterized very distinctly by the different proportions of carbon and silicon. Specimen (*c*) was very difficult to treat in the refining furnace, as well as in the puddling oven, and was of a large roundish grain, silver white, and contracting very much during the process of cooling. Specimen (*d*) was almost infusible in the refining as well as the puddling fires, and producing an iron red-short as well as cold-short, which would not weld under any circumstances. The quantities of sulphur and arsenic cannot be the cause of these singularities, as both ingredients are found in a less proportion in (*c*) and (*d*) than in the specimens (*a*) and (*b*). The silicon alone is predominant in the last specimen; but if we compare its quantity of silicon with that contained in the first analysed gray iron from Vienne, we find it there in twice as great a proportion. Nevertheless the iron ran as liquid as water. The real cause of the difference in quality of those two last specimens must therefore be sought for in the different proportions of carbon and silicon combined, and, in reality, the tables show us, that the quantities of silicon in relation to the carbon increase in proportion as the different specimens become less and less fusible. It therefore naturally follows, that the more carbon becomes combined with silicon, the less carbon will be combined with the iron, and consequently our residuums will show the property of becoming ignited by a low degree of heat, and, in fact, the residuum of the last

* If we compare the silicon left by chlorine in the white Welsh iron, we find it equal to the actual determined quantity of silicon. In all other specimens the chlorine had carried away all or a great part of the silicon, viz. where it was combined with the iron, as in the cast iron from Vienne. But in this specimen only almost the whole of the silicon seemed to be combined *with carbon*, on which combination chlorine never acts.

specimen lost after the first ignition instead of gaining as all the other specimens did, and the gain after the first ignition was extremely small in comparison with that of all the other specimens mentioned in the beginning.

If we look back to the specimen of iron first analysed at the Maesteg iron-works, we shall find, that we came to the conclusion, that the second part of this residuum, which during the first ignition neither gained nor lost weight, was combined as follows, in three grains:

Metallic carburet of iron	1·040728
Metallic iron	1·12268
Carbon	0·44138
Silicon	0·159500
Carbon	0·270500

In fact, this property of thus becoming ignited depends entirely on the carburet of iron. The residuum from acids loses this facility of being burnt or oxidised, as well as the corresponding iron in the puddling and boiling furnace, and the difficulty of *converting cast iron into malleable iron* increases in the ratio of the diminution of the carburet of iron in the cast irons.

The specimen (*e*) was made from gray cast iron, produced in the same furnace from the same ore. It melted in the puddling furnace into a very liquid state, and filled the oven during its ebullition with innumerable brilliant sparks, emitting a hissing sound as if a great mass of white hot iron was burning and oxidizing. I made excellent soft malleable iron of it, but the bars had the peculiarity of not welding under any circumstances whatever, notwithstanding the iron was not in the slightest degree red-short.

I kept a pile of this iron, consisting of six single broad puddled bars in a balling or reheating furnace for six hours, during which time the furnace had been several times recharged with similar piles of other iron; yet this pile showed not the slightest inclination to weld: the pile looked as dry as possible in the flames, and seemed to become harder and drier every moment at a white heat, when all the other irons welded easily; and after being taken out of the furnace it was found to be covered with large blisters, exactly like those of blistered steel from the converting furnace. Its outside was silver-white, showing very little traces of oxidation. The inside of the broken bar was very similar in appearance to blistered steel, showing the cubical crystalline form and the large blisters in the inside, covered with the usual colours of blue and yellow. It forged very well, yet hardened but very

little. By actual analysis, a considerable quantity of carbon was found in it, as well as arsenic, but *no trace of silicon*. In the retort, treated with hydrochloric acid as usual, the evolution of gas lasted upwards of three weeks: the evolved gas had *no smell*, which proves that carbon, at *least alone*, cannot be the cause of the bad smell of this description of hydrogen. The residuum of the solution was black, smoking very much during ignition, and leaving a small quantity of dirty red residuum, which was entirely soluble in hydrochloric acid, leaving only a few small black scales behind. The solution contained iron, but no traces of silica.

I must here mention a fact but little known, that all piles of iron which are to be welded in a reverberating-furnace, must rest on a bottom which contains a large quantity of free silica. When the pile of iron is heated in such a reverberating-furnace, the silicon and the iron on the surface of the pile become oxidized, forming a very tough half-melted slag, which does not at all prevent the access of air, and the iron would burn into cinders did not the silica of the bottom combine with the slag next to it, forming a liquid silicate and giving an equal quantity of silica to the uppermost bar of iron in the pile, until this liquid slag is spread over the whole pile and its interstices. Iron piles heated upon a slag bottom will not weld but burn, a circumstance which I found always overlooked.

When iron is heated on a bottom composed of siliceous matter, for a long time and at the highest degree of heat, the silicon of the bottom is reduced by the carbon of the iron as well as of that of the flames, and combining with the iron renders its texture loose, makes it finally melt, and produces that which is usually termed *burnt iron*.

The silicon is, in fact, the cause of the welding property of the iron. Thus silica is sometimes the cause of malleable iron melting in our common fires. The general idea, that malleable iron can be melted even in Sefstrøm's or Knight's blast-furnace, is quite erroneous. An accurate analysis of the iron before and after fusion will soon convince us of the truth of this assertion, and we find invariably that the iron during fusion had combined either with carbon, or with silicon, or with both. We have seen above, that iron, even though it contains a large quantity of carbon, sometimes developes a perfectly inodorous hydrogen, and an inodorous hydrogen is therefore no proof of chemically pure iron. The process of welding iron consists in heating the skeleton grains of iron, contained in the mass, in order to excite all their attractive forces, but at the same time to prevent their combining with any other body, especially carbon, in which case only the

skeleton grains will attract each other and become united. The attractive forces of these grains for carbon are developed only when they are at a white heat, and it is an error, which has remained even in the last edition of Turner's Chemistry, that iron will weld at a red heat. When the grains of iron at a white heat really come into contact with carbon, or when a sufficient quantity of carbon has not been burnt away during the process of puddling, the skeleton grains, instead of adhering to each other, come into a state of fusion during the compressing force of the strokes of the hammer, assume a crystallized form, and produce a sort of cold-short iron.

Chemically-pure iron I could never make to weld. I procured chemically-pure iron by reducing oxide of iron (from which all traces of silica were carefully separated by means of repeated solutions and evaporations) in a current of hydrogen, and inclosed it hermetically in a platinum tube previously filled with dried hydrogen. Several of these tubes were exposed to different degrees of heat, from the dark red-heat to the white-heat, and hammered, in order to give consistency to the inclosed powder of iron; but it never showed any signs of continuity or welding afterwards, and under the microscope itself appeared unaltered.

We have just shown, that silicon principally imparts to the iron the property of welding; but we see likewise in the last specimen of iron examined, that even a considerable quantity of carbon contained in the iron does not impart to it the property of *hardening* after being heated and cooled in water. Besides this last specimen of iron, I melted pure iron with 3 per cent. of charcoal prepared from sugar, in a clay crucible, and, watching the heat very carefully until it had become quite liquid, I poured it into a common ingot mould for cast steel. This iron when broken presented a large round granulation of a bluish-white colour, resembling the specimen *c* before mentioned; under the hammer it forged extremely soft and tough, like Taberg iron, but would not harden at all when dipped red-hot into water, notwithstanding it contained 2.5 per cent. of carbon. But it contained scarcely any traces of silicon; and I found by keeping the liquid mixture for a longer time at a higher degree of heat, the silicon increased, and with it the property of hardening.

We have mentioned in a former paragraph, that iron when heated in a reverberating-furnace in contact with siliceous matter, imbibes a portion of silicon; but the question now is, why the specimen *e*, before-mentioned, did not imbibe silicon from the bottom of the furnace, but combined instead with the carbon of the decomposed flame? The answer is obvious. In the cast iron from which the specimen *e* was pre-

pared, silicon, iron, and arsenic were combined. Arsenic in combination with silicon has the property of rendering the latter more easily oxidized, so that the greater part of the silicon is consumed before the arsenic, which occasioned the extraordinary hissing noise, already mentioned, during the process of ebullition of the specimen *e* in the puddling furnace. The malleable iron thus prepared had entirely lost its silicon, without which no peroxide of iron could be formed sufficiently liquid to resist the reducing power of the flames. The protoxide, wherever it was formed, consisted of a dry powder, which was speedily reduced by the action of the flame into its former metallic state, combining at the same time with carbon, and gradually changing the whole mass into a carburet. When, on the contrary, the protoxide contains sufficient silica, a very liquid silicate of iron is generated, which, not capable of being reduced by the flames, spreads itself over the entire surface, and likewise prevents the action of the flame upon the iron. The state in which the molecules of malleable iron as well as cast steel exist, seems never to have been taken into consideration, and both malleable iron as well as malleable steel were considered to differ from cast iron and cast steel only so far as regarded their chemical properties. But this is altogether an error. Malleable iron and malleable steel owe their properties to the mechanical force of the hammer; and as soon as they lose the peculiar arrangement of their molecules, produced by the hammer, these properties are entirely changed*. The preparation of malleable iron from cast iron shows this very distinctly. The iron is brought into a half-melted state, in which state the larger crystals of the iron, called grains, lose their attractive power in respect to position; but the smaller crystalline compounds of the molecules of iron never lose their form or structure, but retain them during the whole process of puddling, and the slag rising only keeps the small crystals of the iron separate, and by enveloping them prevents their acting directly one upon the other. The softened, but not liquid, grains of the iron begin now to abstract oxygen from the surrounding slag, which is immediately replaced by the *oxygen of the air*†; and thus gene-

* That peculiar sort of steel from which in steel pen manufactories the extremely fine chisels are made to cut the slit into the pen, is entirely produced by long-continued and judiciously-applied hammering.

† By putting a cap of sheet iron, resembling the head of a still, into the boiling iron, the tube of which extending through the door of the furnace dips into water or quicksilver—we soon perceive that air is absorbed, and the water begins to rise in the tube. By blowing an uninterrupted current of air into this apparatus, the boiling is soon re-established;

rating with the carbon of the iron, carbonic oxide gas, as well as carbonic acid ; and the evolution of these gases causes the well-known ebullition of the whole mass. In this separated state of the molecules, it is very difficult for heat to be brought to act on them ; and it is, in fact, well known to all workmen, that several metals are the more difficult to re-melt the more minute their state of division.

Each grain of the iron, not being in a fluid state, by losing the carbon, silicon, &c., is converted into the base of malleable iron, leaving behind only a skeleton of the grains of cast iron. All the crystalline planes are destroyed ; and therefore, instead of the crystals adhering to each other with their planes of crystallization by a force easily to be overcome, an innumerable quantity of points of the skeleton of the crystals adhere together in all directions of the adhesive force ; and as we have before observed, that the skeleton grains are naturally not in a liquid state, the spaces formerly occupied by the carbon, silicon, and portions of iron, which have been consumed, are still vacant, consequently the iron in this state exerts a very great decomposing power upon all chemical bodies, as is the case with oxide of iron reduced at a low degree of heat by hydrogen. This fact likewise explains the circumstance, which I have often observed, that by using bad coals, or when the draught of the furnace is not well regulated, the iron which during the first half of the process of puddling had lost all its sulphur by chemical means, exhibited at the end of the process more sulphur than the cast iron actually contained before the puddling commenced.

The vacant spaces in the skeleton grains which constitute the lumps of iron made in the puddling furnaces, are immediately closed when brought under the forge hammer, and consequently the decomposing power ceases ; but where the malleable iron thus obtained is again kept at a white heat for a considerable space of time, the closed pores in some measure re-open, and their attractive force towards chemical agents begins to re-appear, and thus, to mention one instance, the iron combines in the cementing furnace with carbon, forming *cemented steel*, without altering the juxtaposition of the silicon and iron molecules in the cemented bar. Such a cemented bar, when exposed to a proper degree of heat, will weld to another similar bar, as the mechanical texture of the iron had not been altered. But it is different when the cemented steel, instead of being softened only, is reduced to a perfect liquid state by melting. In this case the iron and si-

and I used a similar apparatus, for ascertaining the nature of the escaping gases, after the boiling mass had been mixed with different chemical agents,

liron combine more closely with the carbon which they imbibe during cementation. The carburet of silicon partially separates from the carburet of iron, and crystallizes during a slow process of evolving carbonic oxide gas, particularly when no more than one definite carburet of iron is contained in the bar of steel. By these means the peculiar appearance by which the Damascus steel is distinguished is produced*. As Wootz, or Indian steel, is only a cast iron, and has therefore not lost any portion of its silicon or aluminum, which a malleable iron bar always has to a greater or less extent lost, and which it never recovers during cementation, those crystals of silicon or aluminum are more developed in Indian steel, than in that made in the common way from malleable iron. As, therefore, iron during cementation is only capable of imbibing carbon, it is evident that the silicon and other electro-negative metals must be already contained in the iron in order to produce good steel, and for this reason certain descriptions of iron only are capable of producing good steel. All iron, and particularly English iron, has during the process of puddling lost a much greater part of its silicon than of its carbon, and its grains are intermixed with an extremely thin layer of a supersilicate of iron, which during cementation is only partially reduced, and leaves a different silicate between the grains of the bar, which makes the steel thus produced invariably red-short. Iron prepared from pure oxide of iron in so-called German fires, or even by a different process in a puddling furnace, is at large intervals only interwoven with almost pure protoxide and peroxide of iron, which during cementation is entirely reduced. The carbonic oxide produced occasions those large blisters from which

* As a proof that a real separation takes place in melted steel between the carburets of silicon and iron, I refer to an experiment made in Mr. Wilson's steel manufactory at Sheffield, when I melted English iron with lamp black and charcoal from sugar. The molten mixture was poured into an ingot mould used for common cast steel, one side of which was hotter than the other. After cooling the metallic parallelepipedon, one half of its cross-fracture was observed to be crystallized, and the other granulated. When drawn out under the tilting hammer, a bar was obtained; one half of its short axis, corresponding to the crystallized side of the ingot, was perfect steel, and the other half the softest iron, just as if a steel bar had been welded to a soft iron bar. The soft part of the bar contained a great quantity of carbon, but merely traces of silicon, too small to be weighed.

In another similar experiment, after the liquid steel had been poured into the ingot mould, a lump of metal the size of a hen's egg remained at the bottom of the crucible. On perceiving this, I put the steel back into the crucible, and re-melted it at the strongest heat I could obtain; and on again pouring it out, I found the lump, still remaining at the bottom of the crucible quite unaltered; 35 grains of this lump, dissolved in hydrochloric acid, left only 0.198 grains of a grayish residuum.

cemented steel has obtained the name of blistered steel, and the oxygen of the part of this gas which is in contact with the sides of the blister occasions those fine tints of yellow and blue with which the inside of the blisters is generally covered.

It is generally asserted that the outside of the cemented bar of steel contains more carbon than the inside, and that therefore the fusion of the cemented bar served to spread the carbon more equally throughout the mass. This is contrary to fact. When the iron bar which is to be cemented is of a suitable thickness, the process of carbonization commences, like the process of the reduction of iron ores, almost simultaneously at the centre and at the outside of the bar; and I have often found that the inside of the cemented bar contained a greater quantity of carbon than the outside. The quantity of carbon imbibed by each portion of the bar depends entirely on the quantity of carbon first contained in the different parts of the iron bar, as well as on the carburet of silicon contained in it; and this different degree of carbonization may even be detected by the eye, from the various forms of crystallization contained in the bar.

This, as well as other chemical phenomena which have not received sufficient attention, may be explained by the law which causes one atom of water in the voltaic circuit to evolve hydrogen and oxygen at the same moment at two distant points.

In conclusion, I annex an analysis of the best English cast steel, to controvert the modern assertion that the best cast-steel bars consist only of a pure compound of carbon and iron. The analysed specimen was a fragment of an excellent razor, forged in my presence, in the workshop of Mr. Rodgers, of Sheffield, of the specific gravity of 7.92.

Silicon . . .	0.52043
Aluminum . . .	0.00000
Manganese . . .	1.92000
Arsenic . . .	0.93400
Antimony . . .	0.12100
Tin	Traces.
Phosphorus . . .	0.00000
Sulphur . . .	1.00200
Azote	0.18310
Carbon	1.42800
Iron	93.79765
Loss	0.09382

100.00000

35 grains of this steel dissolved in hydro-chloric acid of sp. gr. = 1.104, deposited on the vault of the retort, soon after the acid began to act upon the iron, a dark ring of carbonaceous matter twice the diameter of the space occupied by the steel filings at the bottom of the retort. The inside of this ring was gradually filled up by the black residuum, whilst at the same time it collected in rays around the filings at the bottom of the retort, which filings in the same ratio disappeared, till the space formerly occupied by them was filled up entirely by the black powder, gradually increasing towards the centre, each grain of this black powder retaining the form of the particle of steel filing from which it had its origin. The action of the acid and the evolution of gas had not ceased in three weeks. The residuum, of a dark brown greenish colour, was equal to 1.995 grains. Heated in a platinum crucible, a single bright spark appeared towards the centre, which immediately disappeared. Long after this the mass ignited within the crucible, and had increased in weight = 0.959. After being treated by hydrochloric acid, 0.381 of silica remained, contaminated with a little iron. The greatest quantity of arsenic was found to be contained in the acid from the retort.

Arsenic, sulphur, silicon, and azote are constituent parts of all the best English steel which I have analysed, made from the Dannemora iron, called Hoop L. and double Bullet. Their relative proportions in all specimens are nearly the same. In some specimens of very hard steel I found the carbon increased to 1.69 per cent. It is scarcely necessary to add, that the arsenic, antimony, and tin were precipitated together by a current of sulphuretted hydrogen, and sometimes by means of hydro-sulphuret of ammonia. The precipitate was divided into two equal parts; one part dissolved in aqua regia, the diluted solution mixed with tartaric acid and the quantity of sulphuric acid ascertained in the usual way. The other portion of the precipitate obtained by sulphuretted hydrogen was heated carefully in a glass capsule in a current of dry hydrogen, till the arsenic was driven off. The antimony and tin were of course left behind. I endeavoured to separate both these, as proposed by Gay Lussac, by dissolving the residuum in aqua regia, and precipitating one half of the solution by means of metallic zinc; the other by metallic tin, which only separates the antimony. But to gain an exact result, a larger quantity of the material for analysis is necessary than can be conveniently obtained by analysing iron.

If a current of sulphuretted hydrogen is driven through an

acid solution of peroxide of iron, with other metals and silica, the silica invariably falls down with the sulphurets, perhaps in the state of a sulphuret, and remaining, after the treatment of the sulphurets with aqua regia, in such a state as to be insoluble in all acids except hydrofluoric acid.

We cannot enough recommend the utmost care in examining the precipitate obtained by sulphuretted hydrogen from solutions of iron, and *all* the contents of this precipitate ought to be always separated and tried to be procured in their isolated state.

By trying to separate phosphoric acid from iron, by means of alkalies, the phosphoric acid can only be separated entirely from the iron when the mixture shall be kept in a perfect white heat for some time.

Since writing this article several months ago, some remarks have occurred to me, which may serve to elucidate it.

According to the experiments in the previous paper, we consider, that the toughness of the black and gray sorts of cast iron is owing to the siliciuret of iron, while their qualities of strength and fusibility are attributable to the carburet of aluminum, silicon and iron. I must here observe, that really gray cast iron, used for foundry purposes, never changes its appearance from gray into white without changing its chemical composition; while the white crystallized cast iron, produced on the continent from spathose iron ores, at a comparatively low degree of heat, changes its appearance from white into apparently gray cast iron, according to the degree of slowness with which it is cooled. But this ready conversion of white iron into gray iron is only apparent, and the above-mentioned crystallized iron bears, in either of its states, the chemical character of white iron, in which a part of the silicon is replaced by manganese. When apparently converted into gray iron by slow cooling, it has only changed the state of aggregation of its component molecules, and consequently its density; or, in other words, the molecules of this sort of iron have had time to arrange themselves during its cooling into a more developed crystalline form. This crystalline form may be easily distinguished from the regular foliated form of crystallization of real gray cast iron, when viewed under the microscope, by the irregularity, smallness, and thickness of its component leaves or scales, and a stroke of the hammer will invariably restore to the part struck the original white silvery colour peculiar to it. The residuum of both varieties of this iron, after its treatment with hydrochloric acid, have all the characters of the residuum of white iron; they are brown, instead of white or gray, become ignited at a very low degree of heat, and never effervesce with caustic ammonia.

The crystalline form of cast iron generally depends on the relative atomic combination of carbon with silicon. The hardness and whiteness of the compound decreases with the increase of carbon, and has reached its utmost degree of friability in that sort of supercarburet of silicon, which is called *graphite*, or by the iron-smelters, *kish*.

I procured a beautiful specimen of this sort of graphite or *kish* a few weeks ago from one of the blast-furnaces at Merthyr-Tydvil. A rather porous piece of slag, of a yellowish green colour, like impure sulphur, was found to be interwoven with a graphitic formation, consisting of large irregular layers of different sizes, and of a dusty grayish graphitic hue. These large layers or laminæ were found to be composed of smaller rhombic scales, lying one over another, similar to the tiles of a roof, and giving to the surface the appearance of a regular rhombic network*.

The composition of the large laminæ was found to be different in different parts of their thickness. The scales of graphite on the outside were soft, light, and so easily divisible as to soil the fingers. They increase in thickness and become more dark-coloured towards the middle, and the central layer had the appearance and hardness of black cast iron, and its somewhat conchoidal fracture had a lustre betwixt those of glass and pitch. The exterior and thinnest scales were not attracted by the magnet at all; but the interior ones were affected by the magnet almost in the ratio of their increasing thickness.

In hydrochloric acid, the central layer evolved hydrogen rapidly, first a white, and afterwards a yellowish scum of silica were separated, and it showed in fact all the properties of the blackest cast iron.

The scales adjoining were strongly attracted by the magnet, and appeared under the microscope to be covered with small flattened crystals, forming an irregular six-sided prism, of which only four sides were developed; in a similar manner only two opposite sides of the rhombic faces of each end of the crystal were left, corresponding to the smaller sides of the prism.

Those small crystals seemed to constitute a central point, from the sides of which the small leaves of graphite forming the surface of the laminæ appeared to radiate.

I succeeded in separating one of those largest crystals, and in covering it under the microscope with a drop of concentrated

* [In Phil. Mag. First Series, vol. xl. p. 41, will be found an examination, by Mr. (now Prof.) E. Davy, of a native graphite considered by him strongly to resemble *kish*.—EDIT.]

hydrochloric acid. The acid did not attack the crystal until heat was applied, and then quickly formed around the crystal a framework of white tough silica, apparently consisting of leaves or parallel threads, corresponding to the sides of the nucleus, which after having been separated from the silica with a fine needle, was finally converted entirely into a spot of silica.

By repeatedly treating one of the large graphite layers or laminae with boiling hot hydrochloric acid and alkalies, it increased in blackness and brilliancy; the single leaves appeared thinner, their mutual connection was loosened, and the magnet had no further action on them.

With the exception of hydrofluoric acid no single chemical liquid seemed to have any action on those scales, and only the most concentrated hydrofluoric acid slowly attacked them, when in a state of most minute division.

After several fruitless efforts, I finally discovered a new method of decomposing them by means of acids, which gave rise to new and interesting phenomena.

I poured about three fluid-drachms of concentrated sulphuric acid over two grains of these purified graphite scales in a deep platinum crucible, and made the acid boil briskly over a spirit lamp. After this, I removed the crucible from the fire, till the dense fumes which arose began somewhat to cease. I then drew up about one fluid-drachm of strong fuming nitric acid into a long small glass tube, and dropped one half of the acid rather slowly, the other half quickly, into the hot sulphuric acid, which caused the latter to boil again, during a rapid evolution of binoxide of nitrogen. As soon as the boiling began to cease, I placed the crucible again over the lamp, and boiled the liquid till all the nitric acid was decomposed. I found the scales of graphite so much swollen as to fill up the whole lower part of the crucible, so that the liquid was no longer visible. On nearer inspection, I perceived that every single leaf of those scales was converted into a spongy body, of the lustre of coke, and of the same breadth and thickness, about the size of a pea.

Washed with distilled water, and dried at 212° Fahr., those spongy masses weighed 2.18 grains, and lost after ignition 0.39 grains. No degree of heat to be obtained by a large spirit lamp caused any further alteration. Their appearance in this state nearly resembled in lustre and texture pieces of hard coke and foliated charcoal; they were composed of four to five easily separable layers, which were again intersected by several cracks or fissures vertical to them, similar somewhat to the structure of charred wood, and their edges only

appeared of the peculiar metallic lustre of coke. Even the smallest pieces being placed on a platinum foil resisted for a long time all the effects of the flame, but at length began to ignite and to be consumed quickly, always leaving a grayish or brownish residuum, which consisted of silica with a little iron.

As I treated those spongy ignited masses again with sulphuric and nitric acid, in the manner above mentioned, I found their bulk considerably diminished; and after a repetition of the same operation for the fourth time, the last trace of graphite had disappeared, and the acid remained perfectly clear. Diluted and saturated with caustic ammonia, a white, light flocculent precipitate fell, and the whole liquid, evaporated to dryness, and ignited, left a brownish residuum consisting of silica with a little alumina and iron.

The graphite obviously had here been converted into carbonic acid by means of nitric acid; but it is a very curious fact, that this conversion takes place only under the above-mentioned circumstances.

Concentrated nitric acid, dropped on the red-hot graphite, has not the slightest action on it; neither has sulphuric acid dropped into boiling nitric acid. To obtain the expected results, the above precept must be followed strictly, and the crucible must be spacious, as with every drop of nitric acid falling into the sulphuric acid a slight explosion takes place, which might occasion the loss of some of the liquid.

A somewhat probable explanation of the singular action of both acids combined in this manner, seems to be, that the boiling sulphuric acid absorbs the water from the nitric acid, the oxygen of which is only able to combine with the carbon of the supercarburet of silicon at the moment when the sulphuric acid combines with the water of the nitric acid. If the residuum of gray cast iron dissolved in hydrochloric acid be treated in the same way, all graphite scales disappear, and only white silica remains.

LXXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Dec. 5, **N**O paper read.

1839. Dec. 12.—“On the Nerves of the Gravid Uterus.”
By Robert Lee, M.D., F.R.S.

The author, while dissecting a gravid uterus of seven months, on the 8th of April, 1838, observed the trunk of a large nerve proceeding upwards from the cervix to the body of that organ along with

the right uterine vein, and sending off branches to the posterior surface of the uterus; some of which accompanied the vein, and others appeared to be inserted into the peritoneum. A broad band, resembling a plexus of nerves, was seen extending across the posterior surface of the uterus, and covering the nerve about midway from the fundus to the cervix. On the left side, a large plexus of nerves was seen, surrounding the uterine veins at the place where they were about to enter the hypogastric vein. From this plexus three large trunks of nerves were seen accompanying the uterine vein, which increased in size as they ascended to the fundus uteri. From the nerve situated on the posterior surface of the vein, numerous filaments passed off towards the mesial line, as on the right side; some following the smaller veins on the posterior surface of the uterus, and others becoming intimately adherent to the peritoneum. The largest of the nerves which accompanied the uterine vein was traced as high as the part where the Fallopian tube enters the uterus; and there it divided into numerous filaments, which plunged deep into the muscular coat of the uterus along with the vein. A large fasciculated band, like a plexus of nerves, was also seen on the left side under the peritoneum, crossing the body of the uterus; and several branches, apparently nervous, proceeding from this band, were distinctly continuous with some of the smaller branches of nerves accompanying the uterine veins. The preparation of the parts was placed in the Museum of St. George's Hospital, on the 1st of October, 1838; and several anatomists who examined it were of opinion that they were absorbents accompanying the uterine veins, and tendinous fibres spread across the posterior surface.

Dr. Lee availed himself of another opportunity which presented itself, on the 18th of December of the same year, of examining a gravid uterus in the sixth month of pregnancy, which had the spermatic, hypogastric and sacral nerves remaining connected with it; and during the last ten months, he has been diligently occupied in tracing the nerves of this uterus. He believes that he has ascertained that the principal trunks of the hypogastric nerves accompany, not the arteries of the uterus, as all anatomists have represented, but the veins; that these nerves become greatly enlarged during pregnancy; and that their branches are actually incorporated, or coalesce with the branches of the four great fasciculated bands on the anterior and posterior surface of the uterus, bearing a striking resemblance to ganglionic plexuses of nerves, and sending numerous branches to the muscular coat of the uterus.

The author gives the following description of the nerves of the gravid uterus in the sixth month, and of these fasciculated bands as displayed in the dissection.

Behind the uterus, the aortic plexus divides into two portions, to form the right and left hypogastric plexuses. These plexuses, after an intimate union with the nerves accompanying the ureters, descend to the neck of the uterus, upper part of the vagina, and contiguous parts of the bladder and rectum, where they are joined by branches from the third and fourth sacral nerves. The left hypogastric plex-

us, about two inches below the aortic plexus, sends off a large branch, which passes on the inside of the ureter to the superior uterine vein, where it is about to terminate in the hypogastric vein. Here the nerve suddenly expands, becomes broad and thin, and passes into a great plexus of nerves, which completely encircles the vein. This plexus, surrounding the uterine vein, is joined below by two large branches, which proceed from the hypogastric plexus nearer the vagina, and lower down, and from which branches pass on the outside of the ureter. From the upper part of this plexus, surrounding the uterine vein near its termination, three large trunks of nerves proceed upwards with the vein to the superior part of the uterus, and enlarge as they ascend. The posterior branch of these hypogastric nerves sends off in its course smaller branches, which accompany the ramifications of the uterine vein on the posterior surface of the uterus. Passing upwards beyond the junction of the spermatic with the uterine vein, and running between the peritoneum and the left posterior fasciculated band, it spreads out into a web of thin broad branches and slender nervous filaments, some of which are inserted into the peritoneum, and others follow the vein to the fundus uteri, which they completely surround as the vein passes down into the muscular coat of the uterus.

Some of the branches of this nerve, near the fundus uteri, are distributed to the muscular coat, but these are small and few in number.

The middle and anterior branches of the hypogastric nerves adhere closely to the uterine vein as they ascend, and form around it several plexuses, which completely invest the vessel. From these plexuses branches are sent off to the anterior surface of the uterus, some of which, in an arborescent form, follow the trunk and branches of the uterine artery. These two hypogastric nerves ascend, and closely unite with the left posterior fasciculated band.

On the left side of the uterus this band arises near the mesial line, on the back of the uterus, midway between the fundus and cervix, from a mass of fibres, which adhere so firmly both to the peritoneum and muscular coat that it is difficult precisely to determine their arrangement. From these fibres the band proceeds across the uterus, in the form of a thin web, to the point where the spermatic vein is leaving the uterus. After closely uniting with the hypogastric nerves, this band proceeds outwards to the round ligament, becoming less firmly adherent to the peritoneum, where it unites with the left anterior band, and spreads out into a great web, under the peritoneum. The left posterior band is loosely attached, through its whole course, to the subjacent muscular coat by soft cellular membrane.

The spermatic nerves on the left side pass down to the ovarium with the spermatic artery, and first give off several branches to the corpus fimbriatum. A few small branches are then sent into the outer end of the ovary. The spermatic nerves afterwards leave the artery, and proceed with the veins to the uterus, where they firmly unite to the outer extremity of the left posterior band; and after the junction of this band with the prolongations of the anterior

band under the round ligament, numerous small, delicate filaments, apparently nervous, are sent to the base of the ovarium.

On the right side of the uterus, the author finds that the distribution of the hypogastric and spermatic nerves does not essentially differ from that now described as seen on the left side. The form and situation of the right posterior band is, he states, much more clearly seen than on the left side, and presents the appearance of a white pearly fasciculated membrane about a quarter of an inch in breadth, proceeding from the mesial line at right angles to the hypogastric nerves, across the body of the uterus, to the round ligament, where it unites with the anterior band. Numerous branches, strikingly resembling the branches of nerves, are sent off from the upper and lower edges of this band, and from its posterior surface to the muscular coat of the uterus. An extensive and intimate union at various points is distinctly perceptible between these branches sent off from the band and the branches of the hypogastric nerves. On the anterior and upper part of the neck of the uterus, there is a great mass of reddish-coloured fibres, firmly interlaced together, resembling a ganglion of nerves, into which numerous large branches of the hypogastric nerves on both sides enter, and to which they firmly adhere. From the upper part of this fibrous substance there passes up, over the whole anterior surface of the uterus, a thin band of firm white fasciculated fibres, prolongations of which extend to the round ligaments,—into which, and into the posterior band, they are continued by numerous filaments, like those of nerves. From the posterior surface of this great band, numerous branches, also apparently nervous, can be traced to a considerable depth through the muscular coat of the uterus.

The author concludes his paper with the following remark, and a short historical account of the progress of discovery on the subject of the nerves of the uterus:—

“From the form, colour and general appearance of these fasciculated bands, and the resemblance they bear to ganglionic plexuses of nerves, and from their branches actually coalescing with the hypogastric and spermatic nerves, I was induced to conclude, on first discovering them, that they were nervous plexuses, and constituted the special nervous system of the uterus. The recent examination, however, of the gravid uterus of some of the lower animals, in which I have found a structure similar to those bands in large quantity under the peritoneum, has left me in considerable doubt as to the nature of these bands, and until further investigations have been made, I shall not venture to pronounce a positive opinion respecting them.”

The description of the nerves of the uterus contained in Professor Tiedemann's splendid work, the author adds, is usually referred to by anatomical writers as the most accurate and complete which has ever been given. Professor Tiedemann has represented the spermatic nerves as being distributed chiefly to the ovarium; and the hypogastric as invariably accompanying the trunk and branches of the uterine arteries, along the sides of the uterus,—dividing into smaller branches, and quickly disappearing in the muscular coat of the ute-

rus. He has made no mention of the large nervous trunks on both sides of the uterus, which accompany the uterine veins; nor has he noticed fasciculated transverse bands on the anterior and posterior surfaces of the uterus, connected with the hypogastric and spermatic nerves.

“Observations made at the Cape of Good Hope, in the year 1838, with Bradley’s Zenith Sector, for the verification of the amplitude of the Abbé de la Caille’s Arc of the Meridian; by order of the Lords Commissioners of the Admiralty.” By Thomas Maclear, Esq., M.A., F.R.S., &c. Communicated by Sir John Barrow, Bart., V.P.R.S., &c.

The author gives an account of the precautions taken in putting together the different parts of the zenith sector, which he received on the 9th of December, 1837, in erecting it in the central room of the Royal Observatory at the Cape of Good Hope, and in afterwards transferring it to the southern station of La Caille, in Cape Town. He then proceeds to describe La Caille’s observatory, and the particular circumstances of its locality, with relation to the object in view, namely to determine the influence of Table Mountain on the direction of the plumb-line*. He next relates his progress to Klyp Fonteyn, where he arrived on the 24th of March, 1838, and describes the operations resorted to for erecting the sector at that place. He then enters into the details of observations made at different stations, and especially with comparative observations at the summit and foot of the mountain of Pequet Berg. The instrument was lastly conveyed back to Cape Town, and again examined, and the observations made with it repeated. The reduction of the observations occupies the remainder of the paper; and in conclusion, the author remarks, that although these labours have not altogether cleared up the anomaly of La Caille’s arc, yet they show that great credit is due to that distinguished astronomer, who with imperfect means, and at the period in which he lived, arrived at a result, derived from sixteen stars, almost identical with that from 1139 observations on forty stars, made with a celebrated and powerful instrument.

Dec. 19, 1839.—A paper was read, entitled, “An account of experiments made with the view of ascertaining the possibility of obtaining a spark before the circuit of the Voltaic Battery is completed.” By J. P. Gassiot, Esq.

The author of this paper adverts to the fact, of a spark invariably appearing when the circuit of the Voltaic Battery is completed; an effect which Dr. Faraday has shown can be easily produced, even with a single series. He then refers to the experiments of Mr. Children, Sir Humphry Davy, and Professor Daniell, recorded in the Philosophical Transactions; in which experiments, when more powerful and extended series were used, the spark was obtained before contact took place.

In order to ascertain, not only the fact of a spark being obtained, but also the distance through which it may be passed, the author had an instrument prepared, which he denominates a *Micrometer*

[* See L. and E. Phil. Mag., vol. xiv., p. 522.—EDIT.]

Electrometer, and by which an appreciable space of one five-thousandth of an inch could be measured with great accuracy. He describes this instrument; and relates several experiments which he made with a view to test the correctness of its action. He first prepared 160, and then 320 series of the constant battery, in half-pint porcelain cells, excited with solutions of sulphate of copper and muriate of soda; but although the effects, after the contact had been completed, were exceedingly brilliant, not the slightest spark could be obtained. He was equally unsuccessful with a water battery of 150 series, each series being placed in a quart glass vessel; and also with a water battery belonging to Professor Daniell, consisting of 1020 series; but when a Leyden battery of nine jars was introduced into the circuit of the latter, sparks passed to the extent, in one instance, of six five-thousandths of an inch.

The author mentions his having been present at the experiment of Professor Daniell, on the 16th of February, 1839, when that gentleman had 70 series of his large constant battery in action; and having been witness of the powerful effects obtained by this apparatus, he was induced to prepare 100 series of precisely the same dimensions, and similarly excited: but although this powerful apparatus was used under every advantage, and the other effects produced were in every respect in accordance with the extent of the elements employed, still no spark could be obtained until the circuit was completed; *even a single fold* of a silk handkerchief, or a piece of dry tissue paper, was sufficient to insulate the power of a battery, which, after the circuit had been once completed, fused titanium, and heated 16 feet 4 inches of No. 20 platinum wire.

The author then describes a series of experiments made with induced currents. Twelve hundred and twenty iron wires, each insulated by resin, were bent into the form of a horse-shoe. A primary wire of 115 feet and a secondary of 2268 feet, were wound round the iron wires. With this arrangement he obtained a direct spark (through the secondary current), sufficient to pierce paper, to charge a Leyden jar, &c. Several forms of apparatus employed by the author are next described, and also a series of 10,000 of Zamboni's piles. With this arrangement he charged a Leyden battery to a considerable degree of intensity, and obtained direct sparks of three-fiftieths of an inch in length. He ultimately succeeded in obtaining chemical decompositions of a solution of iodide of potassium, the iodine appearing at the end composed of the black oxide of manganese.

The Society then adjourned over the Christmas Vacation, to meet again on the 9th of January, 1840.

Jan. 9, 1840.—A paper was read, entitled, "On the construction and use of Single Achromatic Eye-Pieces, and their superiority to the double eye-piece of Huyghens." By the Rev. J. B. Reade, M.A., F.R.S.

The author observes, that experience has shown it to be impracticable to make a telescope even approach to achromatism, by employing the same object-glass with an astronomical, as with a terrestrial eye-piece: for if the focus of the blue rays from the object-glass be

thrown forwards, as it must be in order to make it impinge upon the focus of the blue rays of the terrestrial eye-glass, then there will be produced a great *over-correction* for the astronomical eye-glass; and *vice versa*. Hence it appears that the application of Huyghenian eye-pieces to refracting telescopes, is incompatible with the conditions of achromatism, throughout the entire range of magnifying power; and that in reflecting telescopes they unavoidably introduce dispersion, because they are not in themselves achromatic. These defects the author proposes wholly to obviate, by substituting for the Huyghenian eye-pieces, single achromatic lenses of corresponding magnifying power; consisting of the well-known combination of the crown-lens, and its correcting flint-lens, having their adjacent surfaces cemented together; thus avoiding internal reflections, and enabling them to act as a single lens. The achromatic eye-pieces which he uses were made by Messrs. Tulley and Ross, and are of the description usually termed *single cemented triples*.

A paper was also read, entitled, "Meteorological Observations made between October, 1837, and April, 1839, at Alten in Finmarken." By Mr. S. H. Thomas, chief mining agent at the Alten Copper Works. Presented to the Royal Society by John R. Crowe, Esq., Her Britannic Majesty's Consul at Finmarken. Communicated by Major Edward Sabine, R.A., V.P.R.S.

This memoir consists of tables of daily observations of the barometer and thermometer, taken at 9 A.M., 2 P.M., and 9 P.M., with remarks on the state of the weather, at Kaafjord, in latitude $69^{\circ} 58' 3''$ north, and longitude $23^{\circ} 43' 10''$ east of Paris.

Jan. 16.—A paper was read, entitled, "On Nobili's Plate of Colours; in a Letter from J. P. Gassiot, Esq., addressed to J. W. Lubbock, Esq., V.P. and Treasurer R.S." Communicated by J. W. Lubbock, Esq.

The effect produced by the late Signor Nobili, of inducing colours on a steel plate, excited the curiosity of the author, and led him to the invention of the following method of producing similar effects. Two of Professor Daniell's large constant cells were excited with the usual solutions of sulphate of copper and sulphuric acid. A highly-polished steel plate was placed in a porcelain soup-plate, and a filtered solution of acetate of lead poured upon it. A piece of card-board, out of which the required figures had been previously cut with a sharp knife, was then placed upon the steel-plate. Over the card, and resting on it, there was fixed a ring of wood, a quarter of an inch thick, and the inner circumference of which was of the same size as the figure. A convex copper-plate was made so that its outer edge might rest on the inner part of the wooden ring; and its centre placed near, but not in actual contact with the card-board. Connexion was then made by the positive electrode of the battery with the steel-plate; the negative being placed in the centre of the copper convex plate. The figure was generally obtained in from 15 to 35 seconds. If a concave, instead of a convex plate be used, the same colours are obtained as in the former experiment, but in an inverse order.

"Geographical position of the principal points of the Triangula-

tions of the Californias and of the Mexican coasts of the Pacific, with the heights of the principal points of that part of the Cordilleras." By the Comte Vincent Piccolomini ; in a letter addressed to Sir John F. W. Herschel, Bart., V.P.R.S. Communicated by Sir John Herschel.

Hauteurs des principaux points des Cordilleres, des côtes de l'Océan Pacifique du Mexique, et de la haute et basse Californie.

Elévation en pieds français sur le ni- veau de la mer pacifique.		Elévation en pieds français sur le ni- veau de la mer pacifique.	
Volcano di Orizaba	18728	Tasco	5971
Volcano di Popocatepetl..	17812	Temascaltepec	5760
Volcano di Tztlacihuatl...	15698	Guernavaia.....	5447
Rio frio	10948	Tchuacan	5398
Real del Monte	10570	Xantetelco	5030
El oro.....	8995	Cuicatlan	5028
Tlalpuhahua	8435	Oajaca	5024
Ameia.....	8247	Cuantla	4587
Naupalucan	8194	Talapa	4542
Las Vigas	7918	Acayucan	4485
Perote.....	7911	Coscomatepec	4451
Ozumba (Etat de Puebla)	7874	Huatusco	4424
Tepeyahualco.....	7702	Talostoc	4421
Ozumba (Etat de Mexico)	7620	Lautepec.....	4019
S. Rosa	7565	Orizaba (ville)	3998
Lagunas de Chalco	7510	Real de Christo	3851
Mexico	7450	Huaitla	3336
Tepeaca	7444	Cordova	2769
Huehuetaca	7121	Dominguillo	2274
Puebla	7078	Villalé.....	1578
Tula	6613	Petapa	617
Tlacotepec	6479	Tehuantepec (Océan Paci- fique.).....	132
Zacualpan	6181		

Position géographique des principaux points de la Triangulation des Californies et des côtes de l'Océan Pacifique du Mexique par le Comte Vincent Piccolomini.

	Latitude.	Longitude cal- culée de Green- wich.
Volcanos de las Virgenes.....	29° 91' 14.35"	121° 46' 30.82"
Cap S. Lucas (Basse Californie).....	23 08 35.27	109 82 15.04
Monterrey (Haute Californie).....	36 58 17.85	121 46 53.09
Guaymas (Département de Sonora)	27 55 00.48	111 45 37.42
Matamoros (Texas).....	25 59 22.07	97 54 18.76
Id. Id. Barra grande de S. Yayo	26 30 27.15	...
Id. Id. Barra del Rio	25 53 03.11	...
Bejar (Texas)	29 74 88.93	98 85 17.74
Mine d'or de S. Yago de los Caballeros	25 13 77.04	106 67 15.87
Vulcans de Tuxtla	18 47 25.91	94 07 43.11
Namampateptl (Province de Vera Cruz)	19 21 48.71	
Bahia de San Francisco (au cap los Reyex dans la Haute Californie)	37 59 17.29	122 37 13.04
Port de San Blas, tour de l'église (Guadalajara) .	21 67 05.54	105 43 17.28
Volcanos de Colima.....	19 03 45.17	103 21 47.04

N.B. L'instrument employé pour déterminer les Longitudes était

un Chronomètre de O. H. Bestor; pour la mesure des triangles de premier ordre je me servis d'un théodolite de dix pouces de diamètre sortant des ateliers de Munich, pourvu de quatre verniers et donnant 10". Les élévations du sol furent déterminées par des observations barométriques faites avec soin, souvent répétées et déduites par le moyen d'observations correspondants; elles furent calculées d'après la méthode d'Oltmanns et vérifiées par celles du Baron Zach. —V.P.

"Report on the co-operation of the Russian and German observers, in a system of simultaneous Magnetical Observations." By the Rev. H. Lloyd, F.R.S., in a letter addressed to Sir John F. W. Herschel, Bart., V.P.R.S. Communicated by Sir John Herschel.

"On Magnetical Observations in Germany, Norway, and Russia." By Major Sabine, R.A., V.P.R.S., in a letter to Baron von Humboldt, For. Mem. R.S., dated Oct. 24th, 1839.

These letters relate to communications which Professor Lloyd and Major Sabine have had, conformably to a resolution of the Council of the Royal Society, with the scientific authorities at Göttingen, Berlin, and St. Petersburg, respecting the organization of a simultaneous system of magnetical observations. It appears, from these letters, that the system proposed by the Royal Society is viewed with general interest and approbation; and nineteen stations are enumerated at which there is reason to expect that magnetical observatories, acting in concert, on that system, will be established.

Jan. 23.—A paper was read, entitled "On the structure of Normal and Adventitious Bone." By Alfred Smee, Esq., communicated by P. M. Roget, M.D. Sec. R.S.

On examining, by means of a microscope, very thin sections of bone, prepared in a peculiar manner, the author observed a number of small, irregularly-shaped, oblong corpuscles, arranged in circular layers round the canals of Havers, and also rows of similar bodies distributed around both the external and the internal margins of the bone. Each corpuscle is connected by numerous filaments, passing in all directions, with the Haversian canals and the margins of the bone, and also with the adjacent corpuscles. He finds that the canals of Havers are vascular tubes containing blood. The corpuscles themselves are hollow, and their cavities occasionally communicate with those of the canals; their length is equal to about two or three diameters of the globules of the blood. They exist in cartilaginous as well as osseous structures, and are found in every instance of adventitious bone, such as callus after fracture, morbid ossific growths either from bone or from other tissues; and the author has also ascertained their presence in the bony and cartilaginous structures of inferior animals, such as birds and fishes. Measurements relating to these corpuscles, by Mr. Bowerbank, are subjoined, from which it appears that their diameters vary from about the 10,000th to the 4000th, and their lengths from the 2300th to the 1400th part of an inch.

"An attempt to establish a new and general Notation, applicable to the doctrine of Life Contingencies." By Peter Hardy, Esq., F.R.S.

After premising a short account of the labours of preceding writers, with reference to a system of notation in the mathematical consideration of life contingencies, the author enters at length into an exposition of the system of symbols which he has himself devised, together with the applications which they admit of in a variety of cases.

Jan. 30.—A paper was read, entitled “Observations on Single Vision with two Eyes.” By T. Wharton Jones, Esq. Communicated by Richard Owen, Esq., F.R.S.

The author animadverts on the doctrine which Mr. Wheatstone, in his paper on the Physiology of Binocular vision, published in the *Philosophical Transactions* for 1838, p. 371*, has advanced, in opposition to the received theory of single vision being dependent on the images of objects falling on corresponding points of the two retinae. He maintains that, under these circumstances, the two impressions are not perceived by the mind at the same instant of time, but sometimes the one and sometimes the other. If one impression be much stronger than the other, the former predominates over, or even excludes the other; but still the appearance resulting from the predominating image is nevertheless in some manner influenced by that which is not perceived. He supposes that there are compartments of the two retinae, having certain limits, of which any one point or papilla of the one corresponds with any one point of the other, so that impressions on them are not perceived separately; and considers that this hypothesis, combined with the principle above stated, is required, in order to explain the phenomena in question.

Feb. 6.—A paper was read, entitled “Observations on the Blood-corpuscles of certain species of the Genus *Cervus*.” By George Gulliver, Esq., F.R.S., Assistant Surgeon to the Royal Regiment of Horse Guards†.

The author has found that the blood of the Muntjac‡, the Porcine§, and the Mexican Deer||, contains, together with corpuscles of the ordinary circular form, a still larger number of particles of less regular shape; some curved and gibbous in the middle, and acutely pointed at the ends, with a concave and convex margin, like a crescent; others approaching more nearly to segments of a circle; some shaped like a comma, being obtuse at one end and terminated by a pointed curve at the other; others having an acute projection of the convex part, so as to constitute a triangular, or even quadrangular outline; some having the figure of the head of a lance; while a few presented a double or sigmoid flexure, as if they had been twisted half round at the middle. Like the ordinary blood-discs, these peculiar corpuscles are deprived of their colouring matter by water; but with only a small quantity of water they quickly swell out, and assume an oval or circular figure, forming long bead-like strings by

* [Noticed in L. and E. Phil. Mag., vol. xiii., p. 461.]

† [Papers by Mr. Gulliver, on the blood-corpuscles of various animals, will be found in the present volume, p. 23, 105, and 195.—EDIT.]

‡ *Cervus Reevesii*.

§ *C. Porcinus*.

|| *C. Mexicanus*.

the approximation of their edges. In saline solutions they become rather smaller, but preserve their figure tolerably well.

In an appendix, the author gives an account of his observations of the blood-corpuscles of a new species of Deer inhabiting the mountains of Persia, of which a specimen has been lately received by the Zoological Society. Many of these corpuscles presented the singular forms above described.

A paper was also read, entitled "Meteorological Register kept at Port Arthur, Van Diemen's Land, during the year 1838." By Deputy-Assistant Commissary-General Lempriere, in south latitude $43^{\circ} 9' 6''$, and east longitude $147^{\circ} 51' 33''$. Communicated by Captain Beaufort, R.N., F.R.S.

The height of the instrument above the level of the sea till the 21st of August was 57 feet, 7 inches; and during the remainder of the year 3 feet.

A paper was also in part read, entitled "Experimental Researches in Electricity, 16th Series." By Michael Faraday, Esq., D.C.L., F.R.S., &c.

Feb. 13.—The reading of a paper, entitled "Experimental Researches in Electricity, 16th Series." By Michael Faraday, Esq., D.C.L., F.R.S., &c., was resumed and concluded. On the source of power in the Voltaic pile. An abstract of this paper has already appeared in our Number for April, p. 329 of the present volume.

Feb. 20.—A paper was read, entitled "On the Wet Summer of 1839." By Luke Howard, Esq. F.R.S. &c.

The observations of the author were made at Ackworth, in Yorkshire; and the following are his results with regard to the mean temperature and the depth of rain, in each month, during 1839.

Mean Temp.		Rain.	Mean Temp.		Rain.
		in inches.			in inches.
Jan.	37·04	1·13	July	59·30	5·13
Feb.	39·64	2·14	Aug.	58·09	2·94
March	39·08	3·21	Sept.	54·49	3·43
April	44·09	0·58	Oct.	48·39	3·40
May	49·94	0·38	Nov.	43·14	4·54
June	56·35	4·89	Dec.	37·29	1·85

Mean temperature of the year $47^{\circ} 24'$.

Total depth of rain in 1839, 33·62 inches.

He states that the climatic mean temperature of the place is about 47° , and the mean annual depth of rain about 26 inches. The excess of rain during the year 1839 was therefore very great.

The author describes the effect of the hurricane of the 7th of January, and follows the changes of the weather during the remainder of the year.

A paper was also in part read, entitled "On the chemical Action of the Rays of the Solar Spectrum on preparations of Silver and other substances, both metallic and non-metallic, and on some photographic processes." By Sir John F. W. Herschel, Bart. V.P.R.S. &c.

INDEX TO VOL. XVI.

- ACETIC** and chloroacetic acids, on, 152.
Achromatic eye-pieces, single, their superiority to the double eye-pieces of Huyghens, 595.
Acids:—acetic, 3, 152; chloroacetic, myronic, 152; polychromatic, 154; salicic, 214; sulphuric, 219.
Æther:—chloric, 6; sulphuretted, 8; chlorosulphuretted, 8; chlorinated acetic, 8; chlorinated formic, 8; chlorinated camphoric, 9; chlorinated benzoic, 9; chlorinated ænanthic, 9; chloropyromucic, 10.
Airy (G. B.) on the determination of the orbits of comets, 72.
Alais, description of some French cast irons prepared near, 571; analysis of irons from, 576; chemical constituents of, 576.
Albumen, action of, on metallic salts, 154.
Alten, in Finmarken, meteorological observations made at, 596.
Amidogene, combinations of salicylic with, 216.
Amplitude of De la Caille's arc of the meridian, verification of the, 594.
Analyses:—of salicine, 210; of oil of peppermint, 419; of minerals from the Skutteruder mine, 483; of iron from Champagne, 522; of iron from de l'Isere, 522; of iron from Siegen, 522; of irons from Alais, 576; of English cast steel, 585.
Arago (M.), experiment proposed by, as a test of the accuracy of the undulatory hypothesis of light, 157.
Arc of the meridian, Abbé de la Caille's, on the, 594.
Ardennes, fossils discovered in the clay slate tract of the, 389.
Argand oil lamp, on obtaining an increased quantity of light from a common, 194.
Arsenic, contained in the human body, on, 341.
Astronomical refractions, on, 89, 434.
Astronomical Society, Royal, 72, 144.
Baily (Francis), letter to, from Professor Schumacher, on the discovery of a comet by M. Galle, 151.
Bakerian Lecture: Mr. Ivory on the theory of the Astronomical Refractions, 89.
Barry (Dr. Martin), researches in embryology; 3rd series: a contribution to the physiology of cells, 526.
Beaumontite, a new mineral, 156.
Berkeley (Rev. M. J.) on a gall gathered in Cuba, by W. S. Macleay, Esq., on the leaf of a plant belonging to the order *Ochnaceæ*, 76.
Berzelius (M.) on the theory of substitutions of M. Dumas, 1; on oxichlorides with compound radicals, 7.
Bessel (M.), catalogue of twenty-seven stars of the Pleiades, 150.
Blood corpuscles of mammiferous animals, on the, 23, 105, 195; sizes of the, 26, 108, 197; of certain species of the genus *Cervus*, 599.
Blood, diabetic, and urine, 238.
Boetian contractions, new researches on the, 136; note on the, 221.
Boetius de Geometria on numerical contractions, 51.
Bone, normal and adventitious, on the structure of, 598.
Books:—Miller's Treatise on Crystallography, 65; Transactions of the Cambridge Philosophical Society, 67; Carpenter's Principles of General and Comparative Physiology, 70; Curtis's British Entomology, 144; Davies's Solutions of the Principal Questions of Dr. Hutton's Course of Mathematics, 524.
Boulder formation of Norfolk, on the, 345.
Bromide of Salicylic, 216.
Bromine, hydrocarburet of, 236.
Brooke (H. J.) on Haydenite and Couzeranite, 175; on crystallized native oxalate of lime, 449.
Buddle (John) on depressions produced in the surface of the ground by excavating beds of coal, 146.
Caldcleugh (Mr.) on earthquakes being felt at sea, 145.
Californias, geographical position of the principal points of the triangulation on the, and of the Mexican coasts of the Pa

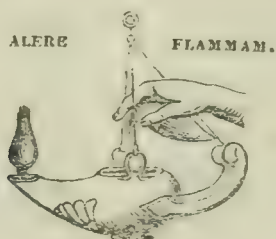
- cific, 597 ; heights of the principal points of that part of the Cordilleras, 597.
- Cambridge Philosophical Society, 16, 67, 220.
- Carbon, employment of, in voltaic combinations, 35 ; on the combinations of with silicon and iron, 44, 297, 426.
- Carburetted hydrogen of acetates, action of chlorine on the, 235.
- Cells, physiology of, a contribution to the, 526.
- Cervus, on the blood corpuscles of certain species of the genus, 599.
- Champagne, analyses of iron from, 522.
- Chemical rays, on the permeability of various bodies to the, 138.
- Chemical theory of galvanism, 485.
- Chemical types, on the law of substitutions and the theory of, 322, 442.
- Chemico-mechanical battery, on a new, 315.
- Chemistry, on the theory of substitutions in, 1.
- Chess board, on the moving the knight over every square of, alternately, 498.
- Chlorate of potash, manufacture of, 237.
- Chloric æther of M. Malaguti, 6.
- Chlorinated sulphuric æther of M. Malaguti, composition of, 7.
- Chloride of salicyle, 215.
- Chlorine, action of, on salicin, 219 ; action of, on the carburetted hydrogen of acetates, 235.
- Chloroacetic acid, composition of, 4.
- Chromate of lead, on the reduction of, 532.
- Circuits, galvanic, 485, 594.
- Clarke (Rev. W. B.) on ashes which fell on board the Roxburgh, off the Cape de Verd Islands, Feb. 1839, 144.
- Coal, beds of, on depressions produced in the surface of the ground by excavating, 146.
- Coahupe (C. T.) on certain modifications of the powers of heat and light when transmitted through glass, 467.
- Cobalt ores, Norwegian, on two, from the Skutteruder mine, 482.
- Coblentz, analysis of iron from, 522.
- Coexistence, on derivation of, 37.
- Comets, the orbits of, on the determination of, 72 ; on the discovery of a, 151.
- Conductors, lightning, on the effects produced by interposing different substances between the, 257 ; on the protection afforded by continuous, 408.
- Contact theory of galvanism, 485.
- Cooper (J. T.) on the employment of carbon in voltaic combinations, 35.
- Copernican theory in England, on the reception of the, 461.
- Copper, on the precipitation of, by voltaic electricity, 309.
- Cordilleras, heights of the principal points of the, 597.
- Cornwall and Devon, geology of, 59.
- Couzeranite and Haydenite, on, 175.
- Crag, Norwich, age of the, 372 ; shells found in the, 372 ; age of the freshwater deposit, 373 ; age and origin of the drift, 373.
- Cromer and Sherringham, cliffs between, 365.
- Cuba, gall gathered in, by W. S. Macleay, Esq. on the leaf of a plant belonging to the order *Ochnaceæ*, 76.
- Currents, induced, of the 3rd, 4th, and 5th orders, on the production and properties of, 260.
- Cyanide of mercury, on a new compound of ferrocyanide of potassium with, 128.
- Cyanil, 154.
- Daguerreotype, on portraits in, 535.
- De la Caille's arc of the meridian, 594.
- Devon and Cornwall, geology of, 59.
- Derivative of the 1st degree, rule for finding the prime, 135 ; for finding the prime derivative of any degree, 135.
- Diabetic blood and urine, 238.
- Discharge, voltaic disruptive, on some phenomena of the, 478.
- Draper (Prof.), experiments made in the south of Virginia, on the light of the sun, 81 ; on the electromotive power of heat, 451 ; on portraits in Daguerreotype, 535.
- Dumas (M.), M. Berzelius on the theory of substitutions of, 1 ; examination of the properties of chloroacetic acid, 3 ; on the theory of substitutions, 152 ; on acetic and chloroacetic acids, 152 ; on the law of substitutions, and the theory of chemical types, 322, 442 ; on mechanical types, 501.
- Eastern Norfolk, disturbed position of the strata of, 376 ; by ordinary subterranean movement, 377 ; by landslips and slides, 378 ; by pressure of drift ice, 379.
- Eifel limestone, fossils common to the, and the carboniferous limestone, 399.
- Electrical discharge, on the, 404.
- Electricity :—on the direction and mode of propagation of the electric force traversing interposed media, 185 ; on electricity and magnetism, 200, 254, 551 ; on the precipitation of copper by voltaic electricity, 309 ; Prof. Faraday's researches in, 329, 336 ; Mr. W. S. Harris, on the electrical discharge, 404.
- Electricity and magnetism, contributions to, 200, 254, 551.
- Electric condition of iron, on an anomalous, 142.

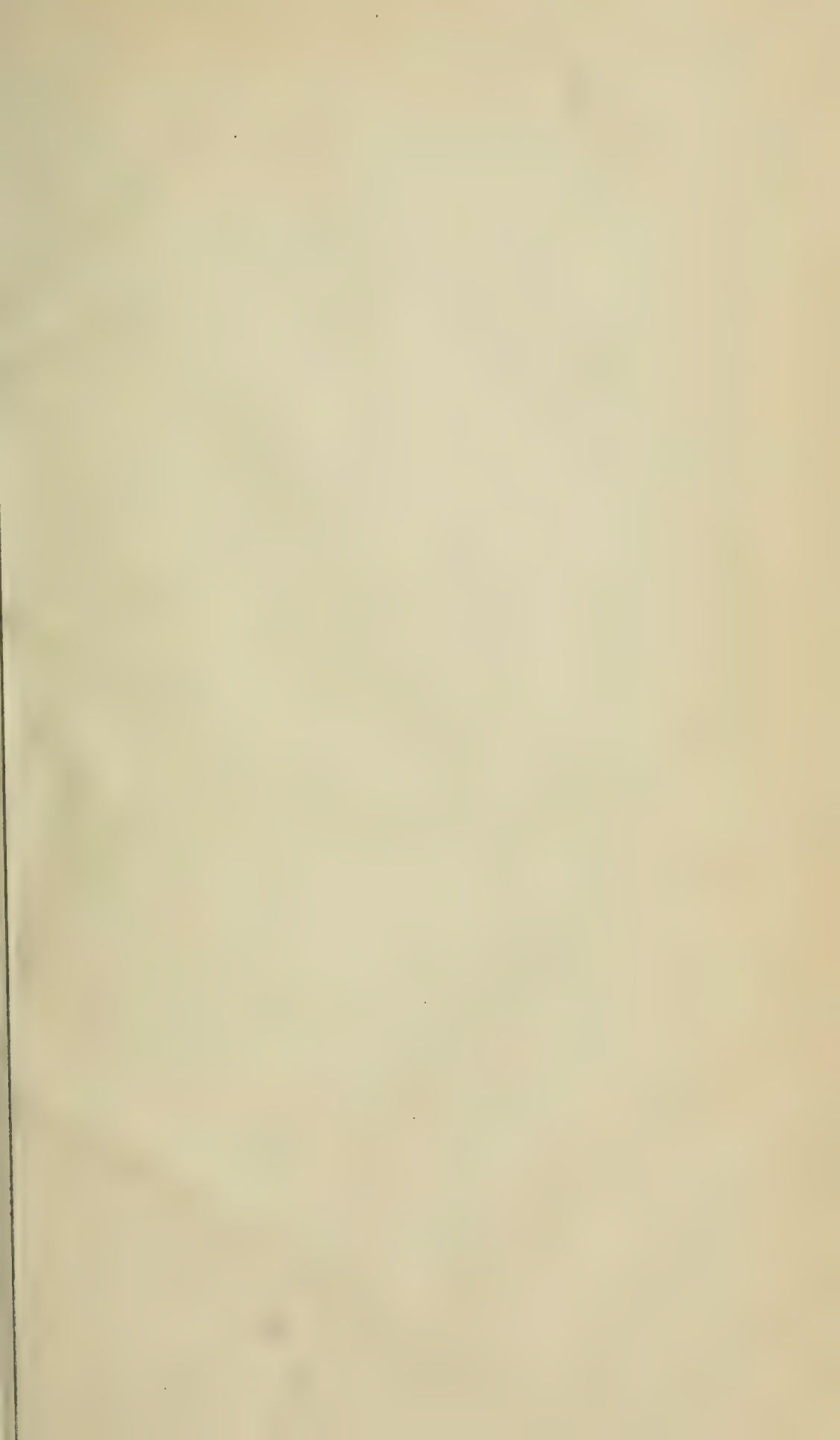
- Electric force traversing interposed media, on the direction and propagation of the, 185.
- Electrodynamic induction, 200, 551.
- Electromotive forces, differences of the on the contact of two metals with two fluids, 495.
- Electromotive power of heat, on the, 451.
- Electrotypes, on the production of, 530.
- Elementary bodies, on the galvanic properties of the, 422.
- Eliminating (x), a rule for absolutely, 132.
- Elimination and derivation by a process of mere inspection, 132.
- Embryology, Dr. Barry's researches in, 526.
- Encke's Comet, on the variation of the apparent diameter of, 150.
- England, Copernican theory in, on the reception of the, 461.
- Equations, combination of the given, 40; leading theorem, 40; inferences from the, 40; of co-existence, statement of the, 40.
- Eudialyte, on the form of, 477.
- Exosmose, mechanical, with reference to determining the magnitude of material particles, 10.
- Eyes, on single vision with two, 599.
- Faraday's (Prof.) experimental researches in electricity, 16th series, 329; 17th series, 336.
- Fermentation, on the supposed formation of inorganic elements during, 251.
- Ferriter's Cove, fossils discovered at, 167.
- Ferrocyanide of potassium, on a new compound of, with cyanide of mercury, 128.
- Films, coloured, produced by electro-chemical agency and by heat, 52.
- Fluids, on galvanic circuits composed of two, 485.
- Fluorine, Mr. Knox's researches on, 192; on a compound of, with selenium 194.
- Forbes (J. D.), letter to Richard Taylor, Esq. on two papers in the L. & E. Phil. Mag. for Jan. 1840, 102; on an apparent inversion of perspective in viewing objects with a telescope, 506.
- French cast irons, description of, prepared near Alais, 571.
- Freshwater deposits, on the, composing the mudcliffs of E. Norfolk, 345.
- Galle (M.) on the discovery of a comet by, 151.
- Galvanic circuits composed of two fluids, and of two metals, not in contact, 485.
- Galvanic series formed of zinc and inactive iron, 115.
- Galvanism, chemical and contact theories of, 485.
- Gas, pond, M. Persoz on, 339.
- Gassiot (J. P.) on obtaining the voltaic spark before the circuit is completed, 594; letter to Mr. Lubbock on Nobili's plate of colours, 596.
- Gay Lussac (M.), analyses of four sorts of iron by, 522.
- Geology:—on the geology of Devon and Cornwall, 59; on the older stratified rocks near Killarney and Dublin, 163; fossils discovered at Ferriter's Cove, 167; on the boulder formation and freshwater deposits of E. Norfolk, 345; *Paludina minuta*, 354; fish, 354; insects, 354; mammalia, 355; plants, 355; protuberances of chalk near Trimmingham, 355; freshwater strata of Runton, 362; shells at 363; crag at, 363; cliffs between Cromer and Sherringham, 365; crag near Weybourne, 370; shells obtained from the, 370; mud cliffs of E. Norfolk, 371; Norwich crag, 372; shells found in, 372; fossils in the clay state tract of the Ardennes, 389; fossils common to the Eifel limestone, 399; on two ores from the Skutteruder mine, 482.
- Geological Society, proceedings of the, 148.
- Germany, Norway and Russia, on magnetic observations in, 598.
- Glass, on modifications of the powers of heat and light, when transmitted through, 467.
- Gravid uterus, on the nerves of the, 590.
- Griffith (R.) on the order of succession of the older stratified rocks near Killarney and Dublin, 163.
- Grove, (W. R.) on voltaic reaction, 338; on some phenomena of the voltaic disruptive discharge, 478.
- Gulliver (G.) on the blood corpuscles or red disks of the mammiferous animals, 23, 105, 195; on the blood corpuscles of certain species of the genus *Cervus*, 599.
- Gutch (J. W. G.) on meteorological phenomena observed at Swansea, 87.
- Hail, on a remarkable fall of, 85.
- Halliwell (J. O.) on the treatise of Boetius de *Geometria* on numerical contractions, 51; on the Boetian contractions, with reference to the explanation given by M. Chasles, 136; note on the Boetian contractions, 221; on the history of the inductive sciences, 461.
- Hardy (Peter) on a new and general notation, applicable to the doctrine of life contingencies, 598.
- Harris (W. S.) on lightning conductors, and effects of, lightning on certain ships of the British Navy; being a further examination of Mr. Sturgeon's memoir on marine lightning conductors, 116, 404; on the electrical discharge, 404.

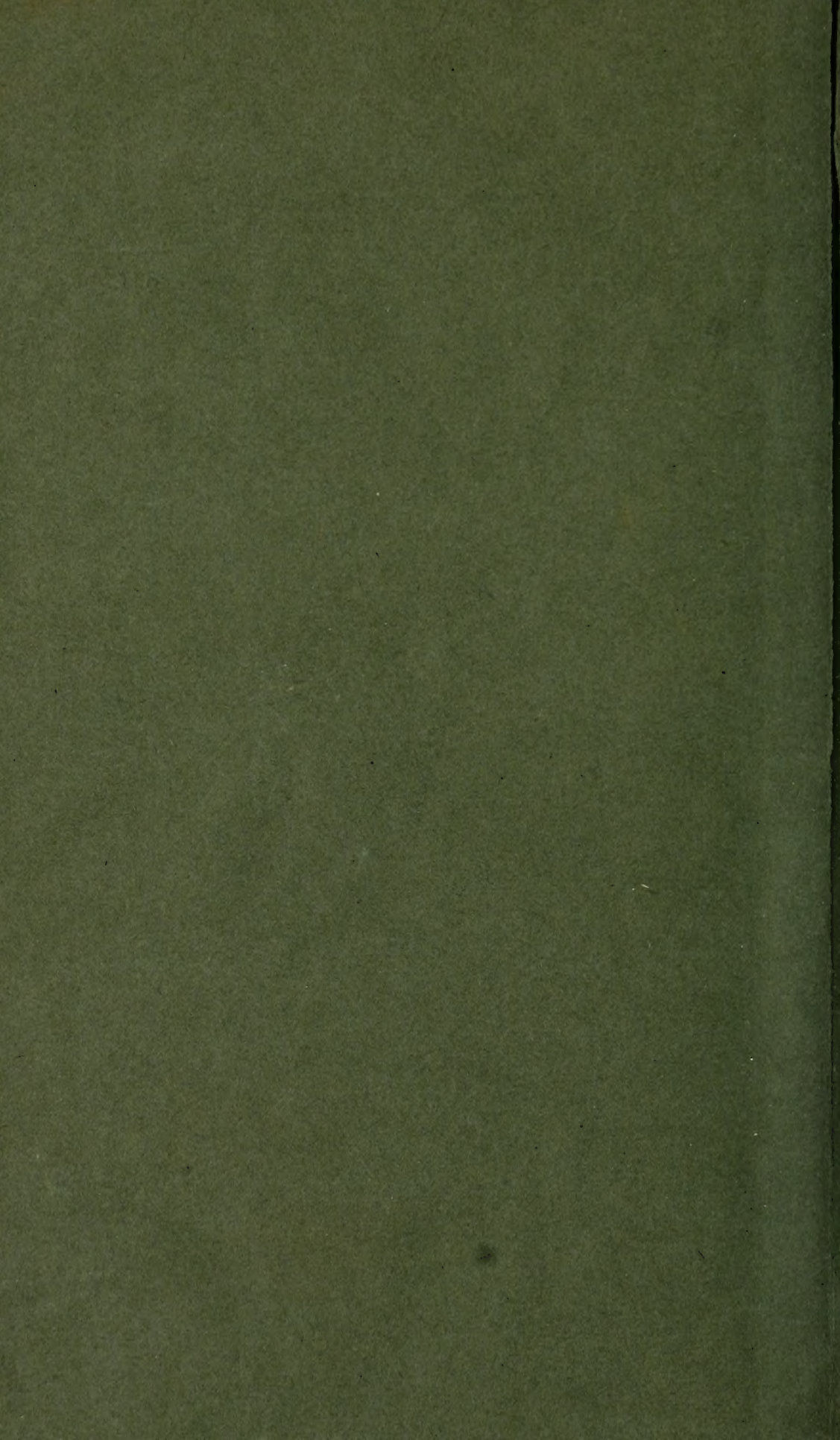
- Hawkins (Th.) on galvanic series formed of zinc and inactive iron, 115.
- Haydenite, 155; and Couzeranite, on, 175.
- Heat and light, on modifications of the powers of, when transmitted through glass, 467.
- Heat of vapours, and on astronomical refractions, 434; general expressions, 438.
- Heat, on the electromotive power of, 451.
- Henderson (T.) on the parallax of Sirius, 148.
- Henry (Prof.), contributions to electricity and magnetism: No. III. on electro-dynamic induction, 200, 254, 551; production of induced currents of the different orders from ordinary electricity, 551.
- Herschel (Sir J. W. F.), letter to, from M. Valz, on the variation of the apparent diameter of Encke's comet, 150; on obtaining an increased quantity of light from a common Argand oil lamp, 194; on the solar spectrum and photometry, 239; on the chemical action of the rays of the solar spectrum, 331.
- Howard (Luke) on the wet summer of 1839, 600.
- Human body, on arsenic contained naturally in the, 341.
- Hunt (R.) on the permeability of various bodies to the chemical rays, 138; results arrived at, 141; on light which has permeated coloured media, and on the chemical action of the solar spectrum, 267.
- Hyacinth, H.M. Ship, effect of lightning on, 405.
- Hydrocarburet of bromine, 236.
- Hydrochloric acid, ascendancy of water over, 537.
- Hydrogen of acetates, carburetted, action of chlorine on the, 235.
- India, on the temperature of the sea and air, during a voyage to, 176.
- Induction, on electro-dynamic, 200; of a current on itself, conditions which influence the, 203; of secondary currents at a distance, on the, 254.
- Inductive sciences, on the history of the, 461.
- Inorganic elements during fermentation, on the supposed formation of, 251.
- Iodide of potassium, on a pseudomorphous variety of, 221.
- Ireland, on the mineral structure of the south of, 276, 388.
- Iron, cast, steel, and malleable iron, on the different species of, 44, 297; zinc and inactive, on galvanic series formed of, 115; decomposition of the neutral sulphate of the peroxide of, by boiling its solution, 130; anomalous electric condition of, 142; precipitation of, by zinc, 235; natural products which originate from the action of the atmosphere on iron pyrites, 265; analysis of four sorts of, 522.
- Isere, de l', analysis of iron from, 522.
- Ivory (J.) on the theory of the astronomical refractions, 89.
- Jeffreys (Julius) on mechanical exosmose with reference to determining the magnitude of material particles, 10.
- Jones (T. Wharton) on single vision with two eyes, 599.
- Kane (Dr. R.) on a new compound of ferrocyanide of potassium, with cyanide of mercury, 123; on a pseudomorphous variety of the iodide of potassium, 223; on the compounds derived from the stearopten of oil of peppermint, 418.
- Killarney and Dublin, on the order of succession of the older stratified rocks near, 163.
- Knight's move at chess, on the problem of the, 306, 498.
- Knox (G. J.) on the direction and mode of propagation of the electric force traversing interposed media, 185; researches on fluorine, 192.
- Kreil (M.), letter from, to M. Kupffer, on magnetic observations at Milan, 241.
- Kupffer (M.), letter to, from M. Kreil, on magnetic observations at Milan, 241.
- La Caille's arc of the meridian, 594.
- Lead, chromate of, on the reduction of, 532.
- Lee (Dr.) on the nerves of the gravid uterus, 590.
- Lemprière (M.), meteorological register kept at Port Arthur, 600.
- Letter barometer, 79.
- Liège and Namur, species of coral found in the carboniferous limestone of, 390.
- Life contingencies, on a new and general notation, applicable to the doctrine of, 598.
- Light of the sun, experiments made in the south of Virginia on the, 81.
- Light:—undulatory hypothesis of, experiment proposed by M. Arago, as a test of the accuracy of the, 157, 181; absorption of, 181; on obtaining an increased quantity of, from a common Argand oil lamp, 194; on interferences as an *experimentum crucis* as to the nature of, 380.
- Lightning conductors, 116; effects of lightning on certain ships in H. M. Navy, 117, 404; on the Athol, 406; on the Snake, 407; on the spire of a church at Kingsbridge, 408; on the Buzzard brigantine, 408; on the Fox re-

- venue cutter, 408; on the *Mignomne*, 411; on H. M. S. *Beagle*, 408; on the *Hawk* cutter, 411.
- Lime, native oxalate of, on crystallized, 449.
- Linnæan Society, proceedings of the, 76; Anniversary meeting of the, 76.
- Lubbock (J. W.) on the heat of vapours and on astronomical refractions, 434, 510, 562; on the pressure of steam, 510; Mr. Gassiot to, on Nobili's plate of colours, 596.
- Lyell (C.) on the boulder formation, and freshwater deposits of Eastern Norfolk, 345.
- Maclear (T.), observations made for the verification of the amplitude of the Abbé de la Caille's Arc of the meridian, 594.
- Magnesia, native sulphate of, 236.
- Magnetic observations at Milan, on, 241; in Germany, Norway, and Russia, on, 598.
- Magnetism and electricity, contributions to, 200, 254, 551.
- Malaguti (M.) chlorinated sulphuric æther of, 7; sulphuretted æther of, 8; chloro-sulphuretted æther of, 8; chlorinated acetic æther of, 8; chlorinated formic æther of, 8; chlorinated camphoric æther of, 9; chlorinated Benzoic æther of, 9; chlorinated cænanthic æther, 9.
- Mammiferous animals, on the blood corpuscles or red disks of the, 23, 105, 195.
- Marchand (R. F.) on the reduction of chromate of lead, 532.
- Marine lightning conductors, examination of Mr. Sturgeon's memoirs on, 116.
- Martin (P. J.) on a remarkable fall of hail, and on the probable nature of such phenomena, 85.
- Media, on light which has permeated coloured, 267.
- Mercury, cyanide of, on a new compound of ferrocyanide of potassium with, 128.
- Metallic salicides, 213.
- Metallic salts, action of albumen on, 154.
- Metals, on the galvanic properties of, 315; on circuits of two, not in contact, 485.
- Meteorological observations, for November 1839, 79; for December, 1839, 159; for Jan. 1840, 239; for Feb. 343; for March, 447; for April, 535.
- Meteorological observations made at Alten, in Finmarken, 596.
- Meteorological phenomena observed at Swansea, on certain, 87.
- Meteorological Register kept at Port Arthur, 600.
- Meteorological Table, for November, 1839, 80; for December, 160; for Jan. 1840, 240; for Feb. 344; for March, 448; for April, 536.
- Miller (Prof.) on the form of Eudialyte, 477.
- Mineralogy:—haydenite, 155, 175; beaumontite, 156; couzeranite, 175; two minerals from the Skutteruder mine, 483.
- Mineral structure of the south of Ireland, on the, 276, 388.
- Mud cliffs of E. Norfolk, on the drift and associated freshwater deposits composing the, 345; age of the deposits composing the, 371.
- Mundesley, *Paludina minuta* from the freshwater beds at, 354; insects found at, 354; fish, 354; mammalia, 355; plants, 355.
- Mustard, oil of, essential, 153.
- Myronic acid, 153.
- Myronin, 153.
- Nerves of the gravid uterus, on the, 590.
- Nobili's plate of colours, on, 596.
- Norfolk, eastern, on the freshwater deposits of, 345.
- Norway, Russia and Germany, on magnetic observations in, 598.
- Norwegian cobalt ores, on two, from the Skutteruder mine, 482.
- Ochnaceæ*, on a gall gathered in Cuba, on the leaf of a plant belonging to the order, 76.
- Oil of mustard, 153; of peppermint, 418.
- Optics, physical, on photometry in connexion with, 16.
- Orfila (M.) on arsenic contained naturally in the human body, 341.
- Oxalate of lime, native, on crystallized, 449.
- Oxichlorides, combinations of, with other bodies, 6; with compound radicals, 7.
- Oxidized pyrites, on the products of, 265.
- Parallax of Sirius, on the, 148.
- Pelouze (M.), letter to, from M. Berzelius, on the theory of substitutions in chemistry of M. Dumas, 1.
- Peppermint, oil of, compounds derived from the stearopten of, 418; analysis of, by Mr. Walter, 419.
- Permutations, positive, 134; negative, 134; positive effectual, 134; negative effectual, 134.
- Peroxide of iron, decomposition of the neutral sulphate of the, by boiling its solution, 130.
- Persoz (M.) on pond gas, 339.
- Perspective, on an apparent inversion of, in viewing objects with a telescope, 506.
- Phænomena, meteorological, observed at Swansea, 87; on some, of the voltaic disruptive discharge, 478.
- Photographic papers, on, 267.

LONDON:
PRINTED BY RICHARD AND JOHN E. TAYLOR,
RED LION COURT, FLEET STREET.







QC

1

P4

ser.3

v.16

Physical &
Applied Sci.
Serials

The Philosophical magazine

PLEASE DO NOT REMOVE
CARDS OR SLIPS FROM THIS POCKET

UNIVERSITY OF TORONTO LIBRARY

